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MEN AND CULTURES

MEN AND CULTURES

Selected Papers of the Fifth International Congress of Anthropological and Ethnological Sciences

Philadelphia, September 1–9, 1956

Edited under the Chairmanship of

Anthony F. C. Wallace

PHILADELPHIA
UNIVERSITY OF PENNSYLVANIA PRESS

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PREFACE

The accompanying papers have been selected by the Publication Committee as representative of the papers presented at the Fifth International Congress of Anthropological and Ethnological Sciences in Philadelphia, September 1–9, 1956. Because of financial limitations, we have been able to publish less than half of the papers actually delivered at the Congress. Nevertheless, we have tried to make a selection which would be approximately representative of the several sciences and of the various countries who joined in the Congress. Many excellent papers have had to be left out; to the authors of these, the Publication Committee extends its sincere apologies for the inconvenience imposed on them by the exigencies of the situation. A list of the papers presented at the Congress is included in the publication. In order to provide as much space as possible for the scientific papers, we have not included the official acts of the Congress in this publication.

Philadelphia,
June 27, 1957

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INTRODUCTION

THE HIAWATHA WAMPUM BELT OF THE IROQUOIS LEAGUE FOR PEACE: A SYMBOL FOR THE INTERNATIONAL CONGRESS OF ANTHROPOLOGY

William N. Fenton

It has been the custom at international congresses in the science of man for the organizing committee to adopt some symbol or example of aboriginal fine art as the emblem of the Congress during that session. With the privilege of choosing the device goes the responsibility, some time during the program, of explaining to the Congress the meaning of the emblem, which appears on the cover of the programme and on the envelopes which were distributed at registration. Your Organizing Committee had no difficulty in agreeing that the most appropriate object for the emblem was the one suggested out of the aboriginal political symbolism of the League of the Iroquois: the Hiawatha Wampum Belt, a mnemonic device for remembering the founding of the original League for Peace in the Stone Age of North America. The object itself is among the most important and valuable specimens in the New York State Museum at Albany. The New York State Museum regrets greatly that because of its fragile condition it could not be moved to Philadelphia. The purpose then of this illustrated lecture is to explain a plan of union which the old men of Onondaga, New York, once proposed in vain as a model to the governors of the New York, Pennsylvania, Maryland, and Virginia colonies but which had a profound effect afterward upon Franklin and the founders of this republic. This symbol was particularly appropriate to this occasion because descendants of the founders of the Iroquois League, who still perpetuate its council and ritual on the Six Nations Reserve in Canada, attended this Congress. I feel thrice blessed then and am happy indeed to discharge the role of Honorary Keeper of the Wampum, an office which the Onondaga Nation, who were the original keepers of the wampums of the Confederacy, conferred upon a former Director of the New York State Museum in 1898.

I shall begin by describing the specimen itself, next I shall raise some questions about its age, third, illustrate its function in "forest diplomacy," then shall I "read the wampum," and, finally, reinterpret its symbolism for this Congress.

I

The Hiawatha Belt (N.Y.S.M. Cat. No. 37,309), is by no means the longest wampum belt in existence, now measuring but 21½ inches, having lost an inch and one-half since it was first described in 1878 (Beauchamp, 1901, p. 411); and it is 38 rows wide (10½ inches) or "deep," as the Indian Records note their width. In the latter dimension it is exceeded by only two others of 45 and 50 rows, which are also considerably longer, as is a fourth which may be the longest belt in existence (6 feet, 3½ inches). The Hiawatha Belt ranks fourth in area. It consists of 6,916 shell beads in the form of a mat, the warps of which are

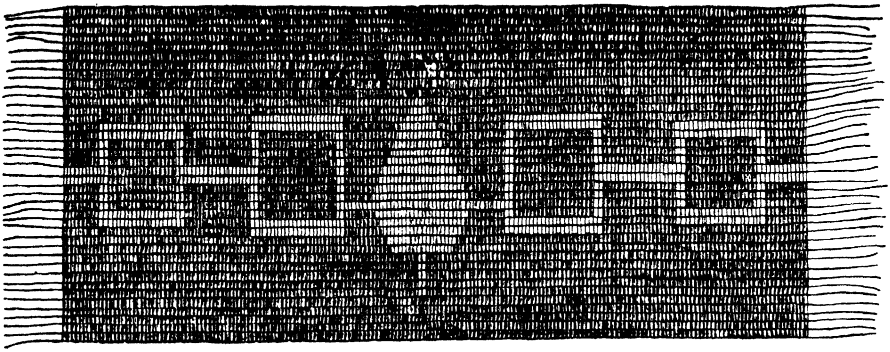

Fig. 1. The Hiawatha Belt

buckskin thongs, the outer ones braided, and the beads are strung as wefts on hemp thread. In the solid purple field is worked a design of white shell beads, illustrating a tree or heart, flanked by two sets of hollow squares which are connected to each other by a double row of white beads which extends to the extremities, and by a single row to the central figure. The design is self-contained and I do not think that the belt has lost many courses of beads at the ends because the fringe shows but few additional courses of wefts which end well within the fringe itself.

II

Old as the design may be, the beads appear to have been made with modern tools, as Beauchamp noted (1901, p. 411), from the shell of the Quahaug or hard clam (*Venus mercenaria*) which once abounded along the Atlantic Coast from Cape Cod south and was the principal source of purple wampum, although a variety of shells furnished the white. It seems probable that the Dutch settlers at Manhattan seized on a trait to which a trade value had already attached because a number of varieties of shell beads appear in graves going back to quite early prehistoric times, and one of these developed into the wampum of colonial times which first appears archeologically during the seventeenth century. For the history of the wampum industry is well established to have begun about 1630 on eastern Long Island when coastal Algonquian Indians commenced to use steel drills for perforating the cylindrical beads from opposite ends, resulting in a broken channel, and wampum factories persisted to the middle of the last century in New Jersey. Historical accounts abound telling us how the fur trade created a demand for the use of wampum as money by the coastal Indians, the Dutch and English colonists, and by the inland Iroquois of New York, who developed elaborate systems of strings and belts as mnemonic devices for treaty making, messages, mourning, and for religious purposes. Purple wampum carried twice the valuation of white wampum out of original scarcity, but as production increased and quality improved during the mid-seventeenth century Gresham's law set in to devalue it. The supply of wampum declined as it was driven off the market at the end of the century and then increased enormously at the mid-eighteenth century when the treaty-making activities of the English colonies with the Iroquois tribes consumed thousands of beads in making a single belt of which twelve to twenty might be passed at a single negotiation. Thus the Iroquois acquired all of their wampum through intermediaries from coastal sources.

Beauchamp in 1901 first questioned the age of some of the Iroquois wampum belts in the New York collection and they have twice been described since by Clarke (1929, 1931); but until quite recently the belts had not been x-rayed to determine the relative age of the beads from which they were woven by the marked improvement in drilling technique during the later period of the wampum industry when the beads were perforated in a single direction, leaving a straight channel. Aside from this general criterion established by Orchard in 1929 for early and late wampum, statistical sorts of wampum from dated graves have not been plotted against samples of similar sorts taken from belts of known and unknown age. As a beginning, x-ray photographs were made through the courtesy of the New York State Department of Health of the Hiawatha belt and of two larger mat belts in our collection, which are likewise undated. These pictures have proved so interesting to scholars and laymen alike that for comparison we have since had x-rayed the Washington Covenant Belt, which has a known date of 1775, and is the longest of the Iroquois belts (6 feet 3½ inches) and contains 10,000 beads.

Of the three belts so far x-rayed—Hiawatha, the Evergrowing Tree, and the Thadodaho or Chain Belt—all of which are referred traditionally to the founding of the Iroquois League—the latter two appear to have been made of uniformly matched but irregularly drilled beads of an early historic period, while the Hiawatha Belt appears to contain wampum of several sorts; it is irregular in size and the length of the beads varies, and the drilling shows two extremes of technical skill, some having been drilled in two directions and others near the margins of the belt clear through. Two of the belts are the work of a single maker who had access to a source of uniform wampum in large quantity. The Hiawatha Belt was redone at some time using beads from two different sources, a fact which suggests that the Indian council unstrung two older belts of differing age and handed the beads to the maker of the present belt, at some later time. The Hiawatha Belt contains one bead that is opaque in the x-ray. Although the symbols they carry may be much older, I think the three belts can be dated from the mid-eighteenth century when purple wampum was abundant, when between 1755 and 1774 forest diplomacy was a great drama played between the Six Nations and Her Majesty's Indian Superintendent, Sir William Johnson, the Penns and the other colonial governors, when Iroquois belts are known to be "mostly black Wampum" . . . and . . . "they describe Castles [their towns] sometimes upon them as square figures of White Wampum" (Johnson to Lee, 1771, in O'Callaghan, 1851, 4:437).

III

The literature of the council fire which records the transactions of forest diplomacy from the mid-seventeenth century and throughout the eighteenth century redounds in metaphors—the path, the clearing by the forest, the darkness of grief, death and war, the fire and the light of the sun, the tree of peace with its roots extending in the cardinal directions to all nations, the column of smoke rising to heaven summoning men everywhere to join in the great peace, the chain of friendship binding men and nations in the great law of mutualities, and the three words of pity—to wipe away the tears, to open one's ears, and to clear one's throat to speak—which were used to condole the mourning kinsmen of other nations and to restore life and society. All of these figures were employed by Indian orators and were learned by White officials alike, who propped up their memories at first with painted sticks and afterward with belts of wampum which became broader and longer and more elaborate in design as the supply of available wampum increased and the ceremony of treaty-making elaborated.

There is no need now to document my words with specific instances and so I shall refrain from quoting the Indian Records, particularly since I do not find an exact description of the belt in question although there are comparable examples in plenty to fix its membership in the genre of mid-eighteenth century treaty belts. The Iroquois themselves have been more careful custodians of this tradition.

IV

The Constitution of the Five Nations, as it was written down by Seth Newhouse, a Mohawk on the Six Nations Reserve in Canada about 1880, and afterward published by the late Dr. Arthur C. Parker, himself a Seneca, an ethnologist and a museologist, refers under Section 60 of the Great Binding Law to the reading of the Hiawatha Belt (Parker, 1916, p. 47):

> "The first of the squares on the left represents the Mohawk nation and its territory; the second square on the left and the one near the heart, represents the Oneida nation and its territory; the white heart in the middle represents the Onondaga nation and its territory; and it also means that the heart of the Five Nations is single in its loyalty to the Great Peace, that the Great Peace is lodged in the heart (meaning the Onondaga Confederate Lords), and that the Council Fire is to burn there for the Five Nations, and further, it means that the authority is given to advance the cause of peace whereby hostile nations out of the Confederacy shall cease warfare; the white square to the right of the heart represents the Cayuga nation and its territory and the fourth and last square represents the Seneca nation and its territory.
>
> " White shall here symbolize that no evil or jealous thoughts shall creep into the minds of the Lords while in council under the Great Peace. White the emblem of peace, love, charity and equity surrounds and guards the Five Nations." (Parker, 1916, p. 47–48.)
>
> "In reversing the belt, the figure of the 'heart' in the center assumes the appearance of a tree and at the same time brings the geographical position of the Five Nations in the correct order on the belt. A figure of a tree might well represent the Onondaga nation as the Onondagas were designated to keep the Council Fire and it was under the Great Tree of Light that the nations met in council." (Clarke, 1931, p. 88–89.)

The Belt can be read in either direction. In sum, it tells us how the Mohawks, the Oneidas, the Onondagas, the Cayugas, and the Senecas were leagued in a confederacy for mutual protection and to advance the great peace.

Surely it means more than this to some of our Iroquois friends who are with us—who with the addition of the Tuscaroras became the Six Nations—whose record of loyalty to the British and American interests on this continent is truly remarkable. And their contributions to our science since Lewis H. Morgan and Horatio Hale worked among them a century ago have been in training many of us in our craft. I hope that Chief Alex General who indeed bears the distinguished Cayuga title DESKAHEH and was the steadfast colleague and friend of the late beloved Professor Frank G. Speck of this University will tell the Congress something about the glory that is the League of the Iroquois when I have spoken.

V

But let me close my remarks to this Congress which is about to form a union by saying they need not look beyond the Hiawatha Belt for a symbol to express the kind of international amity and good will that has marked these meetings. Just as Canasatego, the speaker for the Onondaga Council, reminded the governors of the neighboring colonies but a few miles west of here at Lancaster in 1744, how the Five Nations had united as one to face the world and that where they were formerly weak now they were collectively strong, how he cautioned them . . . "whatever befalls you, never fall out with one another." So I say that we take this symbol and grasp it firmly to our bosoms and impute the continents of the world—Europe, Asia, Africa, the New World—to each of the four squares that flank the tree of peace which stands for the central bureau of our union, or the heart of this Congress, where I presume the smoke of our council fire will next rise at Paris at the Place Trocadero and touch the sky for all the world to see.

New York State Museum,
Albany, New York.

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SECTION I

CURRENT STATUS OF ANTHROPOLOGICAL AND ETHNOLOGICAL STUDIES

CURRENT TRENDS IN THE DEVELOPMENT OF AMERICAN ETHNOLOGY

Ralph L. Beals

A survey of recent trends in American ethnology must be dominated by a sense not only of change but of the immanence of further change.[1] Excellent recent papers by Kroeber, Eggan and others give excellent summaries of longer-range trends and encourage me to emphasize the recent period and to venture some look at the possible future.[2]

A striking feature of the broad field of American anthropology is its rapid growth and professionalization. Most marked in the United States, this trend is apparent in Canada, Mexico, Colombia, Peru, and Brazil, once was true of Argentina, and is beginning in Venezuela and other countries. Unless otherwise indicated, however, my remarks will apply only to the United States.

Increase in the number of anthropologists in the United States has been accompanied by expanding employment opportunities and the opening of many new fields of investigation. In the academic field, not only has the number of institutions granting the Ph.D. degree grown in half a century from four to twenty-two, but the majority of colleges and universities today offer some instruction in the subject. Increasingly anthropology, and especially ethnology, is regarded as a valuable part of education not only for all fields involving human relations and human behavior but for a revitalized humanistic education. In several states ethnological data and concepts play an increasing role in elementary and secondary-school curricula.

One measure of growth and professionalization is the number of Ph.D. degrees in anthropology granted by United States universities. The first such degree was awarded in 1894. By 1924, forty-four dissertations had been accepted, rising by 1944 to 294 and jumping by 1954 to 591 (Table I). Thus the number of doctoral dissertations accepted since 1944 exceeded the number in the previous fifty years. Moreover, the number of doctorates granted since 1944 equals about three-fifths of the number of regular Fellows of the American Anthropological Association in 1955. Before 1945, about 62 per cent of all anthropology dissertations were in ethnology; since 1944, about 72 per cent.

TABLE I

PH.D. DISSERTATIONS IN ANTHROPOLOGY ACCEPTED BY UNITED STATES UNIVERSITIES

	1894–1944	1945–1954	*Totals*
All fields of anthropology	294	297	591
Ethnology and related subjects	184	211	395
Per cent ethnology is of total	62.2	72.0	66.8

Compiled from *Yearbook of Anthropology, 1955.*

Numerically, then, ethnology in the United States is dominated by young people who have secured their doctorates since 1944. It seems certain that these younger people will give important new directions to ethnology in the near future.

In an effort to predict these future trends as well as to gain some idea of current interests, I have made an analysis of the titles of doctoral dissertations accepted by United States universities in the two decades of 1925 to 1934 and 1945 to 1954. I shall use the results of this analysis as the major framework of my discussion of recent and future trends, supplementing it by personal judgments. I have adopted this course because any attempt to cover recent publications or the work of particular men and groups in twenty minutes would leave so many and possibly invidious omissions.

The two decades chosen are a generation apart. The decade 1925–1934 also was the first with a substantial number of doctoral dissertations, 75, of which 43 were in ethnology. Unquestionably the subjects of Ph.D. dissertations to some extent represent the interests of the professors in charge rather than those of the students. Yet thirty-four of the writers of ethnological dissertations in the early decade continued professionally active and are known to me. Of these I estimate that the later work of twenty-four partially reflects interests identifiable in the titles of their dissertations. If this holds true of the later period, then we may expect that the post-doctoral research of about 70 per cent of those who continue professionally active will bear some relation to their dissertations. At the same time, my analysis shows that the dissertations do not reflect some recent trends that I consider important.

Dissertations in the later period show a wider range of geographic interests than in the earlier. The number dealing with peoples or problems within Anglo-America declined from about 58 per cent to 45 per cent (Table II).

TABLE II

Area Subjects or Interests in Ph.D. Dissertations in Anthropology accepted in United States Universities

	1925–1934		1945–1954	
	No.	%	No.	%
Anglo-America	25	58.1	95	45.0
Latin America	3	6.9	43	20.3
Europe	2	4.6	4	1.8
Africa	3	6.9	19	9.0
Asia (including Japan and Philippine Islands)	2	4.6	24	11.3
Oceania (including Australia)	4	9.3	12	5.6
Non-areal, pan-areal or no indication	4	9.3	14	6.6
	43	99.7	211	99.6

Note: Inspection suggests that later dissertations more frequently are based upon or related to field work.

Source: *Yearbook of Anthropology, 1955.*

Dissertations focused on Latin America rose from about 7 to 20 per cent of the total, on Africa from 7 to 9 per cent, and for Asia from 4½ to 11 per cent, although percentages dealing with Europe and Oceania declined. Significantly the proportion of dissertations based upon or related to field work increased substantially. If more field and travel funds become available, I predict a further increase in research outside North America.

Classification of subject matter, problems and methods based solely upon titles, permits the use of only broad categories and rather subjective criteria of classification (Table III). The categories I have employed overlap somewhat and any given dissertation may fall in more than one category.

TABLE III

APPROACHES AND FIELDS OF INTEREST

PhD. Dissertations in Ethnology accepted by United States Universities

	1915–1934		1945–1954	
	No.	%	No.	%
I. Historical, descriptive, distributional				
A. Case studies	12	26.7	17	7.9
B. Trait distributions, area studies, diffusion	10	22.2	21	9.8
C. Ethno-historical	0	0	16	7.5
II. Social structure, social interaction, essentially non-historical				
A. Case studies	8	17.8	56	26.2
B. Comparative	10	22.2	16	7.5
III. Social and cultural change				
A. Problem interest	3	6.7	49	22.9
B. Applied interest	0	0	7	3.3
IV. Individual and culture				
A. Role of individual, socialization, specific psychological processes	2	4.5	10	4.7
B. Culture and personality	0	0	9	4.2
V. Ethos, themes or values	0	0	13	6.1
Total classifiable dissertations	45		214	

Note: Many dissertations are classifiable in more than one category; hence neither numbers or percents may be added. See text for further description of categories.

Source: *Yearbook of Anthropology, 1955.*

One major distinction is between dissertations concerned primarily with historical problems and those with a non-historical focus. Virtually all of the latter may roughly be classified as structural and/or functional in their approach. The objectives of the latter may emphasize dynamic or processual factors or be primarily analytical and generalizing in purpose, although these characteristics are not mutually exclusive.

More than half of the 43 ethnological dissertations submitted between 1925 and 1934 which could be classified with confidence were primarily historical in orientation. This was true of only a quarter of the dissertations submitted in the 1945–1954 period. Moreover, significant differences in the kinds of historical interest appear when the two periods are compared. In the early period over 27 per cent of the dissertations centered about the ethnology of a single tribe, usually tracing its historical development or its relationships with other groups in the same culture area. In the later period less than 10 per cent were in this category. The percentage of dissertations concerned with trait distributions, characterizations of culture areas, or problems of diffusion and historical reconstruction dropped from 23 per cent to 10 per cent. Finally, in the later period 16 or nearly 8 per cent of dissertations were ethno-historical, a classification by my criteria completely lacking in the earlier period. These studies are documented analyses either of the history of a single tribe or, more frequently of problems of culture change or of structural-functional analysis. Hence most ethno-historical dissertations reappear in other classifications.

Dissertations with non-historical orientations may be classified in several ways. One group I have characterized as being concerned with social structure or function, including studies of specific formal institutions, analyses of tribes or communities, and such aspects of social interaction as role, status and stratification. In this group, case studies analyzing a single tribe or community rose from 18 to 27 per cent but comparative studies dropped from 23 per cent to less than 8 per cent.

If diffusion problems be excluded, less than 8 per cent of the earlier dissertations show interest in social or cultural change. In contrast, in the later period more than one quarter of the total are concerned with the dynamics or processes of change, acculturation situations and the like. Of these, a small number are focused on applied problems, a category lacking in the earlier period.

Excluding culture and personality topics, in both periods slightly less than 5 per cent of the dissertations are concerned with the role of the individual in culture, with socialization, or with specific psychological processes such as the channeling of aggression in a given society. Culture and personality topics, lacking in the earlier period, constitute less than 5 per cent in the later decade.

Concern with such things as themes, ethos, or values is not evident in the earlier period but appears in over 6 per cent of the dissertations in the later period.

In the later period the data for some 12 per cent of the dissertations are drawn from studies of peasant or rural communities within a larger society, while data for another 12 per cent are derived from the study of an ethnic minority. This was not true of any dissertation in the earlier period.

Comparing the analysis of the dissertations in the recent period with my own evaluation of recent trends, I find important omissions or differences in emphasis. In both periods covered there is no dissertation dealing with cultural evolution. In view of the number of recent publications and symposia discussing neo-evolutionary or multi-evolutionary problems, one must assume that these are too difficult for dissertation topics. Certainly a revival of interest in cultural evolution is a small but significant recent trend in United States ethnology.

The number of dissertations in ethno-history is consonant with the recent emergence of this field and the founding of a specialized journal last year. One stimulus to this field has been the recent participation of many anthropologists in Indian Lands Claims legal cases as consultants. This has led to new awareness of the wealth of unexploited documentary data about the American Indian.

Certainly the previous discussion does not do justice to the amount and nature of historical interest still current among United States ethnologists. Contrasting the situation today with that in the late twenties and early thirties, one might say that in the United States the historical lamb today lives peaceably with the functionalist tiger. As Eggan argues in the article already cited, there is no necessary incompatibility. Historical studies which emphasize the uniqueness of events and combinations of events there are and will continue to be. But many historical studies today focus not upon events but upon the identification of underlying processes which may be adequately analyzed and may prove to be repetitive. At the same time many followers of the "scientific" approach today consider it necessary to use historical materials to identify unique or unpredictable variables which must be accounted for or allowed for in the development of research models and designs, and which often must form one type of qualification in the formulation of generalizations. As a result there is a great deal more historical work being done than is measured by the avowedly historical studies.

The recent dissertations certainly are not representative of the large amount of comparative research and publication in the last five years. Oscar Lewis's article in the 1955 Yearbook of Anthropology clearly shows important trends toward the development of new varieties of comparative studies and a great increase in the rigorousness of methodology. As Eggan argues, the method of controlled comparison offers perhaps the best hope for the development of the scientific aspects of ethnology. A distinguished psychologist once said to me that, "Anthropology is primarily a comparative study or it is nothing." Recent trends suggest that most ethnologists in the United States would agree with this as one of our major objectives. The small number of comparative studies in recent dissertations probably results from the current heavy emphasis upon field studies in the training of graduate students. It may also be that good comparative studies require more experience and sophistication than most graduate students possess.

Clearly concern with culture change is a major characteristic of contemporary United States ethnology. It appears both in historical studies and in the preoccupation of many of the structural-functional ethnologists with process. The few dissertations addressed to an applied problem all fall in this category. This clearly is not representative of the growth of applied ethnology in recent years. Possibly the small number of doctoral dissertations reflects a belief that applied problems lack sufficient theoretical content, despite the opportunity they may offer for the testing of hypotheses. On the other hand, it may reflect the opinion of some practitioners in the applied field that the best preparation still is training in general ethnology. Despite the great growth of the applied field, many feel that there has been little progress in affecting policy formation and the establishment of proper relations with planners and administrators. Too much applied anthropology, in the opinion of many, is limited to "trouble shooting" and the carrying out of policies conceived without adequate ethnological advice. Nevertheless, the number of ethnologists active today in such fields as medicine, public health, education, industrial relations and economic development is impressive.

Interests in the relation of the individual and culture, and especially the field of culture and personality, seem under-represented in the dissertation topics. Even more surprising is a complete absence of studies in the field of national character. As Kroeber has pointed out, however, these two areas still lack either generally accepted and well-systematized theory or clear methodology. Moreover, they often require either collaborative effort or more training in psychology than most students have time to acquire.

Apart from culture and personality studies, a psychiatrist well-read in ethnological literature recently remarked to me that he discerned a considerable and long continuing interest in United States ethnology in the problems of the development of the individual, the acquisition of culture, and interactions between the culture and the individual. Possibly many life histories express this kind of interest. My psychiatrist friend felt that these interests constituted an unnamed field in ethnology. Here, without the anthropologist becoming a psychologist, much of importance to both disciplines could be accomplished.

In any case it is clear that, whether one accept or reject much of the current work, research along the frontiers between ethnology and psychology has been and continues to be an area of exciting novelty and high potential for new development. The period of challenging if highly speculative theorizing seems about over. Increasingly workers are devoting themselves to the more prosaic task of developing adequate methodologies and hammering out bodies of low and middle theory which, if they still remain somewhat isolated, are scientifically researchable. An example is the recent attempt by psychologists, sociologists and ethnologists to formalize cross-cultural studies of socialization.

An important new trend revealed by our analysis is the interest in themes, patterns, ethos and values. Originating, insofar as it has a single source, in Ruth Benedict's *Patterns of Culture*, this group of interests reflects recognition of the importance of implicit or explicit principles upon the actions of and choices made by human beings in a given cultural framework. In addition, some studies reject older assumptions of complete cultural relativism and suggest that in at least some aspects of culture there may be a hierarchy of values, whether measured by postulated ideal systems or, more empirically, with respect to efficiency in the satisfaction of basic human needs.

Certain other trends not well revealed by dissertation titles deserve comment. One important trend is the series of stimulating attempts to extend typological analysis. The greatest activity seems to be among United States anthropologists concerned with Latin America, perhaps properly so, for much of the stimulus derives from Robert Redfield's pioneering work of a quarter of a century ago. Although I find the approach personally congenial, it is not yet clear whether typological analysis will develop new research tools and theories, or will remain a system of descriptive categories primarily of heuristic value.

The development of typological analysis is related to studies of the small community within a larger society. Here a significant recent trend is greater attention to the interrelations between the community and the larger society of which it is a part. Here the interests of sociologists and ethnologists come especially close together. The same is true of studies of urbanism. While not numerous, the several recent studies by ethnologists of urbanism in non-western societies probably foretell a future significant development. The same may be the case with the several recent studies of larger non-western societies such as China and India which do not confine themselves to the small community or to the analysis of some single institution.

In recent years a few studies have appeared dealing with demography and ecology at sophisticated levels. Although this marks something new in the ethnological literature of the United States, the numbers still are too few to constitute a trend of significance.

Comments on American ethnology outside the United States necessarily are limited by the very much smaller number of ethnologists to be found in other countries of the hemisphere. Even where professional training programs exist, they rarely reach the Ph.D. level. The *Yearbook of Anthropology* lists only one Ph.D. in ethnology for all Latin America and only three are listed for Canada if *essentially* literary folklore dissertations be excluded. But both in Canada and Latin America there has been a substantial increase in persons trained at somewhat lower levels or with various levels of training in the universities of Europe, England and the United States. Furthermore it should be noted that most countries offer few university posts in ethnology, and in Latin America these frequently do not permit full-time devotion either to teaching or research. Most full-time jobs are in the applied field and this in large measure determines the type of ethnological work which is done.

Two major types of ethnological research are apparent outside the United States. An older tradition is primarily oriented toward historical problems or a folkloristic approach. Influence either of Kulturekreise concepts or the older tradition of North American historical studies is evident. A relatively new and growing group is primarily oriented toward applied problems and is influenced mainly by recent trends in British and North American ethnology. The predominantly applied focus, coupled with the variety of local concerns, ensures considerable individuality in Latin American ethnologists but it tends to limit theoretical contributions.

To recapitulate:

1. Studies in the older historical tradition, including diffusion and historical reconstructions, are declining in relative importance. The field of ethnohistory has emerged, and both it and other historical studies increasingly are utilizing problems and concepts from structural-functional approaches. At the same time, many structural-functional studies are utilizing historical data and methods.
2. A significant revival of interest in evolutionary studies is occurring.
3. Analytical studies dealing either with structure, function, or the dynamics of change dominate current research. In very large degree these represent directly or indirectly the "social science" approach, the search for repetitive relationships or processes.
4. Studies concerned with the relationships between culture and the individual, although less numerous, are producing some of the most challenging as well as the most challenged ideas in the field. This is especially true of the strongly psychologically or psychoanalytically oriented studies in culture and personality.
5. Concern with the more intangible aspects of culture such as ethos, themes, patterns or value systems is a small but growing part of recent research.
6. There is renewed interest in comparative studies, accompanied by the emergence of new types and methods of study.
7. Applied ethnology is a field of growing importance.
8. Research interests increasingly are focused outside the North American continent.

A few final comments might be in order. The trends I have outlined may seem contradictory or diffuse and not to lead to any well-defined characterization of American ethnology. I admit this to be the case. The structure of American universities, particularly in the United States, has never led to well-defined schools in ethnology or other disciplines. The so-called Boasian historical school was always in large measure a figment, and its members often disagreed vehemently among themselves and with Boas. Only as viewed from such a new approach as that of the British functionalists was there an appearance of uniformity.

Moreover, the large number of universities, widely scattered in space, and both publicly and privately supported, have contributed to independence of thought. The development of applied anthropology has been accompanied with deep concern and attempts to set up safeguards against encroachments upon the intellectual integrity of the researcher. In intellectual as well as political life, Americans of all nations seek harmony in diversity and tend to reject claims for a single road either to salvation or truth. With the growing number of ethnologists and their entry into increasingly diverse fields, I venture to assert that, as in the past, American ethnology will not be dominated by one or a few competing schools, adhere to any official dogma, or be chained to any *a priori* ideological system.

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Notes

1. Use of the term "ethnology" in this paper follows that of this Congress, broadly interpreted, but employment of the term "anthropology" follows United States usage to include all the various fields of interest to this Congress.

2. Kroeber, A. L., "History of Anthropological Thought." *Yearbook of Anthropology*, 1955, Wenner-Gren Foundation for Anthropological Research, New York, 1955. Eggan, Fred., "Social Anthropology and the Method of Controlled Comparison." *American Anthropologist*, 56: 743–763, 1954. Other papers in the *Yearbook* were helpful, especially Oscar Lewis: "Comparisons in Cultural Anthropology."

RECENT DEVELOPMENTS IN THE STUDY OF THE PREHISTORY OF WESTERN ASIA

Robert J. Braidwood

Some particular parts of the world—because they were the foci of origin and early development of successfully persisting cultural traditions—attract a sort of emotional supercharge of intellectual concern. The Near East, an area doubtless correctly accepted as the fountainhead of the western cultural tradition, has long been blighted with this supercharge of concern. Without wandering far into the history of ideas, one easily recalls Montelius' *Ex Oriente Lux* theory and Reinach's counter-theory of *Le Mirage Orientale* as a suggestion of how the notion has affected Near Eastern archaeology. The Americanists, who deal with a set of prehistoric peoples with cultural traditions essentially divorced from their own, are most happily little blighted with the full force of this emotional supercharge.

Prehistoric archaeology in western Asia (I do not propose to deal with the course of Egyptian prehistory here) began in a milieu of two all-pervading assumptions:

1. Somewhere in western Asia lay the Garden of Eden, and
2. The classic six-part French scheme of prehistory (Chellean, Acheulean, Mousterian, Aurignacian, Solutrean, Magdalenian) was an affair of universal world-wide validity.

Until the 1930's, when a few professionally trained prehistorians began to arrive in the area, the course of west Asiatic prehistory was one of an attempt to shoe-horn the available materials into these assumptions, or to throw the baby out with the bath water.

By the beginning of World War II, Professor Dorothy Garrod's excellent excavation and publication of three caves on Mt. Carmel in Palestine was our almost single source of evidence for the range of Pleistocene prehistory in western Asia. It was further suspected that significant additions to knowledge would come from the work of Dougherty and Ewing at Ksar Akil cave in Lebanon, of Rust at the Yabrud caves in Syria, and of Neuville in caves in the Dead Sea valley, but details on these excavations were not then available. Miss Garrod, using a strictly French terminology until her uppermost levels, proposed a general chronological coincidence between her industries and those with equivalent names in western Europe. This interpretation gave her:

1. An occurrence of neanthropic (or at least non-classic neanderthaloid) fossil men who were contemporary with the classic neanderthaloids of western Europe, and
2. An occurrence of blade-tools in context with Acheulean-type hand-axes, hence earlier than the normal European occurrence of the blade-tools.

Vaufrey questioned this interpretation but it was generally accepted and in

1939, in a famous paper, "The Near East as a Gateway for Prehistoric Migration," Garrod proposed some as yet unidentified west Asiatic center (possibly the Iranian plateau) as the formative area for the whole blade-tool tradition.

For the later ranges of prehistory, Garrod's uppermost consistent material at Mt. Carmel, the Natufian, was variously described as "mesolithic" or "neolithic" (there have been and still are a half dozen different meanings for this word in use in the area). Claims of a transition from the final Natufian cave materials to the basal assemblage of the mound of Jericho were rejected. The earliest then (1939) available village materials all seemed to spring fully formed out of nowhere. These then earliest available village materials could be taken as (1) basal Jericho in the Dead Sea valley, (2) the basal Amouq-Mersin materials of Syro-Cilicia, (3) Ninevite I of the Tigris drainage about Mosul—this was to take on real substance and a new name with Lloyd and Safar's 1943–44 work at Hassuna, (4) and basal Sialk in north-central Iran. The materials of Bakun B II in southwestern Iran should perhaps also be added, although their full description is not yet available. This occurrence (Judeo-Levant ridge—Syro-Cilician hill country—Assyrian piedmont into Zagros foothills—Iranian highlands) indicated already the tendency for simple village-farming communities to make their earliest appearance along the hilly flanks of the Fertile Crescent. Following the basal Jericho, Amouq-Mersin, Ninevite I-Hassuna, and Sialk I levels, a more or less complete skeletal outline of the succeeding assemblages up to and into the time of literate history was available.

Since the War, the publications of Rust and Yabrud and of Neuville and Parrot on the Judean hill sites have appeared, and the publication of Ksar Akil nears completion. The appearance of blade-tool industries has been traced northwards along the Levant coast and the south coast of Turkey. Coon has investigated caves near Palmyra in the Syrian desert and the prehistory of Iraq has begun to unfold through the work of Solecki at Shanidar, and the several sites of the Iraq-Jarmo project. Coon's explorations and testings of widely separated areas of Iran have begun for the first time to raise the curtain on the Pleistocene of that area.

So far, the area has revealed nothing with the remote early-middle Pleistocene antiquity of Africa. Pebble-tools occur with an industry of Acheulean-type hand-axes and small flakes in Barda Balka, Iraqi Kurdistan, which Howe and Wright tested in 1951, but Barda Balka is not significantly early. Such blade-tool industries as Coon found on the Iranian plateau seem quite late to Movius, and increased understanding of the details of the blade-tool industries of the Levant and Kurdistan have forced Miss Garrod to abandon her old thesis of an early center for the blade-tool tradition (in its universal sense) in western Asia. In 1953, Garrod concluded that "western Asia, after all, appears to have made no direct contribution to the development of the European blade-tool culture," neither in its proper Aurignacian nor in the Chatelperronian or Gravettian sense. This is not to say that proper blade-tools do not appear in western Asia and early—they do appear there, and can even be interpreted as being seen at various stages of their evolution in (and probably from) an otherwise Levalloiso-Mousterian general type of context.

As Miss Garrod points out, the area has as yet no known climatic or faunal break to set off clearly the "paleolithic" from the "mesolithic" as in Europe. Blade tools continue to be made at least until the village-farming communities are well under way. We have attempted to cut the Gordian knot of difficulty in separating "mesolithic" from "paleolithic" by referring to the late Pleistocene blade-tool industries which include a significant microlithic element as the trace of an era of terminal food-gathering. This is not very precise, but it does tend to delimit a block of materials which suggest a phase of culture somewhat like that of the Willey and Phillips "archaic". A fair number of sites can begin to be listed for this *terminal food-gathering era*, and it is probably significant that some are open-air sites, not rock shelters. In Iraqi Kurdistan, where materials of the Zarzi-Palegawra backed-blade and microlithic industry fall near the end of this range, How grows increasingly impressed with the absence of anything later than the Zarzian in the caves—and with the appearance of some Zarzian open sites. There may be no Natufian equivalent in the caves in Iraq, only open-site material. Recently, Parrot has reported some sort (possibly burial) of open-air Natufian site in northern Palestine.

On typological grounds—and in some cases stratigraphic grounds—the next block of materials includes elements which suggest the fumbling beginnings of food-production. We refer to this as an *era of incipient agriculture.* Some of these materials come from caves, some from open sites. There is ground stone, sickle-sheen on flint blades, and a somewhat more elaborate assemblage than that of the preceding era. The Natufian of the Levant coast and Karim Shahir of Iraqi Kurdistan are cases in point, as are doubtless parts of Coon's Belt and Hotu caves. One calls this material "mesolithic" on pain of confusing it with what goes before, and "neolithic" on pain of confusing it with later materials. The phrase "era of incipient agriculture" seems to express best what is going on within it—the Willey and Phillips "pre-formative" has a different color of meaning.

Next comes a block of material which clearly represents the establishment of the *village-farming community*. Rarely found in caves, these materials come from mound sites with several levels of positive architectural expression. While, at the beginning, flint tools and microliths are still the greatest bulk in the assemblage, and pottery is lacking, there are clear indications of food-production. Jarmo in Iraqi Kurdistan is a case in point; M'lefaat, which we tested very briefly in 1954–55, is a typologically earlier example. Basal Jericho becomes increasingly enigmatic to me, with its large size and architectural elaboration. This era includes another of the types of material often called "neolithic", but since it does represent the beginning phases of settled village-farming communities I propose to refer to it in its own terms. The sites definitely cluster about the hilly flanks of the Fertile Crescent. Very presently there is a fingering into the riverine area proper, where the steps towards the stage of literate history were first taken.

Thus it now *appears* that the foothill-intermontane-valley zone of the flanks of the Crescent will prove to be an area of nuclearity for the formation of cultures of village-farming community type. The previous era—that of incipient agriculture—is still extremely difficult to bring into focus, and will probably always be elusive. If it did have a sort of nuclear area of its own, we don't yet know where this was. It could have been, perhaps, part of the speculative center which Sauer and Wissmann favor for southeastern Asia. Personally, I would rather reserve my judgment in the matter until—with the aid of in-the-field studies by professionally competent natural scientists—we are able to do more in the reconstruction of the pertinent contemporary environments. Back of the era of incipient agriculture lies (at least for the sake of convenience in classification) a terminal era of food-gathering, beyond that, full Pleistocene prehistory.

If western Asia remains politically stable and with conditions which facilitate scientific research, an enormous amount of very useful work can be undertaken. The surface is vast and only scratched from a prehistorian's point of view; the old-style royal-tomb and cuneiform-tablet archeologists were little concerned with the sites we seek or the questions we ask. We are, in fact, only learning ourselves how to put the questions properly.

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Note: the essential items of bibliography are all contained in Robert J. and Linda Braidwood, "The Earliest Village Communities of Southwestern Asia," *Journal of World History* I (1953). It hardly seems necessary to repeat them here.

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ANTHROPOLOGY AND THE SMITHSONIAN INSTITUTION

Leonard Carmichael

As Secretary of the Smithsonian Institution, I consider it a great privilege to be one of those to welcome all members of this Congress to its sessions. It is especially an honor to express a most cordial greeting to the professional anthropologists who are attending the Congress from beyond the borders of the United States. We are conscious of the honor that you have paid us in coming here. I am sure that I speak for all of my colleagues when I say that we are anxious to be of any possible service to you while you are here.

I presume that the authorities of this Congress invited me to be one of the speakers at this session because I am Secretary of the Smithsonian Institution of Washington. For the 110 years of its existence, the Smithsonian has been concerned with matters that now fall within the scope of present-day scientific anthropology. It occurs to me, therefore, that as a footnote to the knowledge of the history of your subject, you might be willing to have me say something for a few minutes about the Smithsonian Institution as it relates to the history of anthropology in America.

James Smithson, our founder, was a distinguished, aristocratic English scientist who had never visited America. On his death he left what was, for that time, a very large fortune to the people of the United States to found in the city of Washington an institution for the increase and diffusion of knowledge among men.

With the formal establishment of the Smithsonian in 1846, the federal government turned to the new Institution to take over the many uncoordinated studies of the American Indian that were then in progress. It is an interesting, historic fact that this type of ethnographic research from the first days of the Smithsonian has been kept strictly separate from the many functions of Indian administration.

The first Secretary of the Smithsonian Institution, Joseph Henry, possibly the greatest scientist born in America between the time of Franklin and the Civil War, showed great interest in all branches of ethnological and anthropological research of his period. In the original plan of organization of the Smithsonian which Henry himself prepared and which was formally adopted by the Board of Regents on December 13, 1847, it was provided as follows: "Ethnological researches, particularly with reference to the different races of men in North America; also explorations and accurate surveys of the mounds and other remains of the ancient people of our country." One of the classes of publications provided for in the program for the diffusion of knowledge was "ethnology, including particular histories, comparative philology, antiquities, etc."

From the first, Secretary Henry made it a point to keep in touch with recognized scholars engaged in research in anthropology, as well as in all other fields of science. One of these scholars was Albert Gallatin, who has been described as the first to bring order and system to the Tower of Babel that up to that time had been the problem of American Indian linguistics. The extended correspondence of Henry with Gallatin and with Lewis Henry Morgan, now in the Smithsonian files, constitutes an interesting record of the development of anthropological and linguistic thought at this significant time.

Secretary Henry energetically sought means of publishing the reports of government-sponsored and transcontinental railroad expeditions as well as those of the Smithsonian itself. It is a notable fact that previous to the formal founding of the Smithsonian's special Bureau of American Ethnology, the Institution had already issued upward of 600 papers dealing with ethnology and archaeology. It may indeed be noted that the first scientific monograph published by the Smithsonian Institution was the now classic "Ancient Monuments of the Mississippi Valley" by E. G. Squier and E. H. Davis. This beautifully illustrated and printed study is Volume I of the well-known series, *Smithsonian Contributions to Knowledge*. Today the Smithsonian has published more than 7,000 books and monographs dealing with a wide range of scientific and academic studies in mathematics, physics, chemistry, biology, geology, and other fields as well as in anthropology.

During the Civil War, Secretary Henry of the Smithsonian Institution was one of those instrumental in founding the National Academy of Sciences. This academy now has an active section on anthropology. The Academy is also concerned in scientific work through its affiliation with the National Research Council. This council has an important Division of Anthropology and Psychology. This division serves as a national center for research related to these two sciences. Through the years the Division of Anthropology and Psychology of the National Research Council has also done much to promote a stable relationship between those aspects of anthropology and psychology which deal in an applied way with man and his behavior.

The great date for anthropology at the Smithsonian was 1879. It was in this year that our amazing John Wesley Powell founded the Bureau of American Ethnology of the Smithsonian Institution. With this event, the Smithsonian historian feels that anthropology in our country came of age. Powell was not enthusiastically favorable in his expressed judgments of even the Smithsonian's own early publications in his chosen field. He asserts, as other later scholars have sometimes also been tempted to say, that "most of the literature of the past is a vast assemblage of argument in support of error." Powell made it his mission to eliminate from consideration in his new Bureau of Ethnology all treatment of unverifiable facts or of merely speculative theories about the lost tribes of Israel and other fancies dealing with the origin of the American Indians.

The energetic one-armed Powell is thus still almost a canonized figure at the Smithsonian. His epic first trip through the Grand Canyon of the Colorado made him a national hero. He began with a scientific interest in geology, but on his exploring trips he soon became more deeply concerned with the human problems involved in a systematic study of the North American Indian. Once he had secured the establishment, under the sheltering arms of the Smithsonian, of his new glistening Bureau of Ethnology, he resigned as Director of the Geological Survey, a great governmental organization which he also had helped found. From that time on he devoted the remainder of his dynamic and colorful life to promoting in his own way the systematic scientific study of every aspect of the archaeology, the physical makeup, the linguistics, and the habits of life of the various American Indian groups.

Powell's very extensive personal dealings with the Indians made him sure that a scientific study of all the Indian peoples would be of lasting value to ethnological science. He also was convinced that such investigations would provide a basis for wise national policies and even for military relations with the great tribes of the west. Powell very early recognized that aboriginal Indian culture was disappearing and was soon destined to vanish. It was thus his continuing aim to record the nature of this life before it was lost forever as an object of scientific study. In his opinion, such a record would be no sterile data pile, but it would rather have far-reaching significance for the sociologists and historians as well as for the anthropologists of the future. He even allowed himself to think that the kind of scientific racial study which he proposed might help in the high endeavor of improving the lot of human beings everywhere, even in modern cultures.

Powell chose as his bibliographer in the new Smithsonian Bureau James C. Pilling. This was a wise choice. Before his death, the Smithsonian published nine large volumes of bibliographical material, based on Pilling's meticulous study of appropriate materials in the major libraries of this country and Europe. With these bibliographic materials as their starting point, Powell and his collaborators, in the tradition of Gallatin, took such strides toward a linguistic classification of Indian languages that in 1891 the famous "linguistic map of North America" was published in the Seventh Annual Report of the Bureau. It is said that this work was so well done that, save for minor refinements, the 65-year-old map is still accepted by scientific linguists as basic in their work with Indian speech.

Powell's other task, which was conducted simultaneously with the linguistic program, was the preparation and publishing of his synonymy designed to eliminate the endless confusion of tribal nomenclature. Eventually almost all the leading anthropologists of the country became collaborators in this monumental work, which was finally published in 1907 as the "Handbook of American Indians North of Mexico." This justly famous "Bulletin 30" of the Bureau of American Ethnology is still in great demand. Dr. Matthew W. Stirling is the present distinguished successor of Powell as head of the Bureau of American Ethnology. At this point, I would like to express my gratitude to Dr. Stirling for his help in writing this paper. Dr. Stirling asserts that this encyclopedic two-volume handbook still remains the "bible" of serious students of North American ethnology. It far transcends its original purpose of settling mere tribal names. It brought order to such matters as the differing social and political organizations, the religious, and the general life practices of the amazingly varied Indian groups of this continent.

Cyrus Thomas was selected by Powell also to bring order to a tangled scientific field. His task was to clear up the chaos of New-World archaeology. In 1890, Thomas published a critical report on all the papers relating to mound explorations that had been so fully published previously by the Smithsonian. In this report the fog of mystery was first lifted from the subject of the Mound Builders. He demonstrated that these great and interesting American human monuments were constructed by Indians. In an especially sad reversal of previous romance, he was able to show that even some of the most impressive mounds were erected by known tribes and in historic times.

While these varied and general studies were being carried forward, the Smithsonian continued to publish monographs on particular tribes and on special topics related to Indians. Among the subjects of such study which may be mentioned is that of H. C. Yarrow on mortuary customs; Garrick Mallery on petroglyphs and picture writing; James Mooney on population statistics; Stewart Culin on games and sports; and William H. Holmes on the lithic industries.

Following Powell's death in 1902, he was succeeded in the headship of the Bureau by one of the investigators he had trained, William H. Holmes. Like his notable predecessor, Holmes was a dynamic as well as a scholarly man. He, also, had been an explorer in the west. He was a most versatile man, for he made lasting contributions to scientific geology, to several areas of anthropology, and, surprisingly enough, also to art. His pictures of the early great West and especially of the Grand Canyon are said to be among the finest ever made. His stratigraphic study, published in 1885 as "Evidences of the Antiquity of Man on the Site of the City of Mexico" brought his name to the favorable attention of scientists throughout the world. His remarkable two-volume work *Archaeological Studies Among the Ancient Cities of Mexico* had a similarly favorable acceptance.

Under Holmes and his successors, the original program outlined by Powell, with constructive modifications, has been continued and is still a vital guide in the work of the active, present-day Bureau of American Ethnology of the Smithsonian. Among some of the more recent publications in the tradition of Powell and Holmes that may be mentioned are John R. Swanton's "Handbook of the Indians of the Southeast," A. L. Kroeber's "Handbook of the Indians of California," and the monumental seven-volume "Handbook of South American Indians," edited by Julian H. Steward.

During the almost fourscore years of its existence, the Bureau has published some 225 volumes on the scientific study of the American Indian. This truly vast accumulation of knowledge preserves for posterity a detailed knowledge of nearly every aspect of American Indian life. Scholars who otherwise could not have known the great, human picture presented by the Indian tribes of our continent thus have a lasting base of accurate scientific working material.

In addition to the Bureau of American Ethnology, the Smithsonian Institution is also concerned in the scientific development of anthropology in a different but closely related administrative organization: the Department of Anthropology of the United States National Museum. This Museum is the largest of the ten units of the Smithsonian, and its more than 43 million catalogued objects makes it now probably the largest museum in the world. William Henry Holmes, who did so much for the Bureau of American Ethnology, more than any other man, must be thought of as creating the present active program of the Department of Anthropology of this great Smithsonian museum. From 1897 to 1902 he served as Head Curator of the Department, then, as we have seen, for a time as Chief of the Smithsonian's Bureau of American Ethnology, only to return from 1910 to 1920 again as Head Curator of the Department of Anthropology of the Museum.

Among Holmes's colleagues were Professor Otis T. Mason, Thomas Wilson, and Charles Rau. Walter Hough, from 1886 until 1935, worked at the great task of cataloguing the Smithsonian's monumental anthropological collections, which are among the largest in the world. He also was concerned in sixteen International Expositions which added to the world's anthropological knowledge as well as to the collections of the Smithsonian.

During the administration of the Smithsonian's notable third Secretary, Samuel P. Langley, a new division of the Department of Anthropology, known as the Division of Physical Anthropology, was created. The initiation of work in this field was largely due to the transfer to the Smithsonian of a small collection of human skulls, then stored in the Army Medical Museum. Dr. Aleš Hrdlička came to the Smithsonian in 1903 to organize this new work. By the time of his retirement in 1942, the physical-anthropological specimens of the Smithsonian numbered 36,814. A large part of these collections had been made and intensely studied and published by Hrdlička himself in his "Catalogues of Human Crania in the United States National Museum." The important work initiated by this distinguished scientist is carried on today by two outstanding members of the Smithsonian staff, Dr. T. Dale Stewart and Dr. M. T. Newman.

During the Second World War the Institute of Social Anthropology of the Smithsonian Institution, under the directorship of Dr. Julian H. Steward and Dr. George M. Foster, conducted a number of specific studies in social anthropology in Central and South America, from which important publications resulted.

In October of 1945 the River Basin Surveys were established as a unit of the Bureau of American Ethnology in order to fulfill the Smithsonian Institution's part in the Inter-Agency Archaeological and Paleontological Salvage Program which had been organized as a cooperative undertaking between the Institution, the National Park Service, the Bureau of Reclamation of the Department of the Interior, and the Corps of Engineers of the Department of the Army for the purpose of recovering such materials as would be lost through the construction of dams in the numerous river basins throughout the United States. Dr. Frank H. H. Roberts, Jr., Associate Director of the Bureau of American Ethnology, has served from the first as Director of the River Basin Surveys program. Field work actually got under way in the summer of 1946 when three parties began a series of surveys in proposed reservoir areas in the Missouri Basin. The program expanded rapidly to other parts of the country and by the summer of 1950 the original staff of six archaeologists has grown to fourteen full-time archaeologists, one geologist, and eighteen clerical and laboratory workers.

During the ten years in which the River Basin Surveys have been operating, archaeological surveys and excavations have been conducted in 244 reservoir areas in 27 states, and four canal areas and one lock project also have been investigated. The survey parties located and reported 4,365 archaeological sites and recommended 862 of them for limited testing or excavation. By the end of the 1956 summer field season 335 sites in 47 reservoir basins scattered over 17 different states had either been tested or partially dug. The information obtained from them has added greatly to the knowledge of the history of the American Indian and of the early contacts between the aborigines and the encroaching European civilization. The excavations have been in sites ranging from those occupied 10,000 years ago by hunting peoples to those occupied as late as the 1870's by earth-lodge dwelling Indians on the Western Plains. The results of certain phases of the excavation have appeared in various scientific journals, in Bulletins of the Bureau of American Ethnology, and in the Miscellaneous Collections of the Smithsonian Institution. At present a large number of manuscripts have been completed and are awaiting publication. Most of them will appear in the Bulletin Series of the Bureau of American Ethnology. During the course of the investigations numerous state and local institutions have cooperated in the program and have contributed to its success.

Besides those whose names have already been mentioned, the active staff of the Smithsonian in the field of anthropology now includes Dr. Frank H. H. Roberts, Jr., Dr. Henry B. Collins, Dr. W. C. Sturtevant, Frank M. Setzler—parenthetically, may I here also express my appreciation to Mr. Setzler for his help in preparing this paper—H. W. Krieger, Dr. Saul H. Riesenberg, Dr. W. R. Wedel, Dr. Clifford Evans, his wife, Dr. Betty J. Meggers, and a number of other scientists.

As the seventh Secretary of the Smithsonian, I am happy to continue to support the academic program in anthropology devised by my first predecessor, Joseph Henry. He and his successors have found the dictates of James Smithson to be at once inspiring and also amazingly realistic. In anthropology as in other fields of science, therefore, the modern Smithsonian concerns itself with both Smithson's increase of knowledge, that is, research, and also with the task of the diffusion of knowledge. The main avenues of diffusion of knowledge at the Smithsonian are publication and the development of educational exhibits in its museums to inform the public, in what it is hoped is an attractive way, concerning the advances of knowledge in each great scientific field. The record of the Smithsonian Institution during its first 110 years in the science of anthropology is thus surely one of useful service.

This present international congress will certainly result in the clarification of new ideas and the explication of new research techniques. Your work here will thus help the Smithsonian Institution and all other anthropological research centers to do even better the great work of advancing a sound and factual science of man. By your assistance we at the Smithsonian will gain in knowledge concerning the physical, social, spiritual, and material constructs and insights of a strange talking mammal called *Homo sapiens.*

Smithsonian Institution,
Washington, D.C.

ANTHROPOLOGIE PHYSIQUE EN U.R.S.S.

G. F. Debetz

En URSS le terme l'anthropologie tout court veut dire l'anthropologie physique.

Cette science est représentée par les institutions suivantes:

1) L'Institut de l'Anthropologie de l'Université de Moscou. Cette institution est réservée exclusivement aux recherches scientifiques. Un musée est adjoint à l'Institut.

2) Chaire de l'Anthropologie de l'Université de Moscou. La chaire s'occupe de l'enseignement ainsi que des recherches scientifiques.

L'Institut et la Chaire constituent la section d'Anthropologie du Conseil Scientifique de la Faculté de Biologie.

3) Section d'Anthropologie de l'Institut d'Ethnographie de l'Académie des Sciences de l'URSS. Le laboratoire d'anthropologie du Musée à Léningrad est attaché à cette section, ainsi qu'un laboratoire spécial qui s'occupe de la restitution des têtes des Hommes préhistoriques.

En outre il existe une Chaire d'Anthropologie à l'Université de Tachkent et des laboratoires d'Anthropologie aux Académies des Sciences de l'Ukraine, de la Géorgie, d'Azerbaidjan, du Tadjikistan et de l'Esthonie.

Enfin les recherches anthropologiques se poursuivent d'une manière plus ou moins privée par quelques anatomistes des Instituts de Médecine.

L'activité anthropologique se poursuit dans trois domaines.

1) Dans celui de la Morphologie humaine y compris l'anthropologie appliquée.

2) Dans le domaine de l'Anthropogenèse y compris les études des Primates.

3) Dans le domaine de l'Anthropologie ethnique (Raciologie).

Les recherches morphologiques durant la dernière décade concernent surtout les études sur l'anthropologie appliquée (standardisation de vêtements, de chaussures etc.). Des procédés biométriques ont été établis par M. V. Ignatiév pour calculer la quantité la plus rationnelle de différentes pointures de tel ou tel objet.

Au cours de ces recherches on a rassemblé une vaste documentation anthropométrique concernant les personnes adultes et les enfants des deux sexes. Ces matériaux servent maintenant pour des études les plus diverses. On a constaté par exemple que les courbes de croissance des dimensions de la tête et de la face ne présentent pas la prépondérance des filles par rapport aux garçons qui se manifeste comme on sait dans la courbe de la croissance de la taille à l'âge de 11 à 14 ans. Les matériaux céphalométriques ont permis de vérifier les tableaux de croissance des dimensions du crâne établis d'après l'étude de quelques centaines de crânes d'enfants.

Les résultats des travaux du laboratoire de la restitution plastique dirigé par M. M. Guérassimov sont exposés dans les Musées de Moscou, de Léningrad et d'autres villes soviétiques. Pour le grand public les têtes restituées dans le laboratoire sont beaucoup plus éloquentes que les crânes. Un album de ces têtes est en état de préparation. Mais ce n'est pas seulement la popularisation de la science qui est le but unique des travaux de ce laboratoire. Des questions purement morphologiques s'y posent également. Les corrélations entre les particularités du squelette facial d'une part et les tissus et les téguments de l'autre étudiées surtout à l'aide des radiographies sont l'objet principal des travaux effectués par le laboratoire.

Une communication spéciale a été déja présentée par l'auteur du présent rapport sur les découvertes paléoanthropologiques le nombre desquelles est considérablement accru durant les dernières années.

Ce sont surtout les problèmes de l'évolution de l'humanité qui ont attiré l'attention des chercheurs soviétiques dans le domaine de l'anthropogenèse. Des études critiques ont été publiées par J. J. Roguinski sur les trouvailles de Swanscombe et de Fontéchevade. Dans ces études l'auteur met en doute l'hypothèse sur l'existence du Praesapiens. Nombre d'hypothèses ont été émises sur les causes de la transformation de l'Homme néandertalien ou prénéandertalien en Homme actuel. D'après P. P. Efimenko c'est l'exogamie qui a provoqué la formation de la nouvelle espèce par suite d'un mélange intense de différents groupes, jusqu'ici isolés. Selon G. A. Bontch-Osmolovski c'est l'adaptation progressive du tronc et des membres à la marche bipède et aux conditions du travail qui étaient les agents principaux au cours de la susdite transformation. Enfin d'après J. J. Roguinski ce sont plutôt les qualités mentales étroitement liées avec les caractères physiques qui ont permis à l'Homo sapiens de prendre le dessus au cours de la sélection naturelle.

La question des causes de la naissance de l'Homme actuel se pose non seulement devant les partisans de la thèorie concernant l'existence d'une phase néandertalienne (ce n'étaient pas nécessairement les Hommes moustériens de l'Europe), au cours de développement de l'humanité. De même pour les adeptes de la théorie sur l'existence du Praesapiens des questions analogues se posent (au moins si l'on admet la conception monophylétique) car l'ascendant commun des Praesapiens et des Néandertaliens devait en tous les cas avoir les pricipaux caractères néandertaloïdes ou pithécanthropoïdes: boîte cranienne aplatie, front fuyant, face grande, menton absent, posture bipède plus ou moins imparfaite etc.

Une discussion a été organisée en 1949 sur les problèmes de l'anthropogenèse. Deux points de vue ont été discutés.

Selon les uns (et c'est la majorité des anthropologistes soviétiques avec J. J. Roguinski en tête) l'espèce de l'Homme actuel (Homo sapiens) s'est formée sur une aire relativement limitée. Il existait ainsi un Homo sapiens non divisé en races. Selon ce point de vue les Néandertaliens "classiques" (L'Homme de La Chapelle etc.), les Hommes de Ngandong et de Rhodésie ne sont pas les ascendants des races actuelles.

Selon les autres (et c'est l'avis de l'auteur de la présente communication) les races du stade néandertalien de l'Europe de l'Asie et de l'Afrique ont donné naissance aux races modernes. Un Homo sapiens indifférencié n'existait donc jamais. Ainsi les Néandertaliens de l'Europe sont les ascendants des Europoïdes; les Hommes de Ngandong et de Broken-Hill ceux des Australonégroïdes; on n'a pas encore trouvé les ascendants des Mongoloïdes modernes, mais c'est peut-être le Sinanthrope qui en est un. Donc F. Weidenreich avait raison en ce qui concerne les rapports phylogénétiques mais l'éminent anthropologiste avait tort quand il pensait que le développement de l'humanité était dû à des causes spontanées. C'est toujours dans les conditions de la vie qu'il faut chercher ces causes.

L'étude, encore inachevée, des encéphales des Hommes fossiles comparés aux ceux des Primates est entreprise par le laboratoire de cérébrologie dirigé par J. G. Chevtchenko (Institut d'Anthropologie de l'Université de Moscou).

Les études raciologiques se poursuivent surtout dans trois domaines: les questions théoriques constituent la base du premier; la paléoanthropologie raciale (du Néolithique, de l'Age du Bronze etc.) appartient au deuxième; enfin le troisième domaine traite les questions sur la somatologie ethnique.

Les questions de la valeur relative des caractères anthropologiques a été largement discutée. On a établi une échelle hiérarchique des caractères somatologiques et craniologiques. En ce qui concerne le crâne c'est l'étude de l'aplatissement transversal de la face en général et du nez en particulier qui a surtout attiré l'attention des chercheurs. Aucune comparaison de types raciaux (fossiles ou modernes) n'est plus possible sans tenir compte de la valeur systématique (taxinomique) de tel ou tel autre caractère.

On a constaté que les caractères les plus importants ne peuvent pas être exprimés par les mensurations. Aucun diamètre, aucun indice anthropométrique ne peut servir pour la distinction des deux grand'races qui forment la population de l'URSS. La pilosité, la morphologie des pommettes, du nez, du pli palpébrale etc. doivent être fixés dans les enquêtes anthropométriques d'une façon descriptive. De graves difficultés méthodiques se posent alors. Il devient presque impossible de comparer statistiquement les descriptions faites par de différents chercheurs surtout quand il s'agit de types plus ou moins proches. On a imaginé des échantillons servant de modèles pour avoir la possibilité de comparer plus exactement les observations de divers chercheurs. Mais avec cela le problème n'est pas encore complétement résolu. On est souvent obligé de répéter les recherches, au moins partiellement, de corriger les notes des enquêtes à l'aide des photographies etc. On a cherché à établir des moyens pour mesurer les soi-disants "caractères descriptifs" mais jusqu'à présent cela n'a donné aucun résultat satisfaisant. Le procédé le plus efficace pour comparer les résultats obtenus par de différents chercheurs est l'étude simultanée de divers groupes.

Par exemple la mission anthropologique de l'Académie des Sciences de la Géorgie (1950–54) fait des enquêtes non seulement sur les Géorgiens (près de 5.000) mais aussi sur un nombre en somme à peu près égal d'autres peuples qui habitent la Géorgie et régions avoisinantes: les colons Russes et Esthoniens, sans parler des Arméniens, des Azerbaidjanais etc. De la même façon quelques groupes de Géorgiens et d'Arméniens sont étudiés par la mission azerbaidjanienne (1951–52). Les matériaux de la mission des républiques baltiques (1951–53) englobent à part quelque milliers d'Esthoniens, de Lettoniens et de Lithuaniens aussi les Russes et les Biélorussiens. La mission de Kirghisie (1953–54) a rassemblé 1.500 enquêtes sur les Kirghises et un nombre en somme à peu près égal sur les Ouzbeks, les Tadjiks, les Kasakhes et même les Altaïens. Les travaux effectués parmi les Russes du Nord englobent également les Karéliens et les Zyrianes etc. Avec ces "points de connexion" on arrive à la possibilité de comparer les différences relatives sinon les résultats des observations directes. On peut pousser la méthode plus loin encore.

Pour comparer par exemple les résultats des observations morphologiques des anthropologistes soviétiques et les anthropologistes américains de l'école de Harvard on n'a qu'à confronter d'abord les données obtenues sur l'étude des groupes plus on moins identiques. Comme exemple on peut citer les Esquimaux, les Coréens etc. dont l'étude peut servir de base pour la comparaison réciproque.

On connait déjà bien les caractères des deux grand'races de l'URSS (europoïde et mongoloïde). Cependant il y a encore beaucoup de difficultés en ce qui concerne la classification généalogique des races secondaires (ou types anthropologiques). La plupart des anthropologistes soviétiques sont quand même d'accord sur les questions générales de la classification. On distingue deux grand'races et deux groupes intermédiaires à savoir: le groupe ouralo-laponoïde (plus ancien) et le groupe touranien (plus récent). L'origine du groupe ouralo-laponoïde est encore l'objet de discussion. Deux hypothèses en ont été émises. Selon les uns, le groupe ouralo-laponoïde constitue un type spécial et indifférencié. Selon les autres (et c'est là la majorité des anthropologistes soviétiques) le groupe ouralo-laponoïde s'est formé comme résultat d'anciens métissages au cours du premier peuplement de la Sibérie occidentale et de la partie nord de la Plaine Russe.

L'étude des empreintes digitales et palmaires appuie la deuxième hypothèse. En tout cas il n'y a que des ressemblances exclusivement morphologiques (très limitées d'ailleurs) entre les ouralo-laponoïdes et les types du Nord-Est de l'Asie.

On a proposé une classification de types secondaires de la grand'race mongoloïde à savoir: le type arctique (avec Esquimaux, Tchouktchis et Koryaks comme représentants), le type baïkalien (Tongouses et Youkaghires), le type centrasiatique (avec Bouriates et Touviniens comme représentants les plus pures de cette race).

Pour la grand'race europoïde c'est la pigmentation et la forme du nez qui sont les plus caractèristiques au point de vue généalogique. L'indice céphalique (surtout si on tient compte des déformations dues à l'influence du berceau,—au Caucase et au Turkestan) n'a qu'une valeur très restreinte. Mais de grandes difficultés se posent quand on passe à la craniologie des races europoïdes. On n'a pas établi les caractères craniologiques des Europoïdes blonds et bruns.

La massiveté relative des crânes nordiques doit être considérée comme un caractère sinon primitif du moins protomorphe. Dans les anciennes sépultures du Caucase et du Turkestan, ainsi qu'en Asie antérieure et en Afrique du Nord on trouve souvent des crânes plus ou moins massifs, ce qui ne peut être considéré comme vestige des invasions nordiques. Ce sont plutôt les traces de survivance du stade protoeuropoïde ou cromagnien.

Le rôle et la validité des faits anthropologiques pour l'étude de l'ethnogenèse était également l'objet des études théoriques. Certes il n'existe pas de rapports directs entre la race et la langue, la race et la culture. De même ni la culture ni la langue ne servent pas de caractères raciaux. Mais cela ne signifie pas que les études raciologiques ne sont pas utiles pour les études historiques. Telle ou autre repartition géographique des races, ainsi que telles ou autres migrations ne peuvent pas être envisagées comme cause des évenements historiques. Néanmoins la géographie raciale est étroitement liée à l'histoire. Ainsi la somatologie ethnique et la paléoanthropologie raciale deviennent des sciences historiques.

Ce sont les anthropologistes qui ont révélés aux historiens que les éléments asiatiques ont pris part au peuplement du Nord de la Plaine Russe après la retraite des glaciers. C'est l'anthropologie qui a montré que les anciens habitants des steppes de la Sibérie méridionale ont été en relations étroites avec l'Europe. Les études anthropologiques ont permis de constater que les grands mouvements des peuples asiatiques ont commencé quelque siécles avant l'invasion des Huns. La somatologie ethnique a réussi d'établir la parenté la plus étroite entre les Toungouses et les Yukaghires, ce qui a permis d'affirmer que l'expansion des Toungouses fut surtout l'expansion de la langue toungouse. Au contraire la formation du peuple Yakoute est due réellement à une migration car physiquement les Yakoutes sont beaucoup plus proches aux Bouriates et aux Mongols qu'aux Toungouses. Chez les peuples turques de la région des monts d'Altaï et les Sayans on a procédé à la distinction de trois types ou plutôt trois genres de parenté anthropologique à savoir: parenté des Altaïens septentrionaux avec les Ougriens de la Sibérie occidentale, parenté des Altaïens méridionaux avec les Mongols et enfin parenté des Karagasses avec les Toungouses, surtout avec les Toungouses occidentaux. Ici les faits anthropologiques se concordent parfaitement avec les faits ethnographiques. Cette concordance suscite une observation générale: les types anthropologiques sont liés beaucoup plus étroitement aux groupes culturels qu'aux groupes linguistiques. Chez les Bachkires par exemple la coïncidence des types culturels et des types raciaux est presque parfait. La même observation peut se rapporter aux Ouzbeks: ceux qui étaient encore nomades au XIX^e^ siécle ressemblent aux Kasaks ou aux Kirghises; les Ouzbeks sédentaires sont apparentés aux Tadjiks.

Ce sont justement les rapports et les liens entre les observations anthropologiques, ethnologiques, linguistiques et préhistoriques qui sont actuellement l'objet principal des recherches des anthropologistes soviétiques.

Institute of Ethnography, U.S.S.R.

SUMMARY OF PALEO-ANTHROPOLOGICAL INVESTIGATION IN THE U.S.S.R.

G. F. Debetz

One cannot yet say whether the territory of the U.S.S.R. encompasses an area in which the transformation from ape to man took place. However, it is useful to recall that the morphological particulars of the teeth of the Tertiary ape (Udabnopithecus) found in Georgia in 1939 revealed several specific details similar to human teeth. One can point out among these details especially the position of the "cingulum" on the exterior (labial) side of the tooth, as is true for Sinanthropus, and none on the interior, as found in the chimpanzee or his ancestor, Proconsul.

In any case, it is obvious that man appeared in U.S.S.R. territory during the Chellean period. Hand axes typical of this period have been found in Armenia. In addition, Acheulean finds are known from (Ossetie du Sud), on the Caucasian coast of the Black Sea, in the Crimea, and in the Ukraine. But no remains of Abbevillean or Acheulean man have yet been found in Russian territory.

Mousterian remains are well known over a vast territory. Mousterian sites are known in the south of Bielo-Russia, on the Volga and in Turkestan. The most ancient paleoanthropological finds in the U.S.S.R. also belong to the Mousterian period.

Publication has very recently been made of a work concerning excavations in the Kiik-Koba cave (in the Crimea), where bones of two hands and two feet have been found. By comparing these finds with others, one can say that Kiik-Koba man has traits in common with the Neanderthaloids of Western Europe, and can be clearly distinguished from the fossil men of Mount Carmel in Palestine. The basic difference consists in the width of the feet and hands. The hands and feet of both Kiik-Koba man and the Neanderthals of Western Europe are much larger than those of Palestine man.

The Neanderthal infant's cranium from the Techik-Tache cave (Uzbekistan, 1938) has recently been given an additional examination. It has been stated that this cranium most closely resembles the infant crania from La Quina (France) and from Subalyuk (Hungary). The characteristic Neanderthaloid peculiarities are less pronounced in the Techik-Tache cranium than in that of the infant Engis II (Belgium). These peculiarities are, however, much more striking than those of the child's cranium from Palestine, Skhul I.

In 1953, in the Late Mousterian deposits of the Starosselie cave in the Crimea, the skeleton of a year-and-a-half-old child was found. In spite of several primitive characteristics (large teeth, etc.) Starosselie man uncontestably belongs to the species Homo Sapiens. There is no chance at all that it even belonged to the same developmental stage as that of Techik-Tache man; the latter is much more primitive. Skhul I occupies a place between Techik-Tache man and Starosselie man.

All these comparisons are based on extensive documentation brought together by Soviet anatomists—some not yet published, concerning the morphology of infant crania.

However, one must ask whether one has the right to use data obtained through studies made on modern man to define the level of phylogentic development of any fossil skull. With this in mind, we have made many comparisons with chimpanzee skulls. Of course, the dental age was taken into account.

The fact that the cranium of Starosselie belonged to the species Homo Sapiens makes it possible to postulate three alternatives:

1. That the cranium has been dated incorrectly. This supposition seems to us to be unlikely.
2. Starosselie man is the Homo Sapiens representative belonging to the same geological period as Neanderthal man. The author does not consider this hypothesis to be likely.
3. Starosselie man belongs to a very late Mousterian period, it is even possible that he represents a "mousteriform" culture, of a period between the Mousterian and the Late (Upper, Recent) Paleolithic.

The latter hypothesis, corresponding to many archaeological and geological data, seems in the opinion of the author most likely.

In addition to the regions previously inhabited, Upper Paleolithic man occupied the Russian plain as far as the Kama River, and, in Siberia, east to the Transbaikal region, and as far north as 55–6 degrees latitude.

One can already distinguish in the physical type of Upper Paleolithic man the earlier forms of modern races. At least this is the personal opinion of the author and of many Soviet researchers. Others (such as, for example, V. V. Bounak) believe that Upper Paleolithic man represents a sub-species polymorph, not yet divided into races.

In 1937, at the site of Afontova Gora near Krasnoiarsk, a fragment of a frontal bone with a very much flattened nasal ridge was found. One may recall that this is the essential characteristic of the Mongoloid race.

During the course of the construction of the Volga-Don Canal one found, next to the fossil bones of Pleistocene animals, cervus megaceros and others of the human skulls. One of these has a facial skeleton well preserved and is of the Cromagnon type. The other, of which the skull cap only is preserved, shows a few traits of resemblance with the Brno I skull. The fluoric analysis gives a considerable antiquity to these skulls, but their archaeological age is rather doubtful.

In the region of the village of Kostienki (close to the town of Voronej), where archaeological work has been carried on for a long time, four tombs were found in 1952–54, well dated from the recent Paleolithic. Among these, a whole skeleton was discovered at the Markina Gora station, well preserved, of a young adult male. The skull is characterized by an accentuated prognathism and by a large nose. The author seems to believe that the skeleton is related to the Grimaldi race, but it must be noted that it is distinguished by a very projecting nose. He is a small dolichocephal, and measures 160 cm. in height. At the Kostienki II station another skeleton of a more recent period was found; it is rather poorly preserved, only the facial bones being present. He is a Cromagnon, though the height is slighter than the usual or typical Cromagnon. Two other skeletons are children, one shows Cromagnon traits and the other seems related to the Przedmost skulls.

The skeletons of Fatma-Koba and of Moursak-Koba in Crimea are dated from the Mesolithic. These skeletons are Cromagnon in the broadest sense of the word. However, they are marked by a few isolated Negroid traits which are not strongly marked. During the past few years, several necropolis dating from the Mesolithic have been found in Ukraine not far from Dniepropetrovsk. There also skulls showing Cromagnon traits have been found, but most of these are characterized by original traits. They are strongly dolichocephalic (on several the index is as low as 60), and the face is narrow and long, the orbits high, which distinguishes this type clearly from the Cromagnon. The closest analogies are found in Kenya; the author believes these skeletons to be Paleomediterranean. The questions dealing with the genetic relationships of this type with Cromagnon are far from clear at present.

Several other series of skulls found in different regions seem to belong to the Neolithic and the Bronze age. In the trans-Baikal region is found a Mongoloid type with a very flat face, but relatively low. The Mongoloid type is predominant also in the region north of Lake Baikal, in the Irkoutsk region, but this time with a European admixture.

Further to the west, starting with the region of Minoussinsk and Altai, the Europoid groups are predominant. Here it is interesting to note that the traits of the Cromagnons (large face, low orbits) are preserved during a long time in U.S.S.R. The average of the bizygometric diameter of twenty male skulls coming from the Neolithic necropolis of Vovnighi, in Ukraine, is 145 mm; the average of eight female skulls is 134 mm. The descendants of the Paleomediterraneans with a narrow face, discovered in the Mesolithic necropolis in Ukraine, are unknown in the Neolithic period of this region. Their predominance in Mesolithic collections is apparently accidental. The Cromagnons of the Neolithic are genetically related not with the Paleomediterraneans of the Mesolithic but with the Cromagnons of the Paleolithic. The Cromagnon type is also characteristic of the Neolithic necropolis of Oleni Ostrov (on Lake Onega). But among these latter the face is slightly flatter, which makes one posit here the presence of some amount of Mongoloid admixture.

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RECENT TRENDS IN BRITISH SOCIAL ANTHROPOLOGY

Raymond Firth

An historian friend of mine once jokingly remarked that British social anthropologists are like comets—they blaze for a while across the sky with a brilliant light and then fade out, leaving a trail of gas behind. But maybe this was only by contrast with the fixed—and duller—stars in the galaxy of historians. It is certainly not true of Radcliffe-Brown, whose light burned with increasing intensity for over thirty years.

The death of Radcliffe-Brown a year ago has not in itself meant any marked change of direction in our intellectual road, since trends not in conformity with his approach were already in evidence long before. But his passing does mark the end of an epoch in British social anthropology—the period of recognition and establishment of the study as a discipline (if not a science) in its own right. His creative achievement in this is well known, but it is fitting that I should commemorate it here before this international gathering.

The loss of this Olympian figure, inspiring both respect and affection, does have certain implications. In view of Fortes's recent appreciative sketch (1955), it may not be felt that an immediate systematic stock-taking of Radcliffe-Brown's contribution to our subject is demanded generally. But more critical as well as eulogistic views are seeking expression. Even before his death one could perceive, among younger scholars who hardly knew him, a readiness to test his work in a neutral dispassionate spirit, to treat his principles—such as the equivalence of siblings or the solidarity of the lineage group—as heuristic tools but not necessarily as valid statements about a social reality.

Then for the first time for over thirty years—ever since Malinowski came to teach regularly in London—British social anthropologists have become an acephalous society. We have lost in Radcliffe-Brown a head, a Founding Father, senior not only by a full generation but senior also in proven power of theoretical construction, as his recent Introduction to *African Systems of Kinship and Marriage* showed. Now seniority is divided among a group of varying age and different kinds of achievement and outlook. As social anthropologists we all share the same technique and immediate aims, and internal harmony is probably greater now than at any former period. But our conceptions of the scope and method of our discipline, and of the nature of the generalizations we can produce, do differ, as in particular does the language in which we express them. These differences, now that Radcliffe-Brown has gone, may result in a diversification of our theoretical framework. There are signs that our various institutional emphases—one cannot call them "schools"—have resulted in a fruitful cross-fertilization which is likely to produce even more valuable work. But there may well be some change in balance, some more overt acknowledgement of alternative schemes of interpretation, now that Radcliffe-Brown can no longer serve in person as the classical standard of authority.

Since we are relatively a small body—not many more than 100 in our professional Association of Social Anthropologists—another recent death may also affect the trend of our work. In Radcliffe-Brown we have lost a systematist on a major scale. In Nadel we have lost the only one of our number who has as yet declared himself with a comparable breadth of system-making interest. An intellectual free-lance, blending in a skilful way the theoretical ingredients of sociology and social anthropology with a strong flavouring of philosophy, a trenchant critic yet a bold and constructive thinker with an insight into the middle-range of generalization, he has had a powerful influence. His untimely loss may mean the absence, for the younger generation especially, of an important bridging influence and synoptic viewpoint of a systematic order.

One further note on the subject of personnel. The intellectual strength of British social anthropology in the post-war years has been undoubtedly helped greatly by the expansion of research facilities and of teaching posts, in the United Kingdom and in the Commonwealth at large. Our teaching departments and our research institutes are still very active. But the period of expansion is now for the most part at an end. Indeed, it is felt that there is grave need for systematic financial support, on a considerable scale, for British field research, the life-blood of its scholarship. But leaving this aside, the check in pace of recruitment, at a time when publication of results from the earlier research has attained a steady flow, is likely to usher in a period of stock-taking and consolidation.

Linked to some extent with this halt in expansion has been the tapering off of an interesting and fruitful association, whereby in research and teaching posts young American scholars in Britain and the overseas Institutes have exposed themselves to the virus of our social anthropology and infected us with their own critical enthusiasms in return. Coupled with the frequent transatlantic academic visits, and the growing flood of American and European fieldwork abroad, this has made us in Britain feel that we share more than ever before a common set of problems and experiences in social anthropology with colleagues in many countries.

But it is with the progress and results of scholarship rather than with its personnel and machinery that we are concerned here.

In talking of trends in British social anthropology I must emphasize the continuity in studies of an orthodox structural type—though perhaps in a rather looser frame than formerly. Studies of social alignment, of groups, relationships and roles, as component elements of an integral social system, still occupy a basic place in our work, with kinship and allied themes in a central position. This is only partly a legacy of our earlier enthusiasm for the discovery of structural analysis as an intellectual, even aesthetic, instrument. It rests on our conviction, upheld now by more varied experience, that this is one of the inescapable aspects of our definition of social phenomena. There is now perhaps a more overt stress too upon functional concepts once again, but not to the neglect of the structural key.

Thematically, along these lines, there have been some developments. Concentration upon lineage theory in the narrow sense has given way to interest in a broader range of variation, including the more amorphous field of cognatic (bilateral) systems. Interest in specifically local ties in small groups has grown; status characteristics and structures, and institutional types are receiving more attention. Examples of such neo-structuralism are: establishment of the concept of "perpetual kinship" as a symbolic expression of political ties in some systems; exploration of the theory of marriage stability; study of lines of tension in social structure as revealed by witchcraft accusation; analysis of the integrative value of conflict.

Analysis has been extended from the simple, isolated, primitive, rural area to the diversified, contact-conscious, sophisticated, urban communities, including what may be termed nascent social systems. Studies of changing ideologies, of the clash and the coincidence of old and new elites, of the formation of new occupational categories and class groupings, of the dynamics of caste organization, are coming to be common among us.

Regionally, much of this work is still centred on Africa, with Southeast Asia and the Pacific in second place. But in common with the world-wide broadening of interest, India and the Middle East have been receiving more serious attention. A development of a rather special kind here has been research on communities of overseas expatriate Asiatics—of Chinese in Malaya, Borneo and Hong Kong, and of Indians in Fiji, East Africa, Mauritius and the West Indies. A further broadening of our field base, in common also with trends among our colleagues in other countries, is seen in the growing amount of research done in our own Western type of society. Unlike some of our American colleagues, we do not yet regard ourselves as "committed" to the study of our own society; we are still rather cautiously exploring our competence to analyse certain limited sectors of it. But we have already ranged from a Norwegian or a Cumberland parish to a London borough; from a housing estate to a coal mine, a printing works or an ethnic minority in dockland. We are aware that intensive studies of the structure and operations of small-scale communities or social aggregates need supplementation if they are to be significant for interpretation of the affairs of these communities or aggregates as sectors of larger national wholes. But we are becoming increasingly convinced of the value of such micro-sociological studies, even in highly industrialized urban conditions. Even the preoccupation with kinship, once thought to be an eccentricity in an urban industrial society, is coming to be recognized as a vital part of the anthropologist's contribution. We need not go so far as Nadel, in holding that its study may be one of the most important skills the anthropologist can bring to the study of modern societies. But in terms of theory, we can see how important sections of British society the family of orientation plays a much more important part in moulding the family of procreation than was suspected. And, from a practical angle, we are coming to see how an understanding of the workings of kinship in a Western setting can throw light upon social problems, as in the fields of education or of medicine.

In all this, the study of societies in change is not necessarily a special field or branch of social inquiry but an essential part or dimension of it. The more our attention has been focused upon change, the more have we come to regard it as falling within our normal terms of reference. At the same time, this tendency to admit social change as an aspect rather than a subject of study has posed certain theoretical problems for the conventional structural approach—problems of integration, of equilibrium—which we are only now beginning to face.

Partly for this reason there has in recent years been some critical overhaul of past theories and conceptual frames. (One instance of this is a book by about a dozen of us, re-evaluating the contribution of Malinowski to social anthropology.) What Beattie (1955) in his useful review has seen as "an increased concern with anthropological theory and method—with what social anthropologists are trying to do and should be trying to do"—has emerged in various forms. In one form it has become manifest as a more conscious construction and use of models of social systems or part-systems—a work in which we have derived much help from our colleague Lévi-Strauss. This is a salutary reminder in its stress upon the role of the anthropologist as *artifex*, not simple recorder. Yet such emphasis on his creative relation to his material may not be without its dangers if it fails to indicate how the model may be related to the human behaviour which our science purports to describe. But in the way it promotes exploration of alternative structures and their social implications, such model-making is beginning to do a valuable service in the study of social variation, its correlates and its limits.

Linked with this, though at the empirical, not the abstract end of the institutional scale, is a sharpened interest in quantification. There is also a renewed attention to comparative method. Practised fairly rigorously by Radcliffe-Brown in the *Social Organization of Australian Tribes* (1930–31), its revivals in the various Africanist symposia have been parallel rather than strictly comparable essays, as Schapera has pointed out. But recently narrow-range comparisons of stricter type have been produced, including Nadel's examples on religion and witchcraft from his own field-work.

Like model-making, such analyses demand the tranquillity and, even more, the literature, of the study. If R. H. Tawney told economic historians that they should get out on the ground more and see things for themselves—that what they needed was not more books but stronger boots—we now in British anthropology are beginning to murmur that what we need is more armchairs and fewer notebooks. These rumblings are to some extent contrariness. But they do indicate true feelings that the present richness of a decade of post-war field materials calls for a period of digestion. In such process leisure and reflection are important—and here we are finding in our professional life that in an economist's terms, time is a scarce good.

Another major trend is a more explicit concern with history—or perhaps better, with the time dimension of our material. I think it is correct to say that at no time have British social anthropologists seriously meant to ignore history properly so-called. It may be that like the man to whom truth was such a precious thing that he used it very sparingly, we have respected history so much that we have refrained too much from applying to it. We may have been purist in our firm distinction between history and pseudo- or quasi-history. But most of us have used historical materials, i.e., documentation, fairly freely when we could get them; some of us find the work of historians very germane to our problems and welcome historians into anthropological collaboration. Schapera has become a socio-historian with his studies of Moffat and Livingstone; and Evans-Pritchard has even been prepared to throw the baby into the bath-water and argue that social anthropology after all is only a kind of writing of micro-history. But we have all insisted on maintaining the distinction between interpretation of materials from the actual past, and from the present or near-present. We have also stressed the necessity of a sociological, not merely chronological, interpretation of tradition. In conformity with our more conscious interest in social change as part of ordinary process, with our increasing study of more sophisticated literate societies, with the greater accessibility of documents bearing on our problems—and perhaps also, the greater approachability of historians—a more definite place is made for history in our analyses, as, for example, by Barnes. We have even become less austere about the introduction of conjectural history into our accounts. But what concerns us primarily, and what seems to us very important methodologically, is the way in which the form of the social system conditions the interpretation of history. In contradistinction to what still sometimes occurs elsewhere, the British practice is to use the analysis of the contemporary society to elucidate the significance of its conjectural history rather than the reverse.

Another trend in our work is a reappraisal of the nature of a social system as such, a task in which we are being helped by closer contact with sociologists. Earlier analyses stressed its integration, its tendency to equilibrium, and, without specific admission of this, implied the dominance of the system as such over all individual choice. This view was not equally shared by all British social anthropologists, though the reaction against it took different forms. Recently, dissatisfaction with the rigidities of a purely structural approach, and recognition of the primarily static character of the integration concepts as hitherto expounded, have become more general. There has been insistence on the essential lack of integration evident in some social systems, on the lack of agreed social aims, on their structural inconsistencies, on the high degree of manipulation of the elements in the system by interested parties. There has also been more recognition of the need to allow for the social implications of alternatives in action and the operation of choices. The treatment tends to be rather in terms of open than of closed systems. The emergence of alternative structures is not ruled out. Whether or not this operational field is labelled "social organization," as I would term it, is a matter of convenience and largely verbal. What is significant is that it is a field that is rapidly being enlarged as people come to see its relevance for dynamic studies, whether in the constitution of descent groups, the workings of an authority system, or the application of a body of law.

If we cross-cut now in terms of the "functional" divisions of our subject-matter, we see economic anthropology, still the Cinderella of the group, attracting rather more attention; a stronger focus on political anthropology; and a select but highly sophisticated interest in legal anthropology, starred by Gluckman's recent study of judicial process among the Barotse. A trend of particular interest is the development in studies of ritual and religion, a development not without personal overtones of more than one kind, but likely to lead to a much better appreciation of the significance of symbolic behaviour in social action. Parallel with this is an increasing interest in the study of ideas and beliefs for their own sake, not simply as reflections of social structures. To some degree, though in another setting and with other terminology, this is an expression of interests which have emerged elsewhere in terms of "world-view" or "value" studies.

Finally, a few words about applied anthropology. Some British anthropologists, particularly those associated with the Research Institutes, tend to work fairly closely with governments in the overseas territories, and a few have specific posts in the administration. The task of such collaboration is made somewhat easier with the open committal of many British overseas territories to self-government. But the essential problems of working towards raising levels of living, and towards a more efficient local organization of public affairs remain much as before. Applied anthropology is a complex notion, with many gradings of commitment and clusters of interest.

In quite another field, there have recently emerged more conscious attempts to promote social anthropology as a cultural subject of more than professional interest. On the one hand, British social anthropologists have been encouraged by the BBC radio to put forward both regional and theoretical materials from their study as matters of general significance for any educated audience. On the other hand, such materials have also been offered to specialists in other fields—as Gluckman offered his work on the Barotse to lawyers, and as a number of studies in Britain are offering to medical men, or to industrialists, relevant background information bearing on their special problems. Some of these developments are promising. In conformity with our British tradition of scientific caution, we offer the information from these background studies not necessarily to solve problems directly, but to help others to understand better the nature of their problems. Some of us may regard this as one of the most important contributions our science can make to society.

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TRENDS IN EUROPEAN PREHISTORY[1]

Gutorm Gjessing

It is a very obvious fact that my task this evening is an absolutely impossible one, that it cannot be solved in a short Congress paper at all; thus the simplification necessarily will have to be brutally overdone. For the ethnic differences and contrasts so clearly visible in Europe today are, indeed, so old that they vanish only in an undefinable past, and even in prehistoric research various, more or less contending schools and lines of thought with different foci of interest predominate in different countries. Consequently any such survey will not only be hopelessly incomplete, but also subjective in so far that the relative importance of new discoveries and trends will depend on the particular interests of the individual scholar and on which part of Europe he happens to come from. Perhaps it may also be timely to mention that in the following sketch "archaeology" will be conceived of in the sense of the French *préhistoire*, thus exclusive of the later periods of the Iron Age.

Now, to those particularly interested in cultural origins and in tracing man's existence in their part of Europe as far back in remote times as possible, the big event has doubtless been the establishment of unquestionable evidence of the presence of inter-glacial man in northern Europe. In 1955 the Danish osteologist Ulrik Møhl-Hansen published his investigations on animal bones from inter-glacial deposits in Jutland in which he discovered typical marrow-split bones of fallow deer in a faunistic environment of Merck's Rhinoceros, *Dicerorhinus kirchbergensis*, *Megaleceros giganteus*, Steppe Bison, *Bison priscus*, etc.[2] Later on, Erik Westerby found a flake of flint in Kolding, Jutland, probably from Upper Palaeolithic times.[3] Moreover, A. Rust, the discoverer of the Meiendorf and Stellmoor stages of the Hamburg culture, recently claimed to have discovered remains of inter-glacial man in northern Germany, close to the Danish border, as well.[4] Yet, in any case, Mr. Møhl-Hansen's findings seem perfectly convincing, and corroborate earlier guesses by, *i.a.*, the Dane Hartz with whom inter-glacial man was almost an *idée fixe*, by the Norwegian botanist Rolf Nordhagen who in 1933 on geo-botanical grounds tried to argue for the bold idea of man having survived in Finnmark in northernmost Norway from interglacial times.[5] Dr. Rust, however, has also offered us another sensation, implements of quartzite and sandstone from the same site and the same strata where *Homo heidelbergensis* was found almost fifty years ago.[6]

The study of palaeolithic cultures has, on the whole, been intensively conducted over most parts of Europe. Thus the first "Five-Year Plan of Hungarian Archaeology" for the years 1950–54 proposed as its first point "the exploration of the caves in the Hungarian central chains of mountains; the discovery of the earliest traces of man, and the stratigraphical authentication of the observations made so far, and in connection with this the elucidation of chronology of the Upper Palaeolithic Era."[7] I have not had the opportunity to ascertain whether or not this five-year plan has been fulfilled, but the first year's explorations in the caves of the Bükk Mountains were claimed to have helped in determining the characteristics of Middle Aurignacian.[8]

The achievements of European palaeolithic studies have, however, been very aptly summarized by Hallam L. Movius both in the bibliographies of the Peabody Museum[9] of Harvard University and in the volume *Anthropology Today*.[10] Nevertheless the literature, even after his publication, has been very comprehensive, not least in France, the classical crade of palaeolithic studies, in part dealing with the possible correlation between Quaternary phenomena and the palaeolithic industry, in part extending the studies to aspects such as religion and, of course, art—and indeed even language and social relations. Applying the quantitative method of linguistics, A. Cailleux claims to have reached as far back as the Upper Palaeolithic period.[11]

German palaeolithic research seems still to be concerned chiefly with taxonomic and distributional studies partly influenced by the "Kulturkreislehre" and its derivate, the chorological method as formulated particularly by K. H. Jacob-Friesen in 1928.[12] It is very significant that the most recent (1954) German survey of European palaeolithic archaeology by Karl J. Narr has been titled "Formengruppen und Kulturkreise im europäischen Paläolithikum."[13] In Czechoslovakia, on the other hand, one is more interested in intensive explorations of total situations aiming at research on local, palaeolithic communities. As phrased by Bohuslav Klíma: "Thus . . . Czechoslovak archaeologists are beginning to show that the great palaeolithic loess sites were more than temporary encampments, but were considerable settlements with the oldest-known buildings of man, inhabited by a numerous society over a long period."[14] Among other things Dr. Klíma has published excavations at the Upper-Palaeolithic settlement on Pavlov Hills near Dolní Věstonice in Moravia which provide the first known palaeolithic huts outside the Soviet Union (such as those at Kostienki, Pushkari and some other places). Whereas one of the Pavlov huts was an ordinary dwelling, another one, according to Dr. Klíma, represented "the âtelier where the famous clay animal figures were fired, and where their maker, the medicine-man or shaman of the hunters, may also have lived."[15] The excavations have not been finished in 1954, and the aim of future research would, according to Dr. Klíma, be "to concentrate primarily on the elucidation of the general plan of the settlement, and of the relationship between the various buildings; in other words to study the problems leading to a clearer understanding of the structure, social and economic, of the society which inhabited southern Moravia twenty-five thousand years ago."[16]

Here Czechoslovak archaeologists are completely in conformity with a marked trend in the archaeology of northwestern Europe, most markedly represented in Great Britain and Denmark, where the words "economy" and "community" increasingly often occur in archaeological books and papers, the latter one often replacing the term "culture"; as phrased by Gordon Childe, the cultures defined by archaeologists "represent societies or phases in the development of societies."[17] This again means an increasing interest in what could be termed "socio-archaeology," only that some, perhaps even most, archaeologists find the study of economic systems based upon a close scrutiny of the ecological environment to be a necessary inquiry in order to attain valid reconstructions of the social systems of prehistoric communities.

This trend implies less stress to be put upon what Walter Taylor called "taxonomic rosettes"[18] and also upon far-flung comparisons between individual types of artifacts as against a deeper and more intensified study of archaeological situations, leading also to a renewed interest in the problem of migrations supplementing the traditional study of diffusion.[19] This whole trend has led to elaborations and refinement of the techniques of excavation as well.

Moreover it implies close collaboration with other fields of study, such as human geography and ecology, pollen analysis, botany, zoology, and also with various branches of anthropology. Thus Grahame Clark and others have stressed the importance of folk-culture for European prehistory,[20] the fruitfulness of which, by the way, was beautifully demonstrated by the Norwegian A. W. Brøgger as early as 1925.[21] Gordon Childe, on the other hand, to a certain extent following Marxist archaeologists, has amply shown the fruitfulness of drawing on the results of social anthropology.

The most conspicuous results so far have been an extremely thorough study of prehistoric agriculture initiated by Osbert Crawford's introduction of air photography in British archaeological research in 1922, by which he discovered traces of cultivated fields of types obviously of prehistoric origin.[22] The better definition of these types was made in Denmark in the course of the 1930's through the excellent excavations of Gudmund Hatt, and in Britain not least through the work of E. Cecil Curwen. Of paramount importance was the Danish pollen-analyst Johs. Iversen's 1941 study, "Land Occupation in Denmark's Stone Age,"[23] showing that a microscopic examination of the contemporary peat suggests that the forest clearings were made by burning the trees, and also that, then as now, weeds like mugwort, sorrel and plantain gave trouble to the farmer. The suddenness with which these clearings appeared in the pollen diagrams definitely suggested a concerted immigration of agriculturists. Later studies conclusively demonstrating that these immigrants were not the first agriculturists in Denmark but represented the later immigration of "battle-axe peoples" have put considerable emphasis upon the problems of the earliest introduction of agriculture in Denmark. Agreement seems to have been reached upon the "megalithic culture" being no adequate term, since the culture is basically more or less identical with the Central European "funnel-necked beaker" culture, whereas the megalithic tombs represent a later addition. As Stuart Piggott says in his broad discussion of British neolithic cultures: "To define a local culture in terms of its chambered tombs may be no more than indicating the boundaries of a sect."[24]

The most radical view so far is that of Troels-Smith, claiming that agriculture and cattle-breeding were integral elements in the Campignien-like, classical Ertebølle culture, the Ertebølle people thus being semi-farmers subsisting on hunting, fishing, gathering, and cattle-breeding as well as on cultivating small areas.[25] Thus the occupational culture of the Ertebølle people should have agreed in principle with that of the inhabitants of the early Swiss pile dwellings (Michelsberg and earliest Cortaillod), who, according to recent studies by E. Vogt, W. U. Guyan, J. Speck, Troels-Smith and others, grew grain and had domestic animals, but who nevertheless lived largely by hunting, fishing, and gathering of wild plants.[26]

Now, to return to the cultivated fields, a close study of their shape and dimensions has produced extremely interesting results. In part the form of the plots suggests the implements that were used in tilling them, since a plot cultivated with digging sticks or hoes would be shaped differently from one tilled by means of a spade or a crook plow, while these latter would be shaped differently than fields cultivated by means of plows turning the furrows in such a way that the cross-plowing of the spade-plow would not be necessary. The various types of fields can be dated by means of grave mounds and other prehistoric remains placed upon them, thus providing a *terminus ante quem.* In this way Poul Kjærum recently gave conclusive evidence for plows having already been used in Denmark in Middle Neolithic times, and P. J. R. Moddermann has published an account of neolithic cultivated fields associated with long houses of "band-ceramic" type in the loess area in Holland.[27]

Yet, as already mentioned, one of the chief characteristics of modern archaeology in northwestern Europe is the study of complex situations, and a combined study of cultivated fields, house ruins, fences, etc., has produced results of a much broader scope than this. Grahame Clark pointed out that in prehistoric times western Europe was almost entirely covered with forest and scrub,[28] consequently grasslands must have been very limited and largely the result of human tree-felling. The cultivated fields naturally lay around the homesteads, and it is accordingly possible to ascertain whether the people lived in separated farms or in villages. Moreover—and now I feel inclined to transcend the limits of the *préhistoire*, and for a moment to enter into the *protohistoire* or *Frühgeschichte*—the types of implements used may also suggest the stratification of society, as has been brought out clearly in Great Britain after the coming of the Angles and Saxons who brought with them the heavy, wheeled plow drawn by a team of at least four, but most often eight, oxen. A study of the English open-field system as it existed in the Middle Ages has shown that the Anglo-Saxon agriculture was possible only through cooperation since one peasant owned the plow, another one ox, a third one another ox, and so on. And in any event by the time of the Domesday Survey in 1086 the manorial system was firmly established.

I mention these British and Danish studies which have been aptly summarized by E. Cecil Curwen in his and Gudmund Hatt's book, *Plough and Pasture, 1954*,[29] because they clearly demonstrate that even research on agricultural techniques ultimately aims at determining the social systems. Yet it would be extremely unfair to believe that this trend is limited to Danish and British archaeology. In my own country, for instance, archaeologists are struggling with precisely the same problems, although the interest has here so far been more strongly focused on the social situation in the later Iron Age periods; thus the results fall outside the scope of the present survey.

The most comprehensive exposition of this trend, however, is to be found in Grahame Clark's big volume *Prehistoric Europe: The Economic Basis, 1952*, dealing not only with farming communities but with the whole range of systems of economic activities found in prehistoric Europe, and it is highly suggestive that the title of its first chapter reads: "Ecological Zones and Economic Stages." With the author's intimate knowledge of the archaeological material of all Europe, Clark has here made a really outstanding contribution.

Gordon Childe, perhaps the most versatile and learned archaeologist in all Europe today, recently has attacked the social aspects of prehistoric communities in a more direct way. His book *Social Evolution* is a most intriguing attempt to reconstruct the transformation of social systems through the ages. Although there is certainly room for considerable disagreement on many points, his book nevertheless shows clearly the fruitfulness of a functional and theoretical sociological approach to prehistoric problems. As far as the "Arctic Stone Age" of north Norway is concerned in an article "Prehistoric Social Groups in North Norway," which is still unpublished, I myself tried to combine archaeological results with ecological conditions and general social anthropological theory, and came to some interesting results. The population was organized in seminomadic, unstratified groups settled in villages of from a dozen to about thirty households. The groups most probably were clanless and unstable as far as the residents were concerned, with bilateral descent and households of one hunter and approximately three other persons. The political organization was extremely weak if existing at all.[30] Similar investigations should also be possible as far as agricultural communities are concerned.

Now, the trends mentioned here seem by and large to be confined to western and northwestern Europe and to the communistic parts of the Continent where they appear in a theoretically divergent disguise. On the rest of the Continent and in eastern Fenno-Scandia archaeology to a greater extent seems to be concerned with taxonomy, problems of diffusion, and with ethnic questions. Even here very important results have been achieved.

In Germany, as I have already mentioned, chorological methods are highly regarded. This, of course, being no novelty *per se*, is nevertheless a characteristic feature of post-war German study of prehistory and *Frühgeschichte*, so much so that in 1950 even a specialized journal devoted to chorological archaeology, *Archaeologica Geographica*, with Hans Jürgen Eggers as editor, was started with the ambitious aim of providing material for a comprehensive prehistoric atlas covering all Europe, and with M.-E. Marïen as editor.

Time has permitted me only to sketch the European situation with very few strokes. The picture thus given is, of course, extremely inadequate, all the shadings and quite a few important features of the archaeological landscape having had to be omitted; various European archaeologists certainly would find the picture closer to reality if quite different lines were drawn. This is inevitable. What I have tried to do—and the only thing I could do—has been to draw attention to some aspects which I personally find to be important.

Oslo, Norway.

Notes

1. I am greatly indebted to my friends Dr. Anders Hagen (Universitetets Oldsaksamling, Oslo) and Prof. C. J. Becker (University of Copenhagen) for valuable assistance in selecting relevant literature.
2. *U. Møhl-Hansen:* "Føste sikre spor af mannesker fra interglacialtid i Danmark," *Aarbøger for nordisk Oldkyndighed*, pp. 101–126 (summary in English), 1954.
3. *C. J. Becker:* "Istidsmennesker i Danmark," *Berlingske Aftenavis' Kronik*, Jan. 3, 1956 (Copenhagen).
4. *Becker: loc. cit.*
5. *N. Hartz:* "Bidrag til Danmarks tertiære og diluviale Flora," *Danmarks Geologiske Undersøgelse*, 2. Række, Nr. 20 (summary in English), 1909. *R. Nordhagen: De senkvartære klimavekslinger i Nord-Europa og deres betydning for kulturforskningen*, Oslo, 1933.
6. *A. Rust: Artefakte aus der Zeit des Homo heidelbergensis in Sud- und Nord-Deutschland*, Bonn, 1956.
7. *F. Fülep:* "The Five-Year Plan for Hungarian Archaeology," *Acta Archaeologica* (Budapest): 10–14, 1951.
8. *F. Fülep:* "The Results of Hungarian Archaeological Research in 1950," *Ibid.*, pp. 325–326.
9. *H. L. Movius: Recent Publications Mainly in Old World Palaeolithic Archaeology and Palaeo-Anthropology*, American School of Prehistoric Research. Old World Bibliography, Nos. 1–5 (Mimeographed), 1948–1953.
10. *H. L. Movius:* "Old World Prehistory," *Anthropology Today*, ed. *A. L. Kroeber*, pp. 163–174, 1953.
11. *R. Lantier:* "Recherches publiées en 1953," *Gallia*, 13 : (2) : 228–245, Paris, 1955.

12. *K. H. Jacob-Friesen: Grundfragen der Urgeschichtsforschung*, pp. 170 *et seq*, Hannover, 1928.

13. 34. *Bericht der Römisch-Germanischen Kommission* 1951–53 (Berlin), pp. 1–40. In his most recent publication, his contribution to the Wenner-Gren symposium on *Man's Role in Changing the Face of the Earth*, (ed. by William L. Thomas) 1956, however, Dr. Narr has taken a decidedly ecological point of view.

14. *B. Klíma:* "Palaeolithic Huts at Dolní Věstonice, Czechoslovakia," *Antiquity* (London), 28 : 14, 1954.

15. *Klíma: loc. cit.*, p. 14. The somewhat hypothetical huts published by A. Rust are mesolithic, whereas a couple of still more hypothetical tent-sites by Rust have been dated to Magdalenian, *vide* A. Rust in *Festschrift für Gustav Schwantes*, ed. *Karl Kersten*, p. 56, Figs. 4–7, Neumünster, 1951.

16. *Klíma: loc. cit.*

17. *V. G. Childe: Social Evolution*, p. 17. London, 1951.

18. *W. Taylor: A Study in Archaeology*, American Anthropologist, Memoirs, No. 69, 1948.

19. *V. G. Childe: Prehistoric Migrations in Europe*, Oslo, 1950. *N.-G. Gejvall, C.-A. Moberg*, and *G. Gjessing:* "Vittnesbörd om folkvandringar," *Fornvännen* 1955, h.1, Stockholm.

20. *J. G. D. Clark:* "Folk-Culture and the Study of European Prehistory," *Aspects of Archaeology in Britain and Beyond:* Essays Presented to O. G. S. Crawford, pp. 49–65. London, 1951.

21. *A. W. Brøgger: Det norske folk i oldtiden*, Oslo, 1925.

22. Air photography, however, is much older, the first known being a photograph of Paris in 1858. The first known application of air photography to archaeology was made by the Germans in Turkey during World War I under command of Dr. *Theodor Wiegand. Vide O. G. S. Crawford:* "A Century of Air-Photography," *Antiquity* (London), 28 : 7, 206.

23. *Danmarks Geologiske Undersøgelse* II, Række Nr. 66.

24. *Stuart Piggott: Neolithic Cultures of the British Isles*, p. 123. Cambridge, 1954.

25. *J. Troels-Smith:* "Ertebøllekultur-Bondekulture," *Aarbøger for nordisk Oldkyndighed* 1953 (summary in English). Among other important contributions to this whole problem may be mentioned *C. J. Becker:* "Die mittel-neolithischen Kulturen in Südskandinavien," *Acta Archaeologica*, XXV, Copenhagen, 1955; *Erik Hinsch:* "Traktbegerkultur—megalittkultur," *Universitetets Oldsaksamlings Årbok* (summary in French), Oslo, 1951–1953; and "Yngre steinalders stridsøkskulturer i Norge," *Universitetet i Bergen Årbok* 1954, Historisk-antikvarisk rekke Nr. 1 (summary in English).

26. *W. U. Guyan* (Ed.): "Das Pfahlbauproblem," *Monographien zur Ur- und Frühgeschichte der Schweiz*, XI. Basel, 1955.

27. *Poul Kjærum:* "Striber på Kryds og Tværs," *Kumbl. Årbog for jysk arkæologisk selskab* (Århus), pp. 18–24 (summary in English), 1954. *P. J. R. Moddermann* in *Bericht van de Rijksdienst voor het Oudheidkund.* Bodenondersoek, 1955.

28. *J. G. D. Clark: Prehistoric Europe: The Economic Basis*, p. 92. London, 1952.

29. New York, 1953.

30. To be published in the *Proceedings of the Prehistoric Society* (Cambridge).

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RECENT DEVELOPMENTS IN ETHNOLOGICAL THEORY IN EUROPE

Robert Heine-Geldern

While preparing this lecture, it soon became evident that it was practically impossible to deal with the whole of Europe within the time limit set. Given the choice between general and therefore meaningless phrases about all the countries of Europe or saying at least a few more or less relevant things about some of them, I have decided for the second alternative. Therefore, I shall speak mainly about ethnological theory in central and northern Europe, with only a few side glances at that in other European countries. I hope that you will condone this deviation from the title of my paper.

I must start with a post mortem, that of the *Kulturkreis* doctrine. This is the more necessary since it seems that its demise has not yet been fully realized in the United States.

There is no need of repeating all the criticisms that have been levelled at the *Kulturkreis* theory. It will suffice to say that the final blow came from archaeology. When Graebner, half a century ago, set up his *Kulturkreise* on the basis of an analysis of Oceanian and Australian cultures, the archaeology of these regions, as well as that of eastern and southeastern Asia, was practically non-existent. Today, even if we were willing to concede that his *Kulturkreise* ever existed in Oceania, archaeological evidence would still force us to recognize that these cultures were merely the results of the local intermixture of various ethnic and cultural waves which at different times came from Asia. It obviously would be senseless to search in other parts of the world for these same complexes of accidentally combined cultural traits. The attempt to use them as a basis for the cultural history of the whole of mankind was bound to lead into error.

For us, in Vienna, it is rather embarrassing that the terms "*Kulturkreis* doctrine" and "Vienna School of Ethnology" are still widely considered as synonyms. Even in its heyday, when Father Schmidt taught at our University, the doctrine was never universally accepted in Vienna and was opposed and criticized by several local ethnologists, including myself. Today, it may still linger on among a few scholars who studied in Vienna in the 1920's or 1930's and who later lost contact with us, but I can assure you that it has not a single partisan left in Austria and, to my knowledge, one only in Germany.

In order not to appear unjust, I wish to add that Father Schmidt and even more so Graebner have the real and lasting merit of having helped to extract European ethnology from the mire of obsolete pseudo-evolutionist and parallelist theories.

One can safely say that today the vast majority of Italian, Central European, and Scandinavian ethnologists conceive of ethnology as of an essentially historical discipline. Even if they do not explicitly say so, history is more or less at the back of their minds. In England and in America it has repeatedly been said in recent years that historical reconstructions are admissible only as far as written documentary evidence is available, and that to go beyond that point is useless and even unscientific. The majority of continental European ethnologists do not consider this narrow interpretation of the term "history" as justified, and reject such a resigned attitude. It is generally felt that the possibilities offered by comparative research in combination with distributional studies, with archaeology, linguistics, and physical anthropology have not nearly been exhausted. We may or may not approve of the views expressed in such works as Lévi-Strauss' *Structures élémentaires de la parenté*, Koppers' *Primitive Man and his World Picture*, Jensen's various books, or Baumann's recent book on the role of bisexuality in myth and ritual. What matters more than their particular conclusions is the fact that such books are still being written. It proves that the spirit of intellectual adventure is still very much alive and that on the old continent ethnology has not yet been affected by that paralyzing timidity—or should we rather say by that somewhat too puritan sobriety?—which seems to have struck some of our non-continental colleagues.

It is a fortunate circumstance that in the part of Europe with which I am dealing ethnology is not at present dominated by any systematized school of thought with all the dangers this would imply, such as dogmatism, intolerance, and the creation of fixed terms and catchwords which can be repeated mechanically as a kind of ritual. Although history is explicitly or tacitly considered as the backbone of ethnology this does not entail any kind of one-sidedness. I doubt that in the whole region a single ethnologist could be found who would not gladly admit the justification and desirability of the sociological, functionalist, psychological, or other legitimate ways of approach, as long as the respective schools do not claim the exclusive possession of the keys to the road to salvation.

In this context it may not be without interest to say a few words about continental feeling with regard to British Social Anthropology, not in order to criticize the latter, but to use it as a foil which may help us to clarify the continental attitude. Essentially, this feeling consists in a mixture of deep and very sincere admiration for the magnificent field-work and the theoretical achievements of British social anthropologists and of an equally sincere regret concerning their self-imposed restrictions, their rejection of history—now fortunately on the wane—and the dogmatism of a few of their more orthodox members. If a continental ethnologist would venture to dissect culture and to regard social organization as a completely separate and independent entity, if he would by principle close his eyes to history, or if he would go so far as to speak contemptuously of ethnology as—I quote—"an old-fashioned and spurious science of man," this would certainly be considered as an almost unthinkable heresy. To be sure, on the continent, too, there exists specialization. There are those who devote their researches to society, or economy, or religion, or technology. But whether they explicitly say so or not, in principle—and this is the point that matters—they consider their results as contributions toward an over-all concept of culture in all its aspects and ramifications. I may add that in general continental ethnologists will not admit that the British social anthropologists' methods are incompatible with their own predominantly historical approach. They feel rather that the two methods are complementary to one another. While they hail the growing awareness of British anthropologists that in the long run they cannot do without history, they feel that they themselves have much to learn from their British colleagues. Today, the two schools do not seem to stand as far apart as was the case a few years ago. Some kind of eventual integration appears not quite inconceivable.

One Central European school of ethnological thought is of a sufficiently distinct character to deserve special mentioning. It is the one based on the concept of "*Kulturmorphologie*," a term as difficult to translate as that of "*Kulturkreis*." I shall not attempt to define it, not only because this would take too much time, but also because, if I see correctly, its meaning seems to have become somewhat fluid and to have changed considerably since the days of Frobenius and Spengler who first applied it. The most prominent representative of the school in question is Professor Jensen in Frankfort. He stresses the irrational sources of culture and the primacy of mythological and religious concepts and rituals. His ideas have been compared to those of Ruth Benedict. There is indeed some kind of slight common denominator, although Jensen was certainly not influenced by Benedict's *Patterns of Culture*. Moreover, being deeply imbued with a sense for history, he is not content with defining the values concerned, but attempts to penetrate to their roots, to show how they originated and developed, and to trace their world-wide interrelations. Even those who, like myself, are not prepared to accept all of his conclusions will have to concede that his approach is a valuable complement to historical ethnology and that in certain respects it allows us a deeper insight into the true character of primitive peoples and cultures than mere sociological description or functionalist analysis. Yet one cannot help feeling reminded of that collection of stimulating essays on African cosmologies by French, Belgian, British, and African scholars which Daryll Forde edited two years ago under the title of *African Worlds*. The accent, it is true, is different. But here, too, the interrelation of social and political organization with cosmological and religious ideas is stressed. The differences between the various schools of ethnology are perhaps, after all, not as fundamental as might appear at first sight.

The historical orientation of most continental European ethnologists implies a strong interest in the dynamics of culture change. The necessity of studies on the acculturation of primitive tribes to western civilization is, of course, recognized, but from the purely scientific point of view less importance is attached to them than, for instance, in the United States. The great disparity of the respective cultures and the enormously powerful impact of western technology and political organization mark that particular kind of acculturation as a unique and abnormal case of culture contact, the results of which can hardly be used for general conclusions. It is felt that it is rather the study of the acculturation of primitive tribes to one another or to archaic or oriental civilizations which can provide us with a stock, certainly not of fixed laws, but of empirical rules and probabilities that may help us to understand similar processes in the past.

Although Father Schmidt was probably the first scholar to emphasize the importance of the study of the individual in primitive society, and although Koppers at an early date contributed a few sketches of the personal character of individual Fuegians, the subject has been sadly neglected in continental Europe, and there is nothing comparable to American research on personality and culture. In general, surprisingly little attention has been paid in Europe in recent years to psychological research among primitive peoples. As far as the German speaking countries are concerned this may in part be due to a reaction against Bastian's and his followers' rather naive psychological fads and against the oppressive bulk of Wundt's ten volume ethnic psychology. However, I wish to avail myself of this opportunity in order to bring to your attention a very brief, but very important paper recently published in the Bulletin de l'École Française d'Extrême-Orient, a paper which in my opinion gives us more really relevant psychological insight than we could expect from a hundred Rorschach tests. Its author, George Condominas, analyzes and compares two long epic poems of the Radé, a pagan tribe in the interior of Indo-China. One of these poems gives us the picture of a solidly established matrilineal society, controlled by supernatural forces. Its hero is dominated by women, particularly by his tyrannical sisters. The whole poem emphasizes the importance of strictly submitting to the social laws, above all to those governing marriage, if one wishes to avoid disaster. On the contrary, the hero of the second poem is constantly in revolt against these laws, relies solely on his own force and cunning, disregards even the warning omens sent by the gods, and yet always triumphs. These opposite tendencies within one and the same tribal culture will perhaps remind you of the comparable ones which Malinowski observed among the Trobriand Islanders. I have little doubt that, if followed up, the line of research so happily initiated by Condominas, the study of oral literature from the psychological, sociological, and historical points of view, may yield most valuable results.

Less than a year ago an American anthropologist, in the course of a seminar in this country, expressed his concern about the tendency to transform ethnology into what he called a "super-science" by including in it all kinds of alien subjects, such as community studies etc. It is obvious that this tendency, if it continues long enough, must finally lead to the disintegration of ethnology. The danger exists not only in America, where it seems to have originated, but also in Europe, and in passing through Paris recently I heard similar complaints. However, the threat is perhaps not as great as might appear at first sight. In reading the paper on the Netherlands in the *Yearbook of Anthropology*, for instance, you will find that a lot of space is devoted to all kinds of marginal subjects and activities which have hardly the remotest connection with anthropology, while ethnological research is very step-motherly put off with a few insignificant lines. One could almost get the impression that in Holland ethnology is on the verge of expiring. Yet, if we look at the things as they really are, we shall see that in Holland orthodox ethnological work is on the upsurge, rather than on the decline. Maybe we should not take the spectacular parading of fashionable tendencies too seriously. Anthropologists are sometimes like children who enjoy displaying their new toys before visitors, but if left alone prefer to play their old games.

Allow me to conclude with a few words *pro domo*. If, today, one can speak of a Vienna School of Ethnology, it is only in the sense that we are perhaps more consistently than others trying to perfect the methods of historical ethnology and to carry its principles through to their logical goal. However, we are also keeping our minds open to other ways of approach than that of history, and we are happy that we were able to send some of our best students to London in order to acquaint themselves with the methods of British social anthropologists. Nor do we in the least wish to sit in an ivory tower. I may mention the fact that some of our historically trained graduates have held UNESCO positions in applied anthropology and have done very well. I need not go further into details, since my colleague, Professor Haekel, has just published an excellent interpretation of our views and methods in the volume commemorating the 25th anniversary of our Institute of Ethnology.

I am painfully aware of the inadequacy of this very fragmentary report on ethnological theory in Europe. I regret particularly that lack of time has prevented me from speaking about the current vigorous development of ethnology in France which constitutes a great hope for the future of our science. But the great diversity of French ethnological interests, as compared to the relative uniformity in the rest of continental Europe, would in itself have required a separate treatment.

Vienna, Austria.

RECENT DEVELOPMENTS AND TRENDS IN ETHNOLOGICAL STUDIES IN CHINA

Huang Wen-Shan and Ho Lien-Kwei

Ethnology, both in its historical and its cultural aspects, had an early development in China, dating as far back as over 3,000 years. However, its study falls mainly within the scope of history. Hence there was no systematic and scientific set-up in this particular field of research. It was not until Western ethnology or cultural anthropology transfused into China that a modern school in this line was conceived and born in a new environment. Briefly speaking, the developmental study of anthropological and ethnological sciences in China during the last forty years or so may be viewed in the following stages.

I. THE EMERGING STAGE

At the end of the nineteenth century the chequered works of Bastian, Morgan, Tylor, and others had much influence on the Chinese intellectual circles. In response to this new current of thought, in 1903 the Manchu regime added anthropology and ethnology as courses of study in its college curriculum. Yet, it was not until after the establishment of the Chinese Republic in 1912 that these two subjects were actually included in China's educational system. Anthropological courses were given in the National Peking University in 1917, when Dr. Tsai Yuan-Pei, an eminent Chinese educator and scholar, became the Chancellor of the University. As an ethnologist himself, he taught aesthetics in the light of cultural theories. At the same time he advocated a theory that aesthetics may serve as a substitute for religious worship. In view of his position and prestige his theory had a great deal of influence upon the youths during the years of the "New Cultural Movement" in China.

II. THE STAGE OF GROWTH

The progress of ethnological study, during the last forty years, may be judged from the following developments:

A. With the establishment of the Academia Sinica in Nanking in the year 1927, Dr. Tsai Yuan-Pei devoted a section of the Institute of Social Sciences to the study of ethnology. While he himself headed this section, his followers undertook field researches in the various areas in China. Such tribesmen or minorities as the Yao in Kwangsi Province, the High Mountain tribes in Taiwan (Formosa), the Goldi in Manchuria, the Miao in Hunan, and the Shemin in Chekiang were for the first time scientifically surveyed by the staff members of the said Institute.

In its enthusiastic moments the Institute had also subsidized H. Stibel, a German professor at the Tungchi Medical University in Shanghai, and Liu Hsien of the Shangtung University, for the purpose of surveying in 1933, the Li tribesmen in Hainan Island. During the years 1935 to 1937 Ling Shunsheng, Tao Yun-Kwei and Ruey Yih-Fu were despatched to Yunnan Province to enlist their combined efforts to the study of many an aboriginal tribe, such as the Yao, the Miao, the Lolo, the Mosa, the Pay-yi (Shan), the Laho, the Lisu, the Kachin, the Wa, etc. In the thick of such activities, F. Jaeger, another German scholar, arrived upon the scene to participate in the field work. Reports and monographs on their studies have been published.

B. During the era of the Chinese intellectual renaissance or the period between 1917 to 1930 generally known as the "New Cultural Movement," the values of social and cultural sciences became acceptable all over the country. The prospect had a very promising look, as anthropological and ethnological courses were given in the National Sun-Yat-Sen University in Canton and the National Central University in Nanking. Famous anthropologists such as S. M. Shirokogoroff of Russia, Radcliffe-Brown of England, and Father W. Schmidt of Austria were invited to deliver lectures in the various universities in Peiping and Nanking during the years 1930 to 1935. An anthropological journal known as *The Ethnological Research*, edited by Huang Wen-Shan was published under the auspices of the Sun Yat Sen Institute for the Advancement of Education and Culture.

For the promotion and propagation of anthropological and ethnological knowledge and study, the creation of the Chinese Ethnological Association in Nanking in the year 1934 with an initial membership of forty was a remarkable phenomenon. As evidenced by the publications and works of the members of the Association during the last three decades, the development of ethnology has no doubt contributed a great deal to the understanding and interpreting of the Chinese people and their culture to the world.

During the second world war almost all research institutes and universities were evacuated and removed either to the southwest provinces or to the northwestern frontiers. Taking this extraordinary opportunity, the Chinese world suddenly became animated by scientific surveys on the Miao, the Lolo, and the Yao, with the result that we have quite a few monographs and a great number of articles on the study of their customs, languages and other ethnic characteristics. Some of the scholars penetrated far into Sikang, Tibet, and Kansu, while the others remained in Szechuan, Kweichow and Kwangsi to pursue their researches. The publication of the reports, on the whole, aroused a keen interest among the Chinese nation.

III. THE RESURGENT STAGE

By the end of 1949, with the emergence of the Chinese Communists on the mainland, anthropologists and ethnologists including Li Chi, Ling Shun-Sheng, Wei Hui-Lin, Ho Lien-Kwei and many others left the mainland of China to live in Taiwan. During the last few years, researchers of the Academia Sinica, the National Taiwan University, and the Provincial Archives Committee set forth to make a study of the aboriginal tribes in the Taichung mountainous regions. Reports on the Atayal, Tsou and Bunun were published. Among these, the works of Ho Lien-Kwei, especially "The Totem Culture and Remanint Totemism in the Mountainous Regions in Central Taiwan" together with "The Folklore and Customs of the Taiwan Native People," are no publications to be passed over lightly. Beside the so-called "Kao-shan" or "High Mountain" tribes, there are still other tribes scattered throughout the island, consisting of nine branches, namely, Atayal, Saisiat, Tsou, Bunun, Rukai, Paiwan, Puyma, Ami and Yami, who have received their due share of ethnological study from ethnologists such as Chen Chi-Lu, Chen Shou-Hsin, etc.

SUMMARY

In theory and methodology, Chinese anthropologists and ethnologists followed the scholars of the West, and they could be described as the followers of the Historical, Evolutional, Neo-Evolutional and other schools. Yet the notable contribution and achievement of these ethnologists over the past three decades have entirely been ignored by the West. In the present summary, however, the following points might be mentioned.

A. Ling Shun-Sheng has since 1949 held that the native tribes in Taiwan have preserved many characteristics of the ancient culture prevalent in southeast Asia. Basing his ethnological study on the mainland, he concluded that the so-called "barbarian tribes" known as "Wu" and "Viet" who had settled along the East China coast, in ancient times together with the "Po Liao" or "Pai-Po" in southwest China, were of the same stock as the Indonesian or so-called "Proto-Malay" now prevailing in southeast Asia. He thus treated this region as one culture area. In his treatise, "An Introduction to the Research of Ancient Culture in Southeast Asia," he divided this region into three subareas namely "continental" "peninsular" and "island." They correspond to the culture strata as labelled by distinction, "Sino-Tibetans," "Indonesian-Malay," and "Melanesian-Negrito." Hence, according to this theory, the so-called Culture of Central China is considered as a blend of the "Oriental Oceanic Culture" with the "Occidental Continental Culture," while the ancient civilization of southeast Asia represents the "Oceanic Culture" which formed the basic stratum of the Chinese culture. The new hypothesis may be a key to further studies which are likely to make promising contributions to this field.

B. Fifty years of Japanese occupation of Taiwan resulted in the development of a scientific classification of the native people by Japanese scholars. However, by emphasizing merely ethnological study, there still was a lack of generalized cultural theories or principles in the analysis or comparison of the cultural aspect of the people. The recent arrival of Wei Hui-Lin, who has devoted a period of five years to this line, brought about some remarkable contributions. Some of his conclusions are:

(a) With relation to the clan system, he found that all the five clannish tribes have been mainly emphasizing communal activities, with the exception of some matrilineal, clannish societies which had changed into the nominal pattern with more or less totemic traits. For instance, communal functions such as participating and hunting, funeral ceremonies, and property distribution or inheritance are still executed according to clan units. On the clan theory he proved that both the patrilineal and matrilineal systems, which have different origins, had parallel developments without anterior or posterior stages one over the other.

(b) With regard to the age grade system, he discovered that there are two basic patterns prevailing in the native societies. They are the Terminal System and Nominal System. The latter, however, develops more fully only in the matrilineal Ami society co-existing with the military organization of the men's house.

(c) In respect to the tribal organization and its chieftainship, he discovered the existence of four patterns called "pan-blood society," "clan society," "two-class society," and "dual structural society," still in vogue among different tribes on the island. The dual-leadership system has been necessary so as to keep a balance of power in harmony with natural supremacy and social superiority.

C. Ruey Yih-Fu made new generalizations on the kinship system of the Miao tribe on the mainland and of the aborigines in Taiwan. First, he compared the Miao system with that of the Chinese proper, and made a further comparative study of it with that of the native tribes on the afore-mentioned island. The terminological identity of parents with their children is interpreted by him as due to two important formative factors: "Teknonymy" and "Tekeinonymy" or "Reverse Teknonymy." From the comparative study of both ancient and modern Chinese kinship terminologies, he further discovered that the former corresponds to what Morgan termed the "Turanian Type" of the "Classification System," or the so-called D type of Kirchhoff, or bifurcate merging terminology of Lowie, while the latter corresponds to the "Descriptive System" or A type, or bifurcate-collateral terminology. Ruey also distinguished the ancient kinship system which refers to the exagamous clan organization, and the modern system which is connected most probably with "Grossfamilie."

D. Huang Wen-Shan, who had studied anthropology under both Boas and Kroeber in the U.S.A., had published several important essays in this line of research since 1934, two main themes of which were most outstanding.

First, on the study of totem culture, which he considers as a culture system rather than as a religious or social system, he adopts ethnological methodology to explain the prehistoric totemism in China. In this research he came well nigh to the philosopher's stone which enabled him to solve the puzzles of an eminent scholar like Ku Che-Kiang. His successful study of some legendary and mythological history in the ancient history of China is known to all. Subsequently, anthropologists and sinologists, including the distinguished Ho Lien-Kwei, made diligent and penetrating researches into the totem system of different tribes as well as in the ancient history of China. While some noted anthropologists, such as Morgan, Tylor, Malinowski, and Boas, did not believe in the common existence of totem culture in China, their theses as such were then disproved by the general conclusion of these ethnologists who held that totem culture, though appearing in different forms and varying in different degrees in different cultural stages, did in fact exist in every corner of the globe. Today, totemism has become a subject of greater magnitude in the study of ethnology and ethnohistory.

Second, Huang also promoted the adoption of the so-called "Culturology" or the Science of Culture as an independent discipline to include the study of cultural phenomena by the use of the cultural-historical method. Since 1934 he has published several books in this field. In 1939 he issued also a book entitled "Collected Papers on Culturology," which was followed by another book, "Culturology and Its Place in the Domain of Social Science." It is remarkable that Leslie A. White, a most brilliant American anthropologist of the Neo-Evolutionist School, holds almost the same position in this respect, despite their differences in philosophical assumptions.

On the whole, ethnology is not a mere theoretical science, but also an applied science. And the study of this subject has its academic value as well as its practical utility. There is a significant trend in this field to apply the results of such a study to facilitate the solving of various concrete problems concerning the Chinese minorities and the overseas Chinese.

Finally, the support of ethnological research by the Chinese government and the various institutes has greatly extended the influence of this science and stimulated all sorts of intellectual activity. In the course of its thirty years of existence, the Chinese Ethnological Association for the Promotion of Ethnological Knowledge is now more than ever living up to its name and purpose.

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CURRENT TRENDS IN ETHNOGRAPHY IN THE U.S.S.R.

I. I. Potekhin

In my short communication I would like to acquaint the members of the Congress with the state of ethnographical science in the U.S.S.R., and with the main trends of ethnographical investigations. I am very grateful to the Programme Committee, and particularly to its chairman Mr. Herskovits for the opportunity they afforded me. Due to some reasons, relations with our foreign colleagues in the last decades have become less intensive, and hence their information on ethnographical science in the U.S.S.R. was insufficient, which promoted the dissemination of a number of wrong notions. I hope that my information will contribute to that mutual understanding which is so necessary for scientific collaboration.

Ethnographical science in the U.S.S.R. is not essentially different from the ethnographical science of any other country as far as its tasks and its problems are concerned. As in any other country, the ethnographers of the U.S.S.R. are engaged in studying the culture and the mode of life of the peoples, their origin, their dispersion and present habitat, their cultural-historical bonds and relations.

However, it is useless to make a secret of the fact that in the questions of theory we have some disagreements with some of our colleagues from other countries. This disagreement sometimes gives rise to a very acute struggle of opinions, which however should not serve as an obstacle for scientific collaboration. On the contrary, the struggle of opinions is an obligatory condition for the development of scientific thought.

The main object of ethnography always was, and remains now, the contemporary peoples and the phenomena of popular life immediately observed by an investigator. Therefore, in our country, as in all other countries, the central place in the work of the ethnographical institutions is occupied by investigations of the contemporary culture and the mode of life of the peoples.

The Soviet Union, as is well known, is a multinational country. Each people has its own characteristic features, its own culture and mode of life. The profound social changes that took place in our country have not eliminated the national specificity of the peoples. Socialist culture, being unitary in content, is developing in very peculiar forms, inherent to each separate people.

In connection with this, a great and responsible task confronts the ethnographers: to investigate how the socialist transformations, unitary in their nature, are embodied in the various national forms, how old forms are filled with new content, how these old forms are developed, adapting themselves to the new content. This is a very profound and complicated process, full of acute contradictions, conflicts, and intense struggle between the new and the old. Besides its scientific significance, the study of this process is also of great practical importance.

Such kind of investigations began immediately after the establishment of Soviet power. They were of great practical use in the creation of our multinational state, and in the elaboration of concrete measures directed towards bringing into life the national policy of the Soviet government.

It is known that at the moment of establishment of Soviet power the peoples of our country were at different stages of development. The peoples of the extreme north, for example, were at the stage of decline of primitive communal society and gradual transformation into a class society. Amongst the peoples of Middle Asia feudal relations were predominant, but they were in a very complicated intercombination with patriarchal clan relations. Survivals of the primitive communal society and feudalism were more or less strong among many other peoples of our country. Therefore, the attention of the ethnographers was, naturally, concentrated on the study of these survivals. They had to give an answer to the question of how to integrate the backward peoples in the common process of socialist construction, what the attitude of the Soviet government should be to survivals of the primitive communal society, etc.

In connection with the victory of socialism the field of activity of ethnographers has changed. Socialism became a mode of life; the new was victorious, the old was left behind. The ethnographers accordingly had to reconstruct their work: instead of investigations of the old way of life, which had already disappeared, they had to study the new way of life, the socialist mode of life.

This does not mean, of course, that ethnographers ceased their study of survivals. These are still alive in the new society, and this new cannot be understood without a study of the survivals. The survivals should also be studied for better understanding of the past. However, attention is now mostly concentrated on the study of the new mode of life.

The reconstruction of ethnographical research, which began on the eve of the second world war and was carried on intensively during the last decade, was conducted in an atmosphere of acute struggle of opinions. The disputes developed in two main directions. First, it was disputed whether ethnographers should study the socialist mode of life of the peoples and whether they are able to do it. A certain group of ethnographers and archaeologists defended the standpoint that ethnography is a science of the primitive-communal society and of its survivals in contemporary life, that ethnographers, therefore, cannot and should not study the contemporary socialist society. Other ethnographers considered that ethnography should study the culture and the mode of life of peoples at all the stages of their historical development from ancient times to our days. The second standpoint gained the victory.

Another direction of the disputes which is less in principle was how to study the socialist mode of life of the peoples, how to delimit the field of ethnographic investigations from those of economists, historians, and representatives of the other humanities which also deal with studies of contemporary life, and how to conserve in this case the specific character of ethnographic science.

This question is still being disputed. And this is quite natural. Ethnographical study of a socialist society is a completely new thing, just as new as socialist society itself. Until recent times ethnographers had no experience in such investigations. The old programmes of collection of field material were of no use; it was necessary to create new programmes. And in conformity with the new programmes it was also necessary to change the methods of collecting of field material and to reorganize field work. But the most difficult task was the generalization of new field materials, the elaboration and formulation of principles in the development of ethnographic phenomena under the new conditions, under the conditions of socialism.

At the present time the study of socialist culture and the new mode of life of the peoples of the U.S.S.R. occupies the main place in the work of all the ethnographic institutions in our country.

The enlargement of the field of ethnographic research, the inclusion of new topics in the programme of ethnographical works, roused a significant revival of interest in this branch of science.

In recent years many ethnographic institutions were created in those Union and Autonomous Republics where ethnography was never studied before. It is a very gratifying circumstance that the number of young scientist-ethnographers among previously backward peoples (Khakasses, Yakuts, Altaians, Chuvashes and many others) increased. Their ethnographical studies of their own peoples are great contributions to science.

The field works embrace now almost all the peoples of the U.S.S.R. The expeditions sent only by the Ethnographical Institute of the Academy of Sciences of the U.S.S.R. work in Russia, in Middle Asia, the Baltic Republics, Siberia, the Volga River area, and the North Caucasus. Materials on such questions as the family and family relations, housing, clothes, food, applied art, spiritual culture, etc., are collected on a large scale. Many facts already accumulated by the expeditions show how complicated are the processes taking place in the life of the peoples.

The efforts of ethnographers were until now concentrated mainly on the study of peasant life. This is a more usual sphere for ethnographers. But we think it impossible to limit ourselves only to the study of the peasantry. Due to the policy of industrialization there took place a great transfer of population from country to towns and industrial centres. Urban population now constitutes 43 per cent of the whole population of the country. In this connection the ethnographical study of the working class is of great scientific interest. But here the Soviet ethnographers are still only at the stage of first experiment. In the near future this branch of ethnographical science should take its proper place.

Ethnography is a historical science. The ethnographical phenomena of our days cannot be understood without a study of the history of their origin and development. Therefore our ethnographers, naturally, do not limit themselves only to a study of contemporary life. The whole history of a given people from the moment of its appearance on the ethnographic map to our days is of interest for ethnographers. Therefore historical ethnography occupies an important place in the work of Soviet ethnographers. In recent years several monographs which contain the history of their culture and modes of life in different historical periods were published on some peoples. Together with historians, ethnographers elaborate general text-books on the history of various peoples of the U.S.S.R.

An important place in the work of our ethnographic institutions is occupied by studies on ethnogenesis, the origin of peoples. In elaboration of these difficult problems the collaboration of scientists of different specialities, ethnographers, anthropologists, archaeologists and linguists, is practised on a wide scale. Special complex expeditions are organized, such as, for example, the Kirghiz complex expedition which worked under the guidance of Professor Debetz, who is present here; it has already finished its work and the results of its investigations will be published in three volumes in the near future.

The main efforts of Soviet ethnographers are, naturally, directed to the study of ethnography of the peoples of our country. Considerably less attention is devoted to ethnography of the peoples of foreign countries. Only in recent years has there taken place a certain revival in this branch of Soviet ethnographic science. This revival was caused by the preparation for publication of a series of ethnographic essays under the general title of "The Peoples of the World." This will be a large fundamental work consisting of fifteen volumes.

The task of this publication is to draw up a single ethnographic picture of the globe, to show the mode of life of all the peoples, and, if possible, of each people, and its contribution to the general depository of world culture. For the time being only one volume, "The Peoples of Africa," has appeared. This year three following volumes, "The Peoples of Siberia," "The Peoples of Australia and Oceania," and "The Peoples of the Near East" will be published. The volumes to follow are "The Peoples of the Caucasus" and "The Peoples of America" and others.

The preparation of the volumes on the "The Peoples of the World," devoted to the peoples of foreign underdeveloped countries, forced us to make a profound study of a number of problems. I shall dwell in brief only on two problems which aroused the greatest interest.

The first problem is the peculiarities of the development of the primitive communal, clan relations among backward peoples, involved, due to colonization, in the system of world capitalist economy. The examination of materials concerning the peoples of Africa has shown that in pure form they do not now exist, having been replaced by others, by new relations, and that the old clan tribal form of organization of a society which still exists does not correspond to these new social relations. Moreover, the artificial conservation of this form became an obstacle in the way of progress.

The second problem is the peculiarities of the formation of a national communities among these peoples. The advanced peoples of the world long ago passed this stage of development when, as the result of mixing and merging of tribes, peoples—nations—were formed. Scientists now try to restore this picture of the formation of nations, using only literature and other historical data. In many underdeveloped countries this process is still going on. We are the witnesses of how tribal structure is decaying, how tribal differences disappear, and how tribal languages yield to national languages.

Our ethnographers have done considerable work in this field and came to interesting conclusions. We would like this problem to be studied by a wider circle of scientists from other countries.

Because of lack of time I shall not dwell on the problems relating to the history of primitive communal society. They occupy a proper place in the work of the ethnographers of our country. Our delegation will distribute among the participants of the Congress a special report on this subject.

As is seen from my brief communication, the circle of interests of Soviet ethnography is very wide; being a special branch of historical science, it studies the development of ethnographic phenomena, beginning from the formation of human society to our days. Concentrating its main efforts on studying the peoples of the U.S.S.R., it also shows a definite interest in the peoples of all other countries. Developing within the general framework of world ethnographic science, it, however, has its own specific tasks.

The presence in the contemporary world of two social-economic systems gives rise to differences in the scope of tasks which confront the enthographers of the countries belonging to this or that system. In the countries where a new, socialist society is being built, the ethnographers are confronted with tasks which ethnography had not dealt with before; these are the tasks of studying this new society. New tasks, as always, involve new difficulties, the search for new ways for their solution, and a struggle of opinions.

Ethnographic study of a socialist society is a new stage in the development of ethnographical science. But this study is not separated by any abyss from its past. On the contrary, a successful development of ethnographic study of a socialist society is possible only on the basis of the utilization of all the best which is available in world ethnographic science—of its best achievements and traditions.

Moscow, U.S.S.R.

RECENT DEVELOPMENTS IN AMERICAN ARCHEOLOGY

Irving Rouse

Interest in method and theory has been increasing among American archeologists. This trend culminated during the summer of 1955 in a series of four seminars on theory which were sponsored by the Society for American Archaeology under a grant from the Carnegie Corporation of New York. The first seminar, held at Harvard University, made a study of archeological instances of contact between cultures. The second, at the University of Michigan, discussed cultural change and persistence, as seen in the archeological record. The topic of the third seminar, at Santa Fe, was "The Prehistoric Southwest: a Problem in Cultural Isolation"; and of the fourth, at Washington, D.C., "Changing Settlement Patterns in American Cultural Evolution." The results of these seminars are being published as a *Memoir* of the Society for American Archaeology (Wauchope, 1956).

American archeologists continue to explore methods of interpreting cultural remains in a functional manner. Perhaps the most fruitful recent innovation in this field has been the study of settlement patterns as a means of drawing inferences about social organization and other non-material aspects of culture, an approach which was pioneered by Willey (1953) in a study of Viru Valley archeology, Peru. In 1955 the American Anthropological Association sponsored a symposium on settlement patterns in American archeology, which is being published by the Wenner-Gren Foundation (Willey, 1956).

A second aspect of method which continues to interest American archeologists is the definition of concepts. Articles have been published during the past few years on the nature of artifact types; of cultures or phases, as some American archeologists prefer to call them; and of traditions and horizons, which link together the phases. Since these matters are being touched upon in another session of the Congress, I shall say no more about them here, except to call to your attention a pair of recent articles in the *American Anthropologist* by Phillips and Willey (1953, 1955), in which they are summarized.

Phillips and Willey's second article also illustrates a third methodological problem which currently occupies the attention of American archeologists: developmental classification, or the grouping of cultures in a series of evolutionary stages. Originally applied by Steward and others to the areas of Indian civilizations in Mexico and Peru (Bennett, 1948), this approach has been expanded by Phillips and Willey to cover the entire New World. They postulate six evolutionary stages: (1) Early Lithic or (as some authors prefer to call it) Paleo-Indian, which corresponds to the Paleolithic in the Old World; (2) Archaic, which is roughly equivalent to the Old World Mesolithic; (3) Pre-Formative, a transitional stage, and (4) Formative, which are more or less comparable to the Old World Neolithic; and (5) Classic and (6) Post-Classic, which take the place of the Chalcolithic, Bronze, and Iron stages of the Old World. These last reflect the fact that the New World civilizations reached a climax in the Classic stage, during the latter part of the first millennium A.D., and subsequently underwent a series of modifications which produced the Post-Classic.

Phillips and Willey themselves, as well as other commentators, have pointed out weaknesses in this scheme of classification (e.g., McKern, 1956). I will not go into them here except to say that I see a parallel between the Phillips-Willey classifications and Childe's reformulation of the Paleolithic-to-Iron classification in the Old World (Childe, 1942). Just as the latter appears to work best in the Near Eastern center of civilization and to break down as one moves out of this center into Europe, Africa, and Asia, so the Phillips-Willey scheme (except, perhaps, in its earliest stages) seems to work best in the centers of New World civilization in Mexico and Peru and to break down as one moves outwards from those centers into the rest of North and South America.

Turning from method and theory to the results of archeological research in the Americas, I shall attempt to outline the prehistory of the American Indian as it is now known, and at the same time to indicate the most important recent discoveries bearing upon that prehistory. I shall begin with the Paleo-Indians, or first settlers of the New World.

American archeologists continue to push back their estimates of the time when the Paleo-Indians reached this hemisphere. Until recently, we would have considered 12,000 to 15,000 years ago to be the maximum, but we now have several radiocarbon dates between 20,000 and 25,000 (Johnson, 1956, Fig. 10) and two others, which seem difficult to believe, of more than 37,000 years ago (personal communication from Alex D. Krieger). These last take us back past the beginning of the final (Wisconsin) glacial advance, although not necessarily into the preceding Sangamon interglacial. Few American archeologists are willing to accept the claim of the geographer, George F. Carter (1952), to have found remains of man near San Diego dating from the Sangamon.

It is still generally agreed that the Paleo-Indian originated in Asia and entered the New World by way of the Bering Straits and Alaska. Of the nature of the culture which he brought in over this route, we know very little. We can say, however, that it was not Folsom, as some authors have supposed. Evidence has accumulated that the classic Folsom point developed relatively late in a rather restricted area on the central U.S. plains. Some archeologists have suggested that the original Paleo-Indian points may have been leaf-shaped, like the Sandia type; others, that they were made of bone rather than stone.

Once the Paleo-Indian became settled in the United States, he appears to have developed at least two major types of culture, one east of the Rocky Mountains and the other to the west of the Rockies. The sequence east of the Rockies is the classic one, beginning with the Clovis fluted and/or Sandia types of projectile points and extending through Folsom fluted to various lanceolate types, which used to be grouped together under the name of Yuma (e.g., Sellards, 1952, pp. 17–75). West of the Rockies, the picture is still somewhat confused, but there appears to have been a different sequence, which emphasized gathering instead of hunting as the principal means of subsistence (cf. the paper delivered by Ruth D. Simpson at this Congress).

Further evidence has accumulated concerning the spread of the Paleo-Indian southward from the United States into Latin America. At the site of Santa Isabel Iztapan in the Valley of Mexico, Aveleyra Arroyo de Anda (1955) and his associates have found projectile points and other artifacts in direct association with two mammoth skeletons; while at El Jobo in Venezuela José M. Cruxent (1956) has discovered somewhat similar artifacts in a series of surface deposits. The El Jobo projectile points, in turn, resemble a series of leaf-shaped points from central Argentina (Rex González, 1952, Pl. XIII), for which a radiocarbon date of 6014 ± 100 B.C. has been obtained (personal communication from E. S. Deevey, Jr.).

While cultural remains of the Paleo-Indian are now well known, we have had less success in finding skeletal remains, i.e., the bones of the Paleo-Indian himself. An important step towards the solution of this problem was made in 1954 with the discovery of a human skeleton at Midland, Texas, in apparent association with Clovis points and preceding the Folsom type (Wendorf, Krieger, and Albritton, 1955). The Midland skeleton confirms the theory that the Paleo-Indian was long-headed, like the Paleo-Mongoloid.

Leaving the Paleo-Indian period, I shall now turn to the time when agriculture and pottery made their appearance in the New World, i.e., to the period which some American archeologists have called Neo-Indian. While certain Old World archeologists maintain that Neo-Indian culture is the result of one or more migrations from Asia by way of the Pacific Islands, I believe it is safe to say that most of us in the New World still look upon Neo-Indian culture as a purely indigenous development, although some of us would not rule out the possibility of secondary influences across the Pacific and most of us are inclined to accept the probability of some diffusion through the Circum-Boreal zone (e.g., Smith, 1953).

For some time, opinion has differed as to the place where the basic inventions took place which touched off the Neo-Indian development—notably the domestication of maize and the invention of pottery. Some authorities have ascribed these inventions to the area in which Meso-American civilization subsequently developed, i.e., to Mexico and Guatemala; while others have assumed that maize was first domesticated and pottery invented in the area of the subsequent Central Andean civilizations, i.e., in Peru and Bolivia. Recent discoveries tend to support the first of these hypotheses. For example, the earliest direct evidence we now have of the cultivation of maize comes from North America: radiocarbon dates of 3651 ± 290 B.C. for Bat Cave in New Mexico and 2491 ± 100 B.C. for La Perra Cave in Tamaulipas, Mexico (Mangelsdorf, 1954). During 1954, Clifford Evans and Betty Meggers (personal communication) discovered pottery on the coast of Ecuador which is surprisingly close to that of Playa de los Muertos in Honduras, and they suggest that it indicates the diffusion of pottery-making (if not also of maize agriculture) from north to south. Reichel-Dolmatoff (1956) similarly views the early part of his ceramic sequence for the Caribbean coast of Colombia as a link between the Formative cultures of Middle America and the Central Andes; e.g., rocker-stamped pottery occurs early in all three areas, as though marking a common sub-stratum out of which civilization developed in both Middle America and the Central Andes (cf. Robert F. Greengo's paper on rocker-stamped pottery at this Congress; also Willey, 1955).

Within Middle America, archeologists continue to concentrate on the later stages in the development of Meso-American civilization. As a result, we know very little about what Phillips and Willey (1955, pp. 765–6) would call the Archaic and Pre-Formative cultures of those areas; our detailed knowledge begins only with the Formative stage, about 1500 B.C. There is a parallel here with the situation in the Near East before the recent work of Braidwood and others; we are faced with a gap between the Paleo-Indian and Neo-Indian cultures, corresponding to the gap between the Paleolithic and Neolithic cultures of the Near East. We shall not be able to demonstrate conclusively the origin of Neo-Indian culture in the New World until this gap has been closed.

The principal Classic civilizations of Middle America are those of Teotihuacan, in the highlands of central Mexico, and of the "Old Empire" Maya, in the eastern lowlands of Guatemala and adjacent countries. These are succeeded by Tula-Toltec and "New Empire" Maya as the principal post-Classic civilizations of the highlands and the lowlands respectively. While identification of the Tula site, rather than Teotihuacan, as the Toltec capital took place some time ago, it is perhaps worth noting here, since some summaries in English still overlook it (e.g., Howells, 1954, pp. 310–14).

From Middle America, it is presumed that agriculture, pottery, and the other elements of Neo-Indian culture spread northward into the United States along two main routes: (1) via the highlands and adjacent coast of western Mexico into the American Southwest and (2) via the east coast of Mexico into the American Southeast. J. Charles Kelley has reported at this Congress on the progress recently made in the study of the western route; and so I need only say here that the basic inventions seem to have originally spread northward along the highlands east of the coastal range. This wave of diffusion culminated in the Hohokam culture of southern Arizona, and it appears to have been followed by a second wave along the coast of the Gulf of California which influenced primarily the Pueblo peoples of northern Arizona and New Mexico.

Within the Southwest, archeologists are still concerned with the question whether the two cultures I have just mentioned, Hohokam and Pueblo or Anasazi, constitute the only major traditions in the area or whether there is a third of equal importance, the Mogollon in the mountains of southeastern Arizona and southwestern New Mexico. (Parties from Harvard and Columbia Universities, the University of Arizona, and the Chicago Natural History Museum have all been recently working in or near this area.) The non-agricultural peoples, such as the Utes and Navaho, continue to receive relatively less attention from Southwestern archeologists than these agricultural groups (e.g., Reed, 1955).

Moving north and west from the Southwest into the Great Basin, the Pacific Coast states of California, Oregon, and Washington, and the province of British Columbia, we gradually leave the area of Southwestern culture and begin to find only the remains of non-ceramic and non-agricultural people on an Archaic level of cultural development. In other words, Neo-Indian influences from the Meso-American center die out, and the native inhabitants retain essentially the same sort of culture which they had at the close of the Paleo-Indian period, with gathering as their principal means of subsistence. Great progress has been made during the past four years in uncovering the remains in this area, largely through the work of the various state and provincial universities (e.g., Beardsley, 1948), but we are still no closer to solving the basic problem of why the Indians here did not adopt agriculture, pottery, and the other elements of Neo-Indian culture, like their neighbors in the Southwest.

East of the Rockies, we find a different course of cultural development, in the Neo-Indian as well as the Paleo-Indian period. As already stated, it is thought that agriculture and, to some degree at least, pottery spread into the eastern United States by way of the Gulf coastal plain. There is a gap between the Meso-American cultures at the southern end of the plain and the Southeastern cultures to the north, but this may be due to the unfavorable environment in the intervening area (e.g., Krieger, 1948).

The cultural sequence in the eastern United States is now thought to include three major periods (Griffin, 1952). In the first, the Indians were on an Archaic level of development, i.e., they subsisted by food gathering and lacked both pottery and agriculture. The latest radiocarbon dates indicate that this period began in Illinois as early as 7000 B.C., that it overlapped the latter part of the Paleo-Indian period in the high plains to the west, as Mayer-Oakes has pointed out in another session of this Congress.

The second great period was marked by the appearance of Woodland cultures, characterized by burial mounds, by conical-based pottery with textured surfaces, and presumably also by agriculture. While the agriculture clearly is derived from Middle America, opinion differs as to whether the burial mounds and/or the pottery may not have originated in Asia and spread to the New World along the supposed route of the previous, Paleo-Indian migration (e.g., Ford, Phillips, and Haag, 1955, p. 155).

The third great period saw the rise in the southeastern part of the United States of what is known as Mississippi culture. This culture, which is characterized by flat-topped temple mounds and by globular, smooth-surfaced pottery, is believed to have had its origin in the middle part of the Mississippi Valley, although Phillips. Ford and Griffin (1951), who recently published a monumental study of the area, failed to find clear evidence of this. The temple mounds and certain other ceremonial traits of Mississippi culture are thought to have diffused into the area from Mexico; they, together with certain presumed ceramic influences from the Southwest, may have touched off the Mississippi development.

Mississippi culture failed to reach either the Plains or the Northeast, on the peripheries of eastern United States. Instead, in the river valleys along the eastern edge of the Plains, Woodland culture developed into a local, agricultural form of life, while on the high plains to the west hunting and gathering cultures continued much as in Paleo-Indian times. The low plains between the two were occupied alternately by agricultural and hunting peoples as the climate varied and as the acquisition of horses from the Spaniards increased the efficiency of hunting (e.g., Lehmer, 1954).

In the Northeast, Woodland culture survived until historic time. It spread as far as the St. Lawrence Valley—far above the latitude at which agriculture stopped west of the Rockies—and reached a climax in the Iroquois culture of New York state, which was formerly thought to be derived from the area of Mississippi culture but is now considered to be a local development (MacNeish, 1952).

On the earlier, Archaic level, radiocarbon dates have demolished the theory that the Eskimo influenced the Northeastern Indians (Ritchie, 1951 : 131). Rather it is the other way round. The so-called Eskimo traits of the Northeastern Archaic must have diffused from the Northeast to the Eskimo area, since they are earlier in the former area than in the latter. This hypothesis has been confirmed by Melgaard in his paper at the present Congress, in which he shows that the Cape Dorset culture, formerly thought to be Eskimo, is instead probably to be derived from the Archaic cultures of the northeastern United States and southeastern Canada.

Turning to the Arctic area itself, we are beyond the range of Neo-Indian influences. Instead, we encounter evidences of diffusion from Asia and, during the medieval period, from Europe. The Denbigh flint complex is the best example of the former; it is a pre-Eskimo hunting culture characterized by microliths, or tiny lamellar flakes, like those of the Mesolithic in Mongolia. Giddings (1952 : 91) has recently strengthened the case for origin of this culture in Asia by showing that it is the only culture in the New World to possess true burins or gravers. The microlithic tradition of the Denbigh flint complex seems to have spread from Alaska across the Arctic to Greenland well before the time of Christ and, in many places, to have survived into the earliest forms of Eskimo culture.

The Denbigh flint complex is succeeded in Alaska (e.g., at the Iyatayet site) by "Paleo-" and then by "Neo-Eskimo" culture, the former characterized by partly inland, partly coastal hunting, and the latter by an emphasis on coastal hunting, particularly of the whale (Giddings, 1952 : 90–1). Subsequently, at the close of the first millennium A.D., the "Neo-Eskimo" Thule culture spread eastward as far as Greenland and Labrador (Collins, 1954: 87–91). If Melgaard is correct in eliminating the Dorset culture from consideration as Eskimo (see above), then Thule must have been the first Eskimo culture in the central and eastern Arctic.

The European influence in the Arctic area came about as a result of the Norse colonization of Greenland in the tenth century A.D. We have good evidence of Norse contact with the Thule-Eskimo, and much research has been done in an attempt to find archeological evidences of their further voyages to the mainland, i.e., to Canada and the United States, as described in the Norse sagas. The results are inconclusive (Brønsted, 1954). As yet, we do not have any generally accepted evidence of Norse activity on the mainland, although it should be there.

The Neo-Indian developments in South America remain to be discussed. Again, I shall deal first with the rise of civilization, in this case in the Central Andean center, and shall then discuss the diffusion (or lack thereof) from the center to the rest of South America.

It is necessary to point out first that the Central Andean center, in Peru and Bolivia, is some distance away from the Meso-American center, in Mexico and Guatemala. Certain archeologists have postulated the existence of lost civilizations in the intervening area, e.g., among the Chibcha of Colombia. Recent work, especially that of Haury and Cesar Cubillos (1953) in the Chibcha area, has disproved these theories. It now appears that the cultures of the northern Andes, while they may have had their origin in a common substratum extending from Mexico to Peru, as already indicated, never reached the degree of development which was attained by their neighbors to the north and south, possibly for environmental reasons.

Central Andean civilization, then, must have developed independently of Meso-American civilization, even though the two probably had a common origin and may have subsequently influenced each other to a certain extent. We know more about the Archaic and Pre-Formative stages of this development than we do in Middle America, thanks to the recent work of Bird (1948) on the shell heaps of northern Peru. The Formative, Classic, and Post-Classic stages are also well known, but cannot be discussed here for lack of time (see, e.g., Bennett and Bird, 1949, pp. 95–244).

East of the Andes, in tropical South America, archeologists have devoted some effort in recent years to testing Steward's "Circum-Caribbean" theory. Working largely with ethnological data, Steward (1947) had postulated that agriculture, pottery, and the other elements of Neo-Indian culture spread into tropical South America by way of the Northern Andes and the Caribbean and

Guiana coasts. He assumed that this spread took place on a Formative level of development, producing what he called Circum-Caribbean culture on the north coast of South America and in the West Indies; that this Circum-Caribbean culture degenerated into Tropical Forest culture in the lowlands, under the influence of an adverse environment; and that, in its degenerated form, it spread up the Orinoco and Amazon Rivers into the interior of tropical South America.

Steward's theory has been disproved by the work of Evans (1955) and Meggers at the mouth of the Amazon and of myself in the Antilles (Rouse, 1953). In both places, we found Tropical Forest cultures underlying Circum-Caribbean, contrary to his theory.

It seems to me that Steward may have been misled by an assumption that the agriculture of tropical South America had the same origin as that of Andean South America. Sauer (1952, pp. 45–66) has recently suggested, to the contrary, that there were two different centers of agriculture in the New World: one of seed crops, especially maize, in Middle America (see above), and the other of root crops, such as manioc, in lowland South America. This distinction corresponds to the one made in the Old World between the Near Eastern center of agriculture, based upon cereals, and a Southeast Asian center, based upon root crops and adapted to tropical forest conditions.

Recent research in Venezuela by J. M. Cruxent and myself tends to support Sauer's theory. We find two great cultural traditions, one centering around Lake Maracaibo in the northwestern part of the country and the other centering in the Orinoco Valley to the south and east. The former is characterized by pottery of Central American type and, so far as can be told from the archeology, by maize agriculture. The latter has instead pottery of Amazonian type and bitter-manioc agriculture. Our radiocarbon dates indicate that both traditions extend well back into the first millennium B.C.—in the case of the eastern, root-crop tradition to at least 900 B.C. (Cruxent and Rouse, 1956). These facts lead us to suggest, as a hypothesis worth considering, that there were two more or less independent centers of Neo-Indian cultural development in South America, one in the Central Andes which is related to the Meso-American center and has evolved through the same general stages, and the other in tropical South America which is based upon locally domesticated crops and has passed through its own distinctive developmental stages.

Turning to the south, we find influences from the Central Andes predominant in the Southern Andes, where the Indians were especially skilled in bronze working (Bennett and Bird, 1949, pp. 86–93). Beyond that area, in southern South America, there is cultural stagnation, like that on the Pacific coast of North America. The Indians of southern South America have been termed "marginal" because for the most part they failed to adopt agriculture, pottery, and the other traits of Neo-Indian culture. Because of their isolated position, too, they were unable to receive influences from the Old World, such as are found in northern North America.

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RECENT DEVELOPMENTS IN THE FIELD OF GENETICS

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My assignment is to discuss some recent developments in human genetics that are of interest to the biological side of anthropology. Because the developments are large in number, and the time available is small, I shall emphasize work carried out or published since 1952, the year of our last Congress in Vienna, where more than a dozen papers, including a general one by Dr. Otmar von Verschuer, were devoted to human genetics.

Anthropology and Pennsylvania have a special importance for the history of human genetics. The beginning of formal human genetics dates from 1903 when a graduate student in anthropology at Harvard reported field work in Pennsylvania which first showed that a morphological trait in man followed a simple Mendelian mode of inheritance. This was in the Ph.D. thesis of William C. Farabee (published 1905) which included a genetic study of brachydactyly in a kindred of 69 Pennsylvanians.

For many years human genetics received much more from general genetics than it returned. Aside from material on certain rare pathological traits, published statements on human genetics during the first quarter or third of this century tended to rest heavily on analogy from other organisms. The situation is starting to change. Today, for certain areas in general genetics, human material is the material of choice—that is, for certain topics of general interest, we know and easily can learn more about man than about any other species—and I am *not* excluding insects, nor plants, nor micro-organisms. Anthropologists can take more than a vicarious pleasure in this state of affairs, for in one of these fields (population genetics) they have collected, or collaborated in the collection, of much of the new data.

During the last 100 years, physical anthropologists have shown primary interest in three main problem areas:

(1st) The problem of the evolution of man and his primate relatives. This is the phylogenetic problem.

(2nd) The problem of the comparative growth of modern man. This is the ontogenetic problem (although anthropologists have neglected somewhat the parts of ontogeny between conception and birth).

(3rd) The problem of the classification of the recent varieties of man. This is the problem of anthropological or human racial taxonomy.

Genetics has had some (but rather uneven) influence on each of these three areas.

Up to now, genetics has had little to do with work on human growth. In part this reflects the vast deficiencies in our knowledge of the genetic aspects of ontogeny in any organism (For an excellent review see Dickerson 1954). We are beginning to understand quite a lot about how genes work on the molecular or biochemical level. And we have strong evidence that genes are somehow of importance in controlling (along with environmental factors) "final" body size in man. Clark's twin study (1956) on the heritability of body size, and much of the family studies now being done in Germany and elsewhere in Europe (reported to us by Dr. Schwidetsky) as background for forensic applications are examples. Despite these and other good beginnings, we remain largely ignorant of the detailed structures and events which connect genotypes and phenotypes in the developmental sense.

Genetics has made a greater impact on the study of the evolution of the higher primates. However, except for the most recent stages of human microevolution—a topic I will consider next under taxonomy—this impact has been more on general evolutionary theory (*e.g.*, Simpson 1953) than on detailed, special, substantive contributions. The 1950 Cold Spring Harbor Symposium on the *Origin and Evolution of Man* (Demerec 1951) is typical of the anthropological application of genetical theories of evolution. Dobzhansky's 1955 book on *Evolution, Genetics, and Man* is also characteristic in that it is an excellent introductory statement on the genetical theory of evolution, but considerably less than five per cent of it has to do specifically with man.

By far the greatest impact has been on the third, or taxonomic, set of problems. In fact, for many contemporary papers on anthropological population genetics, it would be difficult to tell from the content alone whether the authors had been trained as anthropologists, geneticists, serologists, or even biochemists.

I assume that you are familiar with the general features of the genetical theory of evolution (sometimes called the statistical theory).

First a definition: By "evolution" we mean "change in gene frequency". It is most convenient to start with a condition in which there is no evolution—where gene frequencies do not change from one generation to the next. For single autosomal alleles this is defined by starting with the Hardy-Weinberg steady state which holds (given random mating for the alleles in question and certain other usually reasonable assumptions) when the frequency of the heterozygous genotype is equal to twice the product of the square roots of the frequencies of the two homozygous genotypes.

In Sewall Wright's system of population genetics (1931, 1942, 1949a, 1949b, 1951), and Li 1955, there are four major, systematic modes of change in gene frequency which are defined in a way that they are an exhaustive list of the determinate, or partially determinate, modes of change. These are mutation, selection, gene flow (also called migration or mixture), and genetic drift. Of course on a local or even larger scale, unique events or other "non-determinate" affairs may have been very important in human evolution. Insofar as anthropologists can document such unique events, population genetics can accommodate theoretically to them. Where nothing definite is known, we naturally must assume some sort of standardized (and probably over-simplified) conditions.

Before turning to more strictly anthropological affairs, I want briefly to sketch the state of present knowledge on mutation, selection, gene flow, and genetic drift in man. There will not be time to discuss here non-random systems of mating, i.e., inbreeding and assortative mating, but unless they are associated with selection, inbreeding and assortative mating are not very important for the study of evolution because they act to change the population distribution but not the frequency of genes.

Mutation is the beginning of all new genetic variation. We know very little about the submicroscopic, physico-chemical events which occur when a gene mutates. However, enough is known about the phenotypical results of mutation to allow (if we can project backward presently observable rates) some insight into the role of mutation as a factor in recent (say, post-Pleistocene) racial differentiation. Known, spontaneous mutation rates per gene per generation in man tend to cluster about 1 in 50,000 in the more reliable estimates, although rates as high as about 1 in 10,000 are as firmly established as any of the better estimates (For a review, see Spuhler 1956). This statement is based on a survey of 37 different studies of mutation rates for 27 different human genes. It is of interest to note that we know more about mutation rates in man than in any other vertebrate, the record for second place being for six genes in the mouse.

Although of first importance in long-term evolution, mutation probably is not of primary importance in explanations of genetic differences between contemporary human populations, at least on the size scale of continental populations. The known differences in gene frequencies tend to be of degree rather than of kind, and processes other than mutation which is recurrent at low rates in all populations seem sufficient to explain the observed degrees of difference. For example, Vandepitte and associates (1955) have shown that the *maximum* mutation rate (and this is seemingly a *high* maximum) for hemoglobin S is not sufficient to maintain the known high frequency of sickle-cell trait in Africa. Probably we will soon have enough information to demonstrate that mutation rates may differ with geographical location—for example, there are theoretical grounds for believing that areas like the high plateaux of Tibet and Bolivia receive sufficiently more cosmic radiation compared to sea level to raise a mutation rate of 1 in 50,000 by about 6 per cent (Muller 1954).

From several points of view, selection is one of the hottest issues in contemporary population genetics. Perhaps the main reason is that we still do not have satisfactory mathematical models (See, for example, Feller, 1951). If you look at the standard texts (like Li, *Population Genetics*, 1955), you find that everything is discussed in terms of hypothetical single alleles, often called "A" and "a". But selection is against or for whole organisms and not the genes at a single locus. Even if we had satisfactory general theoretical models, the job of collecting the necessary empirical data is enormous; the complexity of the job is illustrated by considering that man has something of the order of 20,000 gene loci. We will have to do better than certain psychologists who reported that they were studying the "whole child", meaning that they were considering at least one more attribute than previous workers.

Most geneticists feel that selection (working on variability provided by mutation and recombination) is the primary evolutionary process. Many anthropologists would agree (e.g., Coon 1955). There is considerable controversy about the relative importance of other factors, for example between some British and American workers regarding genetic drift. Most geneticists would agree with Dr. Garn in his statement yesterday that there are few if any neutral genes; and also with Dr. Lehmann that some genes are more neutral than others. I do feel that Dr. Garn (probably intentionally) somewhat overstated the case for rapid change in gene frequencies for the red blood cellular antigens; it is safe to assume that selection is changing them (Brues 1954) but we lack good information on the rates of change.

The diversity in the ABO gene frequencies for closely related local populations is well established (Mourant 1954). However, it is not firmly established that this diversity mostly is due to selection. It may be; it probably is; we simply do not know. In order to demonstrate a selective effect, it is not enough to show that certain rare disease conditions—or differences in sex ratio—are significantly associated with the distribution of the ABO phenotypes. For an introduction to the rapidly growing literature on this subject see Allan 1954, Buckwalter *et al.* 1956, Cohen and Glass 1956, Johnstone 1954a, 1954b, and Kirk *et al.* 1955. To demonstrate selection we must show a change in gene frequency over two or more generations. For example, Matsunga (1955), using pooled material from metropolitan Tokyo, has demonstrated (for *that* population sample) a significant deficiency in blood group A and B offspring in incompatible matings with O mothers and A and B fathers. And he has shown an increased loss during pregnancy for incompatible as compared to compatible matings. But his rather large series did not demonstrate a significant change in frequencies between generations.

The human animal is both a very good and a very bad one to work with on selection problems. It is good because one can utilize a great deal of important information which has been carefully collected on very large numbers by trained people for other purposes. It is bad because man is a remarkably infertile animal in which many factors other than straight biology are extremely important in determining the number of offspring.

Some of our best information on selection in man has to do with hemoglobin S (Neel 1954). Of course here I am neglecting certain of the rare pathological traits like hemophilia and achondroplasia for which we have relatively sound information, and also cases like infantile amaurotic idiocy for which the selective advantage is known to be zero. Dr. Lehmann yesterday brought up to date Allison's work (1954a, 1954b) suggesting a balanced polymorphism with selective advantage for the heterozygotes for hemoglobin S which allows them to live with a high infestation of *falciparum* malaria. Dr. Lehmann's data suggesting that the selective event is in infancy is an important contribution, for it removes a difficulty in previously available data on adults which do not show an increase in the frequency of sickle-cell trait with advancing adult age, a finding which would be required if the selective event were in adulthood.

Because selection is so much in the air now, I am going to stress a conservative attitude. We all agree that selection is important. We must do a lot more work in order to measure its differential importance for known genes and populations. We ought to be very critical about detailed examples of selection in man (See Neel 1956 for a critical survey of present information on hemoglobin S and other abnormal hemoglobins). For instance, there is an equally good, if not better, fit in distribution between mean annual rainfall (and presumably relative humidity) and sickle-cell trait than between malaria and sickle-cell trait. This is at least worthy of serious study since Zarafonetis *et al.* (1955) reported that persons with sickle-cell trait are unable to concentrate urine with a specific gravity greater than 1.018 even with intramuscular injections of 5 units of pitressin. It may turn out that sickle-cell gene frequencies are controlled by factors other than malaria, factors perhaps closely associated with malarial selection.

Gene flow or migration is like mutation in that it is a process which introduces new gene variation into the population. Lasker (1954) has shown that in several American Indian local populations migration rates are sufficiently high to swamp the effects of genetic drift. Glass and Li (1953), Roberts (1955), and Glass (1955) have shown that gene flow from Africa and from Europe has occurred at rates such that the contemporary American Negro population contains genes from the two sources in ratios of the order 7 to 3 or 8 to 2 and that gene flow from American Indians has been relatively small.

Genetic drift (It might better be called "gene frequency drift") refers to the accidents of sampling in gametogenesis and fertilization whereby the frequencies of a set of alleles in the offspring generation may differ from those in the parental generation. The nature of the process is illustrated easily by considering the fact that the two genes present at each autosomal locus in the child are a sample of the four genes present in the parents at that locus. Genetic drift is especially important in the gene dynamics of small breeding populations, while mutation, selection and gene flow are of particular importance in changes in gene frequency in populations larger than a certain critical size, that is, when N is greater than $1/(4u)$, or $1/(4s)$, or $1/(4m)$, where N is the size of the breeding population, u is the mutation rate per gene per generation, s is the selection coefficient (the rate of survival of a gene in a breeding population with reference to one of its alleles taken as a standard), and m is the rate of gene flow per gene per generation into the population. Birdsell (1953) for native Australia and Glass (1954) for the Dunker isolates in this country are excellent examples of recent studies on genetic drift in human populations.

I am going to say relatively little about the blood groups and anthropology. The story is well known. Excellent summaries on the great volume of information available are given by Race and Sanger (1954) on the serology and genetics of the various systems and by Mourant (1954) on the world distribution of these systems. Ten different systems are of especial anthropological significance: ABO, MNSs, Rh, Lewis, P, Lutheran, Kell, Duffy, Kidd and Diego (On Diego see Levine *et al.* 1956). A great deal remains to be done in the anthropological interpretation of the blood groups data (Boyd, *Genetics and the races of man*, 1950, is still the best introduction). In particular we need to pay much more attention, in both the secondary and the primary literature, to the anthropological characteristics of the populations sampled.

By using an arc sine transformation (See Eisenhart 1947), it is possible to define a coefficient of relationship or divergence for gene frequencies so that the divergence between any number of populations taken two at a time for any number of gene frequencies can be expressed as a single number which is zero for identical, and increasingly large for increasingly divergent populations. A multiple of this coefficient is distributed as chi square with degrees of freedom equal to the number of genes whose frequencies are used in the calculations minus the number of loci represented by these genes. Using such a measure of differences in gene (or chromosomal) frequencies, I have calculated (in an unpublished study) the 210 coefficients between 15 European populations for 13 frequencies of the ABO (p, q, r), MN (m, n), Rh (CDEe, CdEe, cDE, cdE, cDe, cde), and P (p′, p″) systems. I was particularly interested to see if the Basques were distinctive if one used all the available data and not simply the Rh system. Each of the populations is significantly different from the others. The order of divergence of each population from the others (as measured by the mean value of the 14 coefficients between each population and the rest) is as follows: Basques 12, Czechs 8, Danes 2, Dutch 10, English 5, French 1, Germans 7, Greeks 4, Irish 11, North Italians 3, Sicilians 13, Latvians 15, Norwegians 6, Poles 14, and Spanish 9. On this criterion and these data, the Latvians are the most divergent, and the Sicilians and the Poles were more divergent than the Basques who rank 12th in the mean coefficient of divergence.

On the basis of this preliminary study, I would suggest that we are not justified in classifying the Basques as an example of "Early European" as historically distinct from (and ancestral to?) the "European" serological race

(Boyd 1950) unless, of course, one is willing to give distinct taxonomic status to Latvians, Sicilians, and Poles.

Aside from the Basques, it seems that Boyd's classification gives an essentially sound picture (See also Boyd and Boyd 1954). The major gaps in gene frequencies correspond to continents with some overlaps which fall (on other anthropological grounds) in the right places—for example, in North Africa, Middle America, and in the circumpolar region. Probably we will need to recognize at least three major divisions for Oceania. These human taxons of continental size are the "Geographical Races" of Garn and Coon (1955) and others. Within the larger units, it is arbitrary how local breeding populations are united or separated in "Local Races". We will do it in different ways for different problems. The type of information we need to know is the degree of resemblance of each local group to all of the others.

Probably the most fascinating and important of recent developments in human genetics of anthropological interest is the mass of new information on the varieties of normal and abnormal hemoglobin. This series of studies started 46 years ago when Herrick (1910) gave a primarily morphological description of sickle-shaped red blood corpuscles in a case of severe anemia. Today few topics in biology or medicine have such a broad sweep of interest, going from the physical chemistry of large molecules to the complex, inter-species, ecological relationships which join mosquitoes, *Plasmodium falciparum* and man in the Old World tropical and sub-tropical regions.

Hemoglobin molecules, when suitably prepared, will migrate under the influence of an electric field. The direction of migration is a function of the net surface charge and the rate of migration is a function of surface charge density. The discovery suggested by Watson *et al.* in 1948 and demonstrated by Pauling and his associates in 1949, that the basic change in a sickle corpuscle has to do with a change in the hemoglobin molecule led to the discovery of several other varieties of hemoglobin which differ in electrophoretic activity and other ways and whose genetic control and population distribution are of anthropological interest.

These varieties are assigned alphabetical names (Neel *et al.* 1953). In addition to normal adult (A) and fetal (F) hemoglobins, the following ten abnormal varieties are known: C, D, E, G, H, I, J, K, M, and S (Allison 1955, Henderson 1956, Zuelzer *et al.* 1956). Neel, Miller and Livingstone may have found two new varieties in Liberia during the present year. Dr. Lehmann has already reviewed for this Congress something of anthropological significance of the various hemoglobins. For those interested in the classification of human populations in terms of gene frequencies, the hereditary hemoglobinopathies have special importance because of their uneven distribution in the populations of the world.

There is not time to mention details of recent genetic work on thalassemia, PTC taste reaction, cyanide smelling, color vision, body size and certain morphological traits, dermatoglyphics, and variations in pigmentation. Before closing I want to mention two new lines of genetic work with possible anthropological application.

The excretion of β-aminoisobutyric acid (BAIB) in the urine is readily detected by paper chromatography (Sutton 1955). Several investigators have shown excretion of BAIB to be highly characteristic of individuals and mostly to be independent of certain environmental effects including diet. Harris (1953) for English, and Calchi-Novati (1954) for Italian families, as well as others, have put forward a tentative hypothesis that high excretion of BAIB is a simple Mendelian recessive. However, when quantitative measurements are made, population samples show intermediates between high and low excretors (de Grouchy and Sutton 1957). Using a scale based on reflectance readings from the chromatograms where 100 is no detectable excretion of BAIB and 40–50 is very high excretion, de Grouchy and Sutton (1957) find a correlation between the average of the two parents and their offspring of $r = +.53$, which is significant at the 1 per cent level and an indication that excretion of BAIB is rather strongly inherited.

The distribution of BAIB excretion is known for Asiatic Indians, Whites, American Negroes, Thai, Chinese and Japanese for samples which are rather small. None of the Indians (mostly Hindu) are high excretors. The Caucasians are next lower in excretors, while the Negroes, Thai, Chinese, and Japanese all show a high percentage of individuals who excrete moderate or large quantities of BAIB. Ranking tests show three clusters of population samples which differ significantly from each other: (1) Indian–Caucasian, (2) Negro, and (3) Thai–Chinese–Japanese (Sutton and de Grouchy 1956, Sutton and Clark 1955).

By employing starch gels as a supporting medium for zone electrophoresis, Smithies (1955) distinguished three qualitatively different types (called Types I, IIA, and IIB) of α_2 globulins in normal human serum from Caucasians. Genetic studies are under way. Smithies and Walker (1955), in an interim report on 18 families with a total of 39 children, obtained evidence that these groups are controlled by two alleles lacking dominance, Types I and IIB representing the two homozygotes and Type IIA the heterozygote. Sutton *et al.* (1957) have shown that the types differ markedly in frequency in a pooled series of Caucasians from Toronto and Ann Arbor, Michigan, and in Africans from Liberia and the Ivory Coast. Tests have been done on 98 Whites and 80 Africans. The frequency of the gene which when homozygous results in Type I is .41 in the Caucasian sample and .78 in the African series. If supported by future work, this inherited difference in the serum globulins will be of value in studies on human population genetics.

Finally let me mention a possibility which is still in the future. Puck and his associates (1955, 1956), working in the Department of Biophysics at the University of Colorado Medical Center in Denver, have devised tissue-culture methods which allow cultures of human cells to be grown where all of the cells are descendants of a single cell. This provides a population of cells all of identical genotypes. Developments from this method may make possible a vast amount of new work on human biochemical genetics using techniques like those which have been so successful in the study of *Neurospora.* Since the composition of the culture medium can be finely controlled, very delicate differences in cell metabolism can be detected by seeing which cells are able to live and grow in what media. When developments like this are accomplished, we can look forward to the discovery of a large number of biochemical variations of interest to human population genetics and to anthropology.

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SECTION II

THEORY AND METHOD

INDIVIDUAL VARIATION IN CULTURE

Regina Flannery

It is generally accepted that what Benedict called the Dionysian configuration of Plains culture represents a dominant, if not the only trend. By definition a configuration is a generalization lying behind and explaining a number of cultural patterns. A pattern itself is of course an abstraction from the behavior of a number of individuals. The behavior of any one person may vary at different times in respect to a particular type of situation, but it is unlikely that the individual will show the whole range of possible behavior within a pattern; it will cover only a portion thereof. As Linton pointed out, the difference between the individual mode within this segment, and the mode of the behaviors on which the pattern is based, reflects the compromise which every individual has to make between the culture patterns of his society and his own inclinations. It would seem further, then, that if the modes of an individual's behavior-ranges show a consistent displacement with respect to a large series of patterns, it is safe to assume that the direction of this displacement reflects some quality of the individual.[1] The question we wish to raise is this: If the aspect of personality thus revealed runs counter to a dominant trend of his culture, is the individual a misfit in that society?

In attempting an answer, let us examine some individual variations within those patterns of Plains culture in which expression of emotional excess, and dangerous experience were expected of women in one Plains society, that of the Gros Ventres of Montana. We shall outline in some detail the behavior of one Gros Ventre woman in respect of these selected patterns—from which the Dionysian configuration was abstracted—and contrast her behavior with that of others of approximately the same age. All had participated as adults in the old way of life before the disappearance of the buffalo in 1887.

In Gros Ventre culture, as in all the Plains, there was high valuation placed on direct contact of the individual with the supernatural through dreams and visions. Men could actively seek such contact by employing dangerous means, and did so in order to obtain many kinds of power. While women did not go on the actual power quest, they might acquire medicine powers and less frequently, perhaps, powers of clairvoyance. Medicine powers were considered dangerous in that, it was believed, by accepting them a limitation was put upon the life span of the practitioner. But the rewards in terms of prestige for successful performance and of payment for services were high for women as well as for men. Not every individual man was successful in his quest, nor of course was every woman vouchsafed the kinds of dreams which were requisite. Coming Daylight, the woman whose behavior we are examining in particular, was what her contemporaries considered fortunate indeed in having been offered curing powers of several kinds. Nevertheless she rejected the possibility each time, in spite of the fact that she had a living example in the person of her father's sister of what it could mean in prestige to a woman who accepted. As the reasons for her refusal seem significant I shall describe her experience in her own words.

"My experience was by dreaming. One time before I was married—I must have been about eight years old—I was sleeping in a big lodge and a 'person' came and wakened me. He had a bag of medicines on his back. He came to me on four successive nights and laid out these medicines and told me what each was for. On the third of these nights there was a man lying there—he was just skin and bones. The 'person' who had awakened me told me: 'You could take all these medicines and you could cure people like that one lying there' [i.e., a person in the last extremity]. In my dream I would do everything he told me, manipulate my hands to draw out the sickness, and do everything. But I didn't want that power and would tell him so. Each morning too I would relate to my grandmother exactly what had happened in the night but she would try to hush me up, telling me: 'Don't say anything; don't tell anyone. You will be a great medicine woman.' But I didn't want that power. I didn't like it because when medicine people suck on the weak part of the sick person they have to swallow what they extract. I had decided I would rather be quiet and not do anything like that, or sacrifice my flesh, or things of that kind. Perhaps if it had come later I might have taken that power and become great."

Nevertheless when later in life she had other dreams indicating supernatural power, she likewise refused. She explained in regard to one of these experiences, for instance: "The one who awakened me this time had told me to face the rising sun and was showing me how to make a smudge from the moss that grows close on logs; but I told him. 'No. I would rather not have it.' Those who have power die too soon."

Incidentally, still in connection with curing, bleeding was an operation which was performed purely as a natural remedy for a person who felt dizzy or sluggish. Anyone could do it—no supernatural validation was needed. Other women described quite casually how it was done and discussed the various times they had actually performed the operation. Coming Daylight, however, insisted that while there was really nothing to such an operation, she could never bring herself to perform it, nor would she allow anyone else to bleed her; she could not stand the sight of blood.

The aversion to the sight of blood and to self-mutilation was evidently deep-seated and showed itself in a marked manner early in her life. On one occasion a close relative was very ill. In such a situation it was expected that every member of the family would rally around and do whatever he could toward the recovery. Among the things that fell within the scope of activity for female relatives was that of vowing to sacrifice something valuable, including perhaps the blood from one's arm or a finger-joint, if the person recovered. At this particular time, Coming Daylight was a young child, and her grandmother decided that it would be appropriate for the little girl to offer that her finger-tip be removed if the relative regained his health. When it was suggested to her, Coming Daylight refused, even though her grandmother took after her with a stick. She actually ran away and wandered alone, until two nights later the relative died and an uncle finally found her.

On another occasion in her early youth, too, she defied a convention connected with kinship by refusing to join in mourning for an uncle who had been killed in revenge for having instigated the murder of his brother's wife. When her grandmother upbraided Coming Daylight for not expressing grief in the traditional manner, the latter defied her by saying: "I am not going to cry for that man. I don't know why you are crying for him. I have worked for that woman and she has fed me, and has even given me meat to bring home to you. I heard the shots and went later to see what happened, and there I saw her lying across the fire; her dress had burned off and her body was all shriveled. They weren't satisfied to kill her, but they burned her too!" She maintained her attitude and all the scolding from her relatives was of no avail.

Before turning to her adult life, we may refer to another instance which illustrates Coming Daylight's sensitivity to the suffering of others. In the Sacrifice Lodge, which is the Gros Ventre version of the well-known Plains Sun Dance, one of the important features was the spectacular self-torture which the active participants underwent. Each one had a sliver of wood embedded in the flesh of his chest. To the wood was tied a line, the other end of which was fastened high on the center-pole of the lodge. The man, leaning back so that the line was taut, went back and forth until finally the sliver was torn from his flesh. Meanwhile the women cheered on their relatives by "rattling their tongues," singing war songs, and the like, by way of encouraging the men to be brave and see the thing through. Coming Daylight was quite a young child when she first witnessed this performance. She remembered vividly her reaction when one of the young men, in his exhaustion, went to the center-pole and, putting his arms around it, wept aloud. She was not able to stand it any longer and rushed home crying. Her grandmother was very angry with her for leaving in the middle of the performance, but all the little girl could say was: "Oh, I just pitied that young man," and she could not be induced to return to the dance lodge. We may add here that actually in later years she did not participate in the Sacrifice Lodge even so far as women were allowed to do so.

The examples we have given are sufficient to show, I believe, that Coming Daylight had developed as a child a very strong set against violence, against painful and dangerous experiences. She refused medicine power from supernatural sources not only because it was dangerous but also because it involved a kind of physical activity which was repugnant to her. She refused to mutilate herself and was extremely sensitive to painful experiences in others. Her actual deviations from the patterns of accepted or approved behavior all tended in this same direction against violence. Her adult behavior likewise was consistent with this trend in her personality.

In consonance with her feeling against physical violence Coming Daylight avoided as far as possible the rough-and-tumble play in which brother-in-law and sister-in-law were free to, and indeed expected to, indulge. Other women told with glee how they enjoyed "getting even" with their brothers-in-law by, for instance, pushing them when fully clothed into the river, or dumping bucketfuls of water over their heads when they least expected it, and so on; but Coming Daylight admitted without hesitation that she didn't like that sort of thing and never attempted to retaliate even when her brothers-in-law succeeded in playing jokes on her. Similarly while it was not unusual for women rivals to actively fight over a man, she refrained from such undignified behavior, although it must be admitted that she had enough provocation on at least one occasion of which she told me. She had just married a man who was described by others as something of a gay Lothario. A woman who had been trying to force her attentions on him was furious when she heard of the marriage. She rushed to the lodge and pulled Coming Daylight out feet first, yelling insults the while. When the latter regained her feet she made no attempt to hit back even though the other slapped her and pulled off an earring, tearing the lobe of the ear. Coming Daylight, however, bided her time and soon was able to make an opportunity for an exchange of words with the woman at a big social dance.

Her final rejoinder was such a witty and pointed one that everyone laughed heartily at her rival's expense. In this culture, of course, public ridicule was a far more effective retaliation than the biting and scratching that other women said frankly they would have indulged in when attacked.

Expression of emotional excess was institutionalized for the Gros Ventres, as for the rest of the Plains, in the bereavement situation. Upon the death of a husband, son, or father, a woman was expected to start wailing as soon as she heard the news, have her hair cut off, discard good clothes for old ragged ones, gash her arms and legs, go to the hills to mourn and cry, resisting for a time the pleas of her relatives to return, and being watched lest she attempt suicide. Coming Daylight was only a young woman when she was first widowed and the mourning pattern was still in full vogue. It is certain from other evidence than Coming Dayling had a deep affection for this husband to whom she had been given as a child-bride and in whose kindly and indulgent treatment she had found a pleasant escape from the rigorous discipline of her grandmother. Nevertheless the public expression of her grief was minimal—no abandonment for her. Although her sister was sent on the hills with her to keep watch over her in the usual way, Coming Daylight was easily persuaded to return shortly though she herself was proud of the fact that she had stayed out so long that she actually had got a frozen toe! In later years when all that was left of the ritual was the wailing out on the hills, she lost the son whom she herself, and everybody else, acknowledged as her favorite. She had not been on the hills very long when she experienced a significant dream, as follows. She was in a lodge at the rear of which was a big pile of tobacco mixture and a pipe. The owner of the lodge filled the pipe and approached her. She was frightened because she didn't smoke. The owner of the lodge however told her: "Look at these people." She looked and saw many with their hair cut and their bodies bleeding from the gashes inflicted in grief. The man then told her: "That is in the past. Nowadays your only consolation is in smoking." So she accepted the pipe and, after smoking it, she was painted in ceremonial fashion by each of those other mourners. Then the owner of the lodge dismissed her saying: "Now, go home and don't ever go on the hills and mourn again. But always smoke if you want to." She went home right after this dream and when she told her husband of it he accepted it as valid and persuaded her sister to prepare for Coming Daylight her first real smoke. She maintained that the habit subsequently developed was actually a great comfort to her.

The final area with which we shall be concerned here is the war complex—a vital aspect of the old Plains culture. Gros Ventre women were allowed direct but limited participation in these activities in the sense that, while they would not join a party of men who were going on horse-raiding or revenge expeditions, if opportunity offered they might exhibit the same kind of bravery that a man should show and derive considerable status therefrom. For instance, if one of the enemy fell nearby, the women rushed out from camp to count coup, and to grab trophies with which to inaugurate the relationship known as "enemy-friends." Coming Daylight, however, was never one to indulge in this sport. She told of the one—and to her extremely distasteful—occasion on which her hand literally was forced. It seems that during a raid on their camp one of the enemy was shot and her husband scalped him. The victim was lying not far from her lodge and, as her husband dashed off to pursue the rest of the Assiniboine, he shouted for her to come out and see what was going on. Coming Daylight saw the man lying there, face down, covered with blood. He was not dead and, each time he attempted to get to his knees, one of two women who had already reached the scene would strike him on the buttocks with the flat side of a hatchet, making an insulting remark, and both would laugh. Coming Daylight hesitated at some distance from where the other two were gloating over the fallen enemy. When one of them, Woman Chief by name, saw her she ran to her saying: "Why are you standing so far off?" She grabbed her by the arm and pushed a big club into her hand, saying: "He is still alive—you will do a great thing. *Ahe:ya'!*" Woman Chief forced Coming Daylight to strike him.

While both Woman Chief and Coming Daylight closely approximated the ideal for women as faithful wives and efficient workers who gave generously of their material goods to and on behalf of their relatives, they seemingly differed in temperament. Woman Chief was known as a very brave woman, and Coming Daylight was frank in admitting she would never have dreamed of doing the kinds of things for which Woman Chief was renowned. One of the latter's exploits, for example, was as follows. The Gros Ventres and Crow were allies in an encounter with the Piegan. The battle was in full force, but one Crow had withdrawn and was sitting motionless astride his horse at some distance from the melee. Woman Chief, who in contrast to most women was always well mounted, dashed up to the Crow, grabbed his wand from his hand, and rode pellmell into the thick of the battle, counting coups right and left. So brave was she considered in having really risked her life that she was accorded the distinction, most unusual for a woman, of being led around the camp circle with the victorious warriors. Furthermore, so many people came to sing in front of her lodge that she and her relatives, to show their appreciation of the honor, were forced to give away as much property as though she were a man. Judging on the basis of other biographical material, there can be no doubt that she gloried in her reputation and took every opportunity to enhance it. She was one to whom the more violent aspects of Gros Ventre culture were exceedingly congenial.

If we compare and contrast these two women we can say that insofar as the patterns of Dionysian configuration are the basis of the comparison, their behavior exhibits opposite extremes, the one going beyond the average in the direction of the configuration, the other running counter to and participating less than the average therein. Both might be termed deviant personalities in Honigman's sense of the term,[2] one overplaying and the other underplaying modal behavior in these selected patterns. Nevertheless, neither seems to have been a misfit insofar as each represents an extreme of socially accepted or sanctioned behavior within those patterns. If, for instance, Coming Daylight had been completely negative, and refused, say, to carry out at least the culturally defined minimum for mourning, had she, in addition to refusing to mutilate herself for the recovery of her ill relatives, neglected as an adult even the safe and less strenuous means to that end, she would have fallen without the pale, so to speak, and have been a misfit. If, at the opposite end of the scale, Woman Chief had gone further in the direction of masculinity, had she actually gone on raiding expeditions and herself stolen horses from the camp of the enemy, had she learned to shoot the bow and arrow or gun, or had she so much as adopted the vocabulary and/or pronunciation proper for males in the Gros Ventres language, she would have been carrying things too far. She, too, would have been dubbed a misfit.

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Notes

1. Ralph Linton, *The Cultural Background of Personality*, New York, 1945, p. 51.
2. John J. Honigman, *Culture and Personality*, New York, 1954, pp. 211–212.

CONFLICT AND CONGRUENCE IN ANTHROPOLOGICAL THEORY

Linton C. Freeman

Whatever their particular science, scientists have a way of getting themselves classified according to some theoretical position. Thus, we call ourselves (or others call us) evolutionists, or neo-evolutionists, or structural-functionalists, or ideal-typologists, or some other theoretical name. And for the most part we accept these epithets—even with pride—for each of us is proud of his theoretical position; and we can always see the error in the other fellow's ways. The members of any theoretical "school," then, are quick to criticize the theoretical efforts of the "opposition." But this report will attempt to show that critics are often ready to reject whole theories in toto, merely because they cannot accept the assumptions implicit in their presentation. Various seemingly conflicting theories are, in certain fundamental aspects, congruent, or at least complementary, but certainly not in basic conflict.

Perhaps the best place to begin is by asking, "What is a scientific theory?" For the relatively undeveloped theories of the social sciences, we can answer that a theory is the expression of a set of hunches about which things go together in the world of our experience. Clearly, this is an over-simplified and highly informal definition of theory, but it will give us a point from which we can start our discussion. The important thing to note about this definition is that it includes not only hunches or hypotheses about the world, but their expression as well. And this is where the problems arise, for it is in the linguistic expression of a theoretical system that its assumptions can be found, and it is these assumptions which lead to the conflict.

Whitehead (1948, p. 25) has said that, "It often happens . . . that in criticizing a learned book of applied mathematics, or a memoir, one's whole trouble is with the first chapter, or even with the first page. For it is there, at the very outset, that the author will probably be found to slip in his assumptions." Very often it is these assumptions, and not the substance of an author's work, with which his critics are at odds. But since a theory is designed primarily to organize knowledge, its hypotheses—not its assumptions—are its most significant elements.

The assumptions which color the expression of a theory are of two sorts. In the first place, the statement of a theory reflects the methodological assumptions of its author. It may be stated, for example, in the language of functionalism, or the jargon of causality, or the symbolic notation of mathematics. Thus, one biologist might say that the operation of the heart is functional for the maintenance of human life. Or another, that stoppage of the heart causes death. And a third might state the same proposition:

$$(\Delta P)[L(P) \equiv H(P)],$$

which can be read, "For any person, he is a living person if, and only if, he is a person with an operating heart." Each of our biologists is making the same assertion about an empirical covariation, but their methodological assumptions and hence their languages differ; and we may get the impression that they are saying quite different things.

Secondly, the theories of a given time and place are stated in such a fashion that they reflect the *Weltanschauung*—the basic philosophy—of the culture in which they emerge. Thus, Rousseau and Morgan may introduce similar hypotheses about the interrelationships among variables in the empirical world, but these will be placed in very different evaluative settings. Rousseau will tell the tale of the vast degeneration of mankind while Morgan will suggest man's colossal progress. So again, the illusion is created that entirely different schemes are being presented.

Typically, members of one theoretical "school" are ready to reject the theories of another on the basis of their assumptions alone, without ever a glance at their hypotheses. Today, for example, we find ourselves in a cultural setting in which science is, in and of itself, a Good Thing. We are enamoured of the rational-empirical model, and we are quick to reject theoretical schemes which smack of any other basic philosophy. Any theory which includes a suggestion of evaluative criteria is immediately suspect. So we build our theories around an attempt to avoid evaluation and condemn our more value-ridden forebears.

Less consensus, however, exists with reference to methodological assumptions. For today we can still find evolutionists like White (1949) and Child (1951), who view scientific anthropology as a search for diachronic relationships, structural-functionalists like Bennett (1949) and Levy (1952), who view scientific anthropology as a search for synchronic relationships, and culture historians like Mead (1953), who try to avoid viewing scientific anthropology at all. It is among proponents of these three schools of thought that conflicts in theory exist. Such conflicts, however, are centered around methodological assumptions; let us glance briefly at some of their hypotheses and see to what extent real differences in theory do obtain.

To illustrate evolutionism we shall look to the theory of E. B. Tylor. In Tylor's work we find evolutionism in its purest form; it includes not only the search for diachronic relationships, but the use of evaluative criteria as well. Tyler lived and wrote in a period and place where the dominant theme of the culture was progress. People looked about them—at the technological advancement, the political enlightenment, the economic expansion—and they were convinced that they lived in the Best of All Possible Worlds and that it was getting better day by day. A general spirit of social and spiritual improvement was in the air. And Tylor was swept up in the current. So his interest in and thoughts concerning man's social life were built around the concept of evolution.

Tylor (1921) used the concept of "evolutionary stages of human life" to organize his thinking about society. He defined three such stages: savage, barbaric, and civilized. The savage stage is characterized by small settlements, a hunting and gathering economy, and simple wood, stone, or bone tools. When the members of a society "rise" into the barbaric stage they develop agriculture or herding. In this stage there are settled villages, governmental organizations, and the beginnings of metal craft. Civilized life begins with writing. It includes extensive trade with other societies, bilateral reckoning of descent, formalization of government and jurisprudence, specialization, and the development of social classes.

Thus, in proposing these three stages in the development of civilization, Tylor outlined several criteria characteristic of each stage. Some characteristics which appear at earlier stages (say small settlements) disappear and are replaced by others (settled villages) at later stages, while others are cumulative —that is, they emerge at a given level of development and continue to appear at each successive level (metal craft appears at the barbaric stage and continues on into civilization). Thus, to rid this scheme of its evaluative connotations, we find that Tylor was talking of variables when he described his characteristics. For example, the variable occupational specialization has two values; (1) no specialization, and (2) specialization. Just so, settlement pattern as a variable has three values: (1) small bands or hamlets, (2) settled villages, and (3) urban centers or cities. And furthermore, if we reinterpret Tylor's stress upon evolutionary stages, we find that he has suggested a characteristic relationship among his variables. He has proposed, for example, that a change in settlement pattern will be accompanied by a change in occupational specialization—that, in fact, settlement pattern and occupational specialization vary together, and that they are correlated with all the other variables: tool types, social organization, subsistence economy, and the like. In short, Tylor has proposed a specific interrelationship among a set of societal variables.

The general viewpoint of the functionalists was summarized by Malinowski (1944) when he defined culture as ". . . an integral in which the various elements are interdependent." The basic hypothesis here is that, given the presence of a particular element in a society or culture, certain other elements will also be present. Bennett and Tumin (1949) specify this hypothesis by suggesting that a certain form of economic structure (e.g., industrial economy) will be associated with a certain form of social organization (e.g., lack of strong familism). They assert that a certain population size will be associated with a certain form of economic structure, and a certain amount of trade with a certain social structure. And Levy (1952) suggests further that the amount of occupational stratification will be associated with the complexity of the society, and the amount of specialization with the presence of classes.

The approach of the structural-functionalists, then, is synchronic. Instead of looking for stages in the "progress" of a society, they seek to find associations among cultural elements at a given time. Thus, they hypothesize that a society with a large population will have an industrial economy, and conversely, one with a small population will probably lack industry. So again, they are describing variables, and again they are suggesting association among these variables.

We have seen that both the evolutionists and the structural-functionalists propose that a set of societal variables are interrelated. Furthermore, these are, for the most part, the selfsame variables. Both, for example, propose that size, specialization, social organization, economic structure, amount of trade, and the like are associated. Their difference, then, rests in the diachronic approach of the evolutionists versus the synchronic view of the structural-functionalists. But how different are these approaches? If, as the evolutionists propose, one "stage" of society follows another, the elements at any given level must change together into those typical of the next level. Hence, these elements are interdependent, and the functionalists are correct. And if, on the other hand, certain elements in a culture or society imply others, as the structural functionalists would have us believe, then any change must involve the transformation from one functionally interrelated set to another. This implies an ordered series of types of society and, therefore, the evolutionists are right. Each of these schemes, then, implies the other; the structural-functionalists stress patterns, and the evolutionists stress change, but they are both talking about the same thing: a set of socio-cultural variables which vary together—an interrelated set of cultural characteristics. So, fundamentally, when it comes to their hypotheses, there is no conflict between these two theories, there is only congruence.

The work of the culture historians has, for the most part, stressed differences rather than similarities among cultures. Clearly, this approach is not congruent with the two just described. But even the works of such noted proponents of the historical approach as Kroeber and Lowie are not entirely free from hypotheses about regularities among cultures. Thus, Kroeber (1942) has suggested ". . . that among primitive peoples society is structured primarily on the basis of kinship," while ". . . successful technological and political developments . . . characterize the more complex civilizations." And Lowie's book (1940) abounds with distinctions drawn between "primitive" and "civilized" societies. He has summarized these distinctions by saying that:

> ". . . certain cultural traits appear to be organically linked, so that one of them renders the presence of another more probable or, on the contrary, may tend to exclude it. In some instances the nature of the correlation is clear to us; in others we merely recognize its reality and suspect that some intermediate link eludes us. Thus we readily see why pigs do not go with pastoral nomadism and why pottery accompanies a sedentary life." (Lowie, 1940; p. 384.)

Thus, the culture historians are describing the same association among the same variables as did the evolutionists and the structural-functionalists. In this case, the emphasis is upon the unique in a given culture, but comparisons are made and hypotheses concerning regularities are always just beneath the surface. All of these schools, the evolutionists, the structural-functionalists, and the culture historians are expressing the same hypotheses in different linguistic guises. Their methodological assumptions differ, but when we look to their hypotheses their conflict disappears—they become congruent—a single theory of socio-cultural form and process.

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CULTURE AND HUMAN BEHAVIOR

Walter Goldschmidt

In this essay I wish to defend the proposition that—as it pleases me to put it—cultures differ more than people. By which I mean that the varieties of cultures on the earth offer a far greater range of behavior characteristics than will be found if we examine the actual behavior of the peoples who possess—or profess—these cultures. After defining and illustrating what I mean by this statement, I will develop the implications that I believe it has for anthropological theory and the relation between the anthropological enterprise and the sociological.

Let me begin by reminding you that anthropology was once the science of custom. The very word has an old-fashioned ring in these days of demography and ecology and Rorschach. Yet I believe that we have neither moved so far from the science of custom nor that we should move so far from the science of custom as these current events suggest. In an earlier era, we conscientiously studied the couvade, the avunculate, bride capture, etc., etc. We clustered them into complexes, we distributed them over the face of the earth and occasionally we psychologized about them. When we had gathered together all the customs of a people, we had its culture. Thus, reasonably enough, Tylor's definition of culture was fundamentally a listing. Little wonder that in those days culture was a product of history and could be thought of as a thing of shreds and patches. (Parenthetically, Lowie's figure of speech was not so anti-functional as it has been considered. It begged the question of whether the garment fits, and only made not unreasonable assumptions about its provenience.)

As time went on, anthropology more and more became a science of culture—indeed, culture has become the watchword of our discipline. We need not here go over the grounds so ably cultivated by Kroeber and Kluckhohn and discuss the history and variety of that concept. The fact is that we remain the science of culture and, I believe, in the particular way in which I intend to use this word: namely, that culture is the integrated—the functional—presentation of custom.

The basic grist of the anthropological mill is the ethnography: the description of the life-ways of the people, the description of a culture—or that sector of it which the ethnographer was willing and able to record. The ethnography is characteristically a description of normative behavior. Picking Radcliffe-Brown[1] from my shelf and opening it more or less at random, I read: "A belief about the moon which is found in all the tribes, . . . is that he will be very angry if there is any fire . . ." There is no question whether every Andamanese believes this or not. Or again: "If a death occurs, the camp is deserted for several months and a new one is occupied." Here there are no discussions of frequencies, other than the implications embodied in a "generally," an "often," an "occasionally" or sometimes an uncalculated "on the average." My point is this—the ethnographer is describing the expectable, and not the actual. He is not counting occurrences or describing individual behavior. We may say that he is describing the normative and not the statistical norm.

Even when the case method is used, it is generally assumed that the given case is "typical" of a culture or a segment of it. Such a study as Max Gluckman's on Barotse legal procedure,[2] careful as it is to build case upon case, nevertheless ends up with the normative, rather than with actual behavior. Consider, for instance, his emphasis upon the reasonable man as viewed by the Barotse. This concept of reasonableness is a cultural—a normative—assumption. (Perhaps I should not accuse a British social anthropologist of describing culture, but it seems to me that this is actually the case.)

The anthropologist answers this statement by the assertion that the units he characteristically studies, primitive communities, are generally so homogeneous that it is unnecessary to make distinctions. This may indeed be the case but the important point is that the anthropologist does not really worry about the matter until he is challenged. He is not, when engaged in his more usual forms of enterprise, concerned with the occurrences. And rightly, I believe, if we make some assumptions about the nature of the anthropological endeavor.

Let us take a simple example. Anthropologists have long used sesquipedalian words to distinguish certain types of marriage. A society is polygynous, not when all men have more than one wife or when 51 percent of the men have more than one wife, but when polygyny is a recognized or condoned form of marriage. Perhaps when only the chief has more than one wife, the anthropologist is uneasy if he calls the society polygynous, but beyond this he is unconcerned with proportions unless he has set himself some special problem. To this the anthropologist will counter that he is really dealing with the rules and the regulations, the established beliefs, perhaps the ideal practices. In this response the anthropologist is, of course, on safer ground. Indeed, I think he is right. He is talking about the rules of the game, the cultural assumptions, and the patterns of expectability. He is not talking about human behavior at all, except in so far as the behavior of the people he is describing conforms to the customs they profess.

To be sure, ethnographic practice frequently makes this distinction in the actual field report. For example, Fried[3] has explicitly examined the divergence between the normative and the actual behavior, showing how culture operates despite continuous breaches of custom—and others have faced the problem in the field. But the distinction escapes the theorist. Kroeber and Kluckhohn do not develop the distinction, and say very little about actual behavior at all. Some of their definitions are normative in orientation, none are explicitly behavioral, and in their final summing up they say "culture consists of patterns . . . *of and for* behavior. . . ."[4] To them the distinction remains without meaning—passed off in three short words.

What we are saying therefore is that the science of anthropology is a science of the normative. We sometimes speak of ideal culture, but the phrase is misleading, partly because of its popular connotations with respect to positive valuation, but more so because it implies that there is some other kind of culture. That "other kind of culture" is the actual daily patterns of behavior engaged in by a population and is sometimes referred to as "real culture."[5] But it is our thesis that this is not properly considered culture at all rather, it is behavior. We may state the proposition this way: Culture is the "rules of the game." The rules are arbitrary and restrictive. The game is not always played according to the rules. What people do therefore varies from the rules, from culture.

Perhaps only a lexicographer has the right to say that a word means one thing and not another; but what is important is to recognize that two very different kinds of phenomena tend to be subsumed under one term. Furthermore, the history and the usage of the word "culture" place it more clearly with the normative, with the rules-of-the-game aspect than with the actual behavior pattern. As indicated, the two may coincide in this or that instance, but it is harder to find examples than you might think.

If we may accept this distinction as the valid and important one I think it is, we may address the central proposition of this paper: that cultures vary more than behavior. Two points must first be considered.

First, while the basic data of the anthropological discipline constitute the ethnography, the science of anthropology rests upon comparison—the study of variation between people, the spatial and temporal occurrence of culture, behavior, or some aspect of these. In anthropology this comparative approach (and it is implicit in such a study as that of Alor, as well as explicit in most theoretical analyses) is generally a study in comparative culture. We study the distribution of the couvade, without counting the men who lie in; we compare with Francis Hsu the patterns of China and America without recourse to statistics; we analyze kinship behavior without counting the actual frequency of cross-cousin marriage in each "case" where it occurs.

Second, it is necessary to examine the relation between culture and the human potential. (We substitute this phrase for "human nature" since it is part of human nature to subscribe to culture.) It is our belief—and one generally voiced—that culture is restrictive on human behavior. Language exemplifies this in a simple way. We all know that the human organism has the potentiality for expressing and distinguishing an almost infinite number of sounds. We know also that culture limits the regular use of these sounds, frequently rendering the individual helpless in early efforts to articulate others or to disciminate between some. It is in this sense that we view culture as restrictive (carrying no necessary connotation of being repressive). The same may be said for those aspects of culture which relate to the satisfaction of appetites. Food tabus and tastes limit the means of satisfying hunger; sexual regulations limit the possibilities of attaining libidinal gratification. Again this is true for beliefs and attitudes. It is true also for those more generalized aspects of cultural behavior: aggression, competitiveness, industriousness, cooperativeness, self-immolation, and the other qualities which have been ascribed to one people or another. We may say that narrow sectors of the broad arc of human potentialities in these various attributes are selected as culturally acceptable. Man has the capacity for competition and for cooperation, but here a culture focuses on the one, there upon the other. Man has the capacity for repose and for industriousness, but there it fixes on the former, here upon the latter.

Behavior, however, is different from culture. It slops over the narrow range. That is, members do not conform to the normative. Indeed, they may actually reject it and go to an opposite extreme in an act of defiance, or go to an alternate pattern, as when a Plains Indian become transvestite.

If we continue to think of that arc and the narrow sectors of cultural norms, we realize that any position on the dial may be selected, from one extreme to the other. Indeed, it seems as if the culture tends often to fix on extremes, though I sometimes suspect that it is the ethnographer who fixes on the extreme and not the culture. There is also a tendency among us to talk more about such extreme cultures as the Kwakiutl, Eskimo, or Arunta, than about the more bland ones, such as the Tswana or Alaskan Athapaskans. (We have not entirely dissipated what one of my colleagues calls the "Oh-how-quaint School of Anthropology" in our literary emphasis upon the unusual.)

Nonetheless, when we talk about degrees of difference between cultures, we are talking about the extreme range—from celibacy to licentiousness, from industriousness to slovenliness, etc. (Not all cultural variations can be placed on a lineal scale, but the same principle applies.) Such extremes can be held as cultural norms; they cannot, however, be sustained as behavioral attributes.

Let me illustrate with the most famous case of cultural extremes in the ethnological literature: the Kwakiutl and Pueblos as described by Benedict. A recent paper on the amiable side of Kwakiutl life[6] makes us aware that not all Northwest Coast behavior is Dionysian, and makes us feel, even, that the Kwakiutl had the capacity to lampoon their own cultural urges. The obverse of the coin is also exemplified. This point was most ably made years ago by Li An-che[7] who queried some of the assumptions of Benedict and others. But I prefer a story told me by John Adair. While watching a number of Pueblo children at school, he overheard their conversation. They were discussing their handicrafts, and each seemed to be outdoing the other in his assertions that his own work was of no account and inferior to that of his neighbor. It was clear that the self-abnegation so heavily demanded by their culture took on a kind of competitive aspect, that the children were outstripping each other in their abject denial of their own proficiency.

It is in this sense, then, that the behavior patterns of different people with different normative cultures are not nearly so different as the ethnographer has made out. It seems to me we may take either of two positions. The first would be that the enthnographer has tended to misrepresent the character of the culture. The other, and the one to which I subscribe, is that the culture as a normative system does in fact differ as described, but that people everywhere fail to live up to their own norms—whether for good or evil—and that, therefore, the behavioral modes tend toward the center.

This distinction seems to me of particular importance in the light of modern types of anthropological inquiry, in the light of anthropology as a science of culture, as distinct from anthropology as a science of custom. We have already made reference to Benedict's *Patterns of Culture*. It seems to me that the important point with respect to this work, as with her *Chrysanthemum and the Sword*, is that Benedict was talking about culture in the normative sense. Perhaps she indulged in more literary license than circumstances justify, but certainly what she has to say that is valid applies to culture as I have used it in this paper. Indeed, the anthropological effort to make some kind of analysis of Western culture fits the picture quite well. The endeavors by Mead and Gorer must be seen in actuality to be presentations of the normative culture and not of the norms of behavior of the population. For instance, when Margaret Mead speaks of Americans as "all being third generation," she is engaging in a literary hyperbole with no statistical validity. She is—or at least she may be—making a statement with generic validity about urban, middle-class American culture, while at the same time she makes the demographer shudder.

Another theoretical point which emerges when we examine the distinction between the cultural normative and the statistical norm is that it allows us to separate the science of anthropology and the science of sociology on a more reasonable basis. For just as the anthropologist has given priority to the cultural, the sociologist has directed his attention to the behavior of the individual. Whereas Brown will say that the Andamanese believe or do a certain thing, the sociologist will count and proportion the individual responses to a questionnaire or examine the frequency of divorce. To be sure, the anthropologists with their relatively homogeneous communities neither can nor need to be so much concerned with statistical problems and behavior differentials, while the sociologist dealing with large and complex social entities is naturally drawn to making distinctions within his populations, with their heterogeneous cultural background, by counting and proportioning. Yet, in this natural basis for the distinction between the two disciplines, there is a tendency to forget that the basic subject of the discourse is in fact a different one and that culture and behavior are different things.

It is also true that a generation of institutional support for interdisciplinary research and the growth of combined departments and joint appointments has tended to obscure the actual difference on this score. Thus it is that the work of Lloyd Warner appears to be sociological rather than anthropological, while Robert and Helen Lynd in their first study of Middletown tend to present the cultural norms. We are, it might be said, speaking of the normative aspects of the discipline and not of the actual behavior of each anthropologist and sociologist. The point is not to preserve some kind of false distinction but rather to make explicit the manifest differences in the subject of inquiry. For a failure to appreciate this distinction often leads to misunderstanding and rejection.

From what I have already said, it may be presumed that I feel that the study of the normative is somehow less respectable or less rewarding than the study of actual behavior. If I have left this impression, it is a false one. It would be like saying that the study of matter is more legitimate or better than the study of energy. Both enterprises are necessary and legitimate.

Furthermore, both are equally "real." If I were to cast aspersions at all in this paper—and that is not my intent—it would be toward the "hard" scholars who insist on making observations of behavior on the assumption that they are studying reality, and who decry the study of cultural norms as being things of the imagination. The fact is that norms are very real. It has been twenty years since an informant described to me the workings of a secret society in a defunct tribal culture of California Indians. I never discovered whether the things he told me actually did take place, but it was quite clear to me that that was beside the point. The fact that he and members of his society believed them to take place was a social fact of primary importance; it influenced the behavior of the members of the tribe. It is in this sense too that the study of national character has validity and importance, even though it can be shown with great ease that the population in the nation in question does not universally conform to the pattern of cultural expectations.

There is another aspect of this distinction between the normative and the characteristic in behavior: it enables us to study the relationship between the two. This is a very important arena of inquiry which has received growing attention by students in the field. We have already suggested that we can compare two cultures of very different normative behavior and show the similarities in actuality. It is equally important to understand the internal stress between behavior and norms. I think something of the kind was in the mind of Robert Lynd[8] when he set forth conflicting values in our own culture, for most of these can be resolved between cultural ideals and recognized reality. Modern studies of kinship and marital practices frequently analyze and treat with this difference; its importance for cultural change, for internal social stress, and for psychological tensions seems to me to be obvious.

Perhaps, if we gather sufficient information on the relationship between the normative and the behavioral, we will be able to triangulate on the knottiest problem in the science of man: human nature. Earlier we avoided this phrase, for human nature is never free of cultural influences. But we cannot escape the interest in man's natural tendencies. If our distinction is valid, however, we are afforded a means of showing what these natural tendencies are and with which culture endeavors to cope. Is man "naturally" competitive, or are Kropotkin and his more recent disciples right in saying such is alien to his basic nature? Enough cases of cultural self-abnegation together with latent competitiveness—as both Li An-che and the story from Adair suggest—might give us a better understanding of the natural in human nature. That natural element is obscured by cultural dictates, but there is reason to believe that it asserts itself when the culture demands too extreme or too narrow a pattern of behavior.

It has been my central contention that there has been a confusion between culture and behavior that has resulted from the change in the character of the anthropological enterprise. Armed as we were with the concept of culture, we moved into areas of inquiry which required an understanding of frequency of occurrence of actual behavior. The failure to conceptualize the distinction between the two and to recognize the legitimacy of each has resulted in confusions leading to theoretical error. The nature of human activity in the context of the biological drives and the environmental determinants and the characteristic forces of social life means that actual behavior will tend to meet these needs, that it will tend to breach the bonds of restriction that culture endeavors to impose upon it. Therefore, I close as I began by asserting that culture as a systematic set of norms for behavior, will show greater variation as one goes from one place on the earth to another than will the actual normal practices of the populations who profess these different cultures.

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Notes

1. A. R. Radcliffe-Brown, *The Andaman Islands*.

2. Max Gluckman, *Legal Procedure of the Barotse*.

3. Jacob Fried, "The Relation of Ideal Norms to Actual Behavior in Tarahumare Society."

4. Kroeber and Kluckhohn, 1952 : 181. Italics supplied. We are placing emphasis on the point at issue, without questioning that culture has historical, psychological, sociological and biological dimensions. We would not want it to be said that we place the emphasis upon norms as distinct from, say, the historical, since these are not contrastive aspects.

5. Linton (1945 : 43 ff.) uses this term and distinguishes it from cultural constructs (both the natives' and the ethnographer's). But he treats the construct as something unreal, and fails, to my way of thinking, to use the difference he has expressed.

6. Helen Codere, The Amiable Side of Kwakiutl Life.

7. Li An-che, Zuñi: Some Observations and Queries.

8. Robert Lynd, *Knowledge for What?* pp. 60–63.

ANTHROPOLOGY AND ART

H. D. Gunn

I

This paper was envisioned as a modest sort of reckoning up; it was desired in effect to establish at least roughly where anthropologists had arrived at in the sector of art specifically. It was understood clearly from the outset, however, that any such study would have important implications along a very broad front, so to speak; it is doubtless a truism that our understanding of culture as a whole, of society as a whole, is no more advanced than our understanding of the most obscure element, or sector, of the one and the other, but it is a point not always explicitly recognized by anthropological writers, unfortunately.

A period spent by the writer in West Africa following professional training in effect prompted this investigation into the field of art. In Nigeria specifically, the writer became aware of behavioral and artifactual traits either entirely unfamiliar, not reported in the literature, not represented in museum displays from any part of the world, or unfamiliar largely because of their unfamiliar, their living, context, traits which became meaningful, however, in the fullest sense, only when classified as art. The question immediately presented itself, naturally, whether anthropologists had not been too culture-bound with respect to the category *art* to do justice either to the category or to a considerable part, perhaps, of the world's peoples, and thereafter the question, to what extent our means of dealing with—our understanding of—culture as a whole may have been restricted, as it were, by this failure.

The literature dealing with the subject of art was accordingly consulted, primarily, be it understood, that of the past thirty years, or since the publication of what one reviewer has characterized as "the classic and also the only book-length analysis of primitive art as a whole" (Nelson, 1954), that is, Boas's *Primitive Art* (1927). First of all, works of a theoretical character dealing with art as a part of culture in the anthropological sense, by professing anthropologists, but also, in order to gauge the accomplishment of anthropologists in this period, the published works of artists, including teachers of art, of art-critics, -historians, and -philosophers, chiefly the works of U.S. writers, and above all anthropological text-books, because of their crucial position with respect to the development of the discipline and of its various ramifications, but also the works of writers of various other nationalities. Then on to descriptive works, monographs dealing with regional art, those dealing with the art of socio-cultural strata (primitive art, folk art), and, finally, ethnographic monographs dealing with the art of individual societies more or less in context. So substantial did this literature prove to be that sampling—without apology, but with considerable regret—was unavoidable in the limited time available.

It should be noted here that, in addition, occasional reference was made to museum displays in the course of this study, and to exhibition catalogs collected here and abroad over a period of some years.

II

The results of this pleasurable regimen can be stated briefly. Probably in no other field have anthropologists contributed so little, notwithstanding paragraph upon paragraph, page upon page, chapter upon chapter, and a goodly number of volumes by anthropologists dealing with art; and in no other do they appear to be more complacent with their achievement. In fact, quite as much, if not more, insight into the realm of art specifically and into the relationship between art and the rest of culture can be gained from pocket editions of the writings of such critics, historians and philosophers as Cassirer (1953), Langer (1948), Read (1949, 1955), and Talbot Rice (1939), though from the narrowly anthropological point of view these writers have painful limitations, at times, and their works are rarely immediately useful, as it were.

As for the anthropologists, those of the United States on the whole have been prone to begin any discussion of art with a definition of the term, concerned as it were explicitly to establish a universal category; those of other nationalities appear somewhat more inclined to assume that their readers know what they mean by *art*. The two procedures are, however, by no means so divergent as at first glance they might appear: U.S. anthropologists in fact conform to the definition of art offered by standard dictionaries of English, while, so far as anyone can tell, others have merely taken for granted widely current definitions of the equivalent term in their respective languages—*art*, *arte*, *Kunst*, and so on.

Archaeologists, it may be noted in passing, universally have failed, in their interpretive essays, to define art, but by strong implication they subscribe to a rather narrower definition than do most general anthropologists even when dealing with the same (particularly prehistoric) materials. Thus Bunzel explicitly pushes the dawn of art back to the middle of the Paleolithic, presumably with the retouched Acheulean core-biface tool in mind (1938, p. 543), which Weltfish simply labels sculpture (1953, pp. 19–20, 225); on the other hand, Braidwood, like Gordon Childe, is reluctant to go beyond the figurines of the Aurignacian, searching for the beginnings of art (Braidwood, 1951, p. 74; Gordon Childe, 1942, pp. 42–3).

In the U.S., the definition of *art* proposed by Boas gained immediate acceptance among anthropologists, and with some refinement over the years has been repeated by virtually every anthropologist touching upon the subject. Boas was somewhat discursive in his definition, and not entirely explicit, but it would appear that the essence of his definition transpires from the following passage:

> Rhythmical movements of the body or of objects, forms that appeal to the eye, sequences of tones and forms of speech which please the ear, produce artistic effects. *** We may also speak of impressions that appeal to the senses of smell, taste and touch. A composition of scents, a gastronomical repast may be called works of art provided they excite pleasurable sensations. *** When the technical treatment has attained a certain standard of excellence, when the control of the processes involved is such that certain typical forms are produced, we call the process an art, and however simple the forms may be, they may be judged from the point of view of formal perfection; industrial pursuits such as cutting, carving, moulding, weaving, as well as singing, dancing and cooking are capable of attaining technical excellence and fixed forms (Boas, 1927, reprinted 1955, p. 10).

This definition was not significantly altered by Boas when three years later he came again to the subject of art (Boas, 1930, pp. 89–93), and has been paraphrased, in effect, by a whole generation of writers, including Bunzel (1938, *passim*), Jacobs and Stern (1947, pp. 211–15), and, most recently, Beals and Hoijer (1953, pp. 538–41, and 567–8), but perhaps never in more explicit terms than by Herskovits (1948, pp. 378–80), who may be allowed to speak for all:

> . . . In the . . . societies of Euroamerican culture . . ., we are . . . confronted with the effect of compartmentalization. . . . By drawing definitions too finely, we tend to shut out many significant manifestations of a phenomenon. . . . *** In the analysis of art, when we differentiate "pure" from "applied" art, we . . . restrict the play of our aesthetic appreciation. *** In the widest sense, . . . art is to be thought of as *any embellishment of ordinary living that is achieved with competence and has describable form.* *** . . . Any manifestation of the impulse to . . . heighten the pleasure of any phase of living that is so recognized by a people, must be accepted by the student of culture as aesthetically valid, and is, in consequence, to be given the designation "art."

The meanings of words manifestly change. It may accordingly be suspected that ideas attributed to any author writing in the past, even, sometimes, the very recent past, comprise some of the most genuinely revolutionary innovations of which mankind is capable. However by this definition, as originally set down by Boas, and as embellished—to borrow Herskovits' term, used most notably, perhaps, by both Bunzel (1929, p. 1) and Lowie (1934, p. 177) before him—by his followers, it would appear necessary to conclude that every human activity is in fact to some degree an art, a conclusion which one may draw equally from the dictionaries!

III

What traits, then, behavioral and artifactual, are seriously treated as art by anthropologists? In fact, there is a troubling discrepancy between the accepted definition and the very narrow range of activities which anthropologists will assign to the category and analyze as art. Boas, indeed, suggests that a perfume or a meal may qualify as a work of art, but in his alleged analysis of "primitive art as a whole" he stops short on the one hand with what may be lumped together as the graphic and plastic arts, and, on the other, with literature, music and dance—in proportions, it may be noted, subject before now to sharp criticism. The same can be said of the vast majority of writers on the subject, including virtually every author of a monograph including the term art in its title; in practice, U.S. anthropologists have stood shoulder to shoulder with their colleagues abroad, culture-bound, even, it may be suggested, "class"-bound, so that the rather stinging pronouncement by the late, sincerely lamented Professor Griaule, in a work promisingly entitled *Arts de l'Afrique noire*, may be taken as typical:

> *Bien entendu, quand nous parlerons de l'art noir, nous serons obligés de restreindre singulièrement le champ de nos observations. Dans le présent travail nous le limiterons à une certaine espèce de produits matériels, notamment au masque, à la statuaire et à des aspects particuliers des activités qu'ils supposent (peintures rupestres, danse, cosmétique). Nous laissons délibérément de côté une masse énorme de manifestations et de préoccupations esthétiques* (1947, p. 10).

For reasons of pure expediency, the reader is left hopelessly to suppose. No writer canvassed in the course of this study, in fact, retreating from a comprehensive definition to more or less narrowly restricted, arbitrarily selected examples, makes quite explicit his reasons for doing so.

On the other hand, it may be recorded that the authors of countless ethnographic monographs have described a very wide variety of activities in such a way as to lend support to the accepted definition. But these writers too, even when they apply the term "art"—as by a lapse—to this or that process or activity, will analyze seriously as art only a restricted number of activities.

It is also worth noting that all writers regard art in our own culture as something apart, an activity of specialists denied as an outlet, so to speak, to most. Herskovits, for example, states (1948, p. 378), "To understand how closely integrated with all of life, and how expressive of a way of living art can be, is . . . not easy for us who live in the highly specialized societies of Euro-american culture."

Because of what can most generously be termed this absentmindedness of anthropologists in failing as it were to substantiate their definition, then, it would appear unprofitable to treat extensively here what they conceive to be the constituent elements of art. Anthropologists are, indeed, expansive on this very subject, for example, Jacobs and Stern, cogently arguing for four universal "features" of art (1947, pp. 211–12), and Bunzel, with deftness dissecting *form* into a further four elements (1938, pp. 558–9); but the fact remains that these various elements have been derived apparently with reference solely to the graphic and plastic arts, literature, music and dance, no other, and have yet to be proven meaningful in the analysis of, say, perfumes, or foods.

IV

When the fact is finally grasped that the art of no people has, then, been more than barely examined in some small part, it is not surprising that one has far to seek a coherent discussion of the manner in which the totality of art gears in with the other aspects of culture. One looks in vain to the writings of Boas, who, as many have noted, "was interested in dealing with culture, not in systematically theorizing about it" (Kroeber and Kluckhohn, 1952, p. 151). Boas declares that art is most intimately associated—though in his view not necessarily universally, one gathers—with religion, ethics and science, all four comprising together the "subjective" aspect of culture—language, be it noted, constituting for him a phenomenon analytically quite distinct—but he fails to demonstrate conclusively his dictum (1930, pp. 73 and 79). Similarly, one is frustrated by the writings of his disciples, who seem reluctant to go even as far as Boas, though Benedict in *Patterns of Culture* (1946, *passim*), it may be inferred, shows definite interest in the problem.

Neither is the clue to be sought in museums, and certainly not in the lavishly pictorial *genre* of publication so intimately related to the exhibition hall that Malraux has designated it a "museum without walls" (Malraux, 1953, pp. 11–127); both provide only more or less ragged distortions of art as defined, though, ironically, the ethnographic museum has rather more to say about art than the so-called art museum, at least potentially. All, it may be concluded, are far too much part of our own culture to serve adequately to bridge the gap between cultures, or, perhaps more aptly, to breach the barriers; in both these media anthropologists—who are ideally men of two worlds at the very least!—adopt what appears to be a wholly passive role.

Boas does indeed show how, in this or that culture, basketry, or sculpture, and the like gears in with this, that, or the other thing—not very systematically, at that. And his followers have, as it were by definition, done much the same, as has done virtually every other anthropologist touching upon the subject, whatever his background. The result of such treatment, however, is a sort of compartmentalization—the sealing-off of whole cultures—which proves ultimately as much a curb—to the cultural scientist, if not, perhaps, to the practicing artist—as that against which Herskovits has warned.

It is John Gillin who has provided anthropologists with the means of appreciating, the universal relationships between art and other aspects of culture, in his work of 1948, *The Ways of Men.* Gillin analyzes culture into *mental* and *behavioral* customs, and the latter in turn into *actional* and *representational* (1948, pp. 182–4, 314–15); it is probably possible now, nearly ten years after their initial formulation, to refine Gillin's definitions of all these terms, but the writer still finds meaningful the distinction implicit in Gillin's examples—the customs of eating cited as actional, and "art activities" as representational, the one utilitarian primarily, and the other symbolic, the difference between them being one of degree, or of emphasis, as becomes clear when he states:

> All overt or behavioral customs and the artifacts associated with them may be viewed in a double aspect: (1) from the point of view of their utilitarian functions in the actual cultural or social situation, and (2) from the point of view of their symbolic functions. In the representational type of custom . . . the symbolic function is the more prominent and in some cases the utilitarian function is almost indiscernible. *** If we turn to other customs . . . that are usually not thought of as representational in any sense, we usually discover after intimate acquaintance with the culture that they, too, carry a symbolic load. . . .

Clearly, Gillin—who most notably fails to define art—calls for a kind of translation of the definition offered by Boas, rather than a mere rephrasing; for "pleasurable embellishment" one may substitute "meaningful embellishment," perhaps, for example. This represents of course a fundamental difference in viewpoint, for the esthetic drive as such was apparently dear to Boas.

One dares suggest as a further possibility, *pour épater les savants*, that the definition may require further qualification, as it were, specifically with respect to religion and also language, in the light of Gillin's analysis (cp. Langer, 1948, pp. 103–9): religion to be defined as—among other things which need not be touched upon here—a system of mental patterns, including that abstraction language, of which the observable evidence, so to speak, is art (considered as a quality of activity, here, rather than of product), any treatment of art to be understood to extend to the full range of human activities, not excluding speech and ritual. Actually, the position is hardly revolutionary; indeed, the older generation may feel justified in calling it reactionary (cp. Lowie, 1924, pp. 359–60).

To this writer, this revised approach not only to art but also to religion has immediate appeal on account of its very neatness, tying up loose ends that have been let dangle for a generation—though its practical usefulness in both managing ethnographic materials and communicating them must surely be proven. Especially it must be said, however, that Gillin, by equating symbolism with art, in effect, and by underlining "the general symbolic function of overt customs and of artifacts" (1948, p. 465), appears to this writer only to be pressing the ethnographer in the field to realize more fully the potentialities of Boas's definition. In this, it may be concluded, Gillin has proved true to anthropology as anthropology, an intersocietal institution the characteristic function of which is to link man's societies and cultures, anthropologists serving as media of culture-contact and -change, not in one direction, but in all. Whatever the situation in such other fields as economics, political organization, and law, in which a number of anthropologists have distinguished themselves by quite positive contributions, other anthropologists than Gillin dealing with art in the past generation have served as little more than purveyors of myth validating the charter of the institutions of "art" in our own society (or, more likely, a restricted stratum thereof), however remote from their aim this role may have been.

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ZUM PROBLEM DER KONSTANZ IN DER ETHNOLOGIE

Josef Haekel

Beharrung und Wandel, die beiden elementaren Triebkräfte im Kulturleben der Völker, bilden in ihrer eigentümlichen Ambivalenz Hauptprobleme der völkerkundlichen Forschung. Jedes Volk ist, so paradox es klingen mag, sowohl konservativ als auch auf Neuerungen eingestellt. Das Kräftespiel dieser menschlichen Grundstrebungen variiert in seinen Wirkungen allerdings sehr und zwar je nach allgemeiner Kulturartung, Volkscharakter, Kulturelementen, Kulturaspekten and Zeitepoche. Im Grunde genommen sind wohl die meisten Kulturschöpfungen auf Dauer gerichtet, wie lange jedoch ihre ursprüngliche Prägung und Bedeutung erhalten bleibt, ist eine andere Frage. So schrieb M. Herskovits in seinem Buch "Man and his Works" (1948): "Cultural change can be studied only as a part of the problem of cultural stability; cultural stability can be understood only when change is measured against conservatism." Herskovits bemerkte ferner, dass Kulturwandel viel leichter zu erforschen sei als das Moment der Beharrung. Dies mag vielleicht einer der Gründe sein, dass, wie William Fenton, auf gegenwärtige Interessensrichtungen Bezug nehmend, hervorhob, "ethnologists are preoccupied with the process of culture change and not paying enough attention to stability." Die heute in der "Anthropology" so sehr im Vordergrund des Interesses stehenden Akkulturationsstudien beziehen sich bekanntlich hauptsächlich auf den gegenwärtigen Kulturwandel. Die Bedeutung dieser Untersuchungen bedarf wohl keiner näheren Begründung. Die gleiche Berechtigung muss aber auch den Forschungsinteressen jener Ethnologen eingeräumt werden, die von heutigen Gegebenheiten aus frühere Kulturformen und Zustände zu rekonstruieren trachten, wie hypothetisch die Ergebnisse im einzelnen manchmal auch noch sein mögen. Zum vollen Verständnis der Gegenwart kann man nur durch die Erschliessung der Vergangenheit gelangen. Dass unter dieser Rücksicht das Konstanzproblem von besonderer Wichtigkeit ist, liegt auf der Hand.

Konstanz äussert sich in verschiedener Weise. Beharrung als allgemeine Haltung kann im Kulturtyp vorliegen, wie es in der Lebensform des Jägertums, das sich vom Paläolithikum bis heute erhalten hat, zum Ausdruck kommt. Beharrung kann sich auch nur auf einzelne Kulturaspekte oder Kulturelemente beschränken. Die von Ralph Linton bei Kulturerscheinungen getroffene Unterscheidung zwischen Form, Bedeutung, Verwendung und Funktion ist auch für das Konstanzproblem von Bedeutung. So kann z. B. die Form eines Kulturelementes stabil bleiben, während sich Bedeutung und Funktion jeweils ändern. Oder ein Volk schafft eine neue Form, gibt ihr aber Bedeutung und Verwendung der früheren Form.

Schwierig gestaltet sich für gewöhnlich der Nachweis, inwieweit eine Anzahl von Elementen lange Zeit hindurch stets in derselben Vergesellschaftung oder Kohäsion verbleiben. Es handelt sich hier um die Frage der Konstanz eines Kulturkomplexes. In diesem Zusammenhang sei auf die Kulturkreise F. Graebners und W. Schmidts hingewiesen. Ihnen lag eine unrealistische Fassung des Beharrungsprinzipes zu Grunde. Der Kulturkreis, wie er von W. Schmidt formuliert wurde, beruhte nämlich auf der Annahme einer schier unbeschränkt langen Dauer einer kulturellen Ganzheit, in der nicht nur Einzelelemente sondern auch ihre Integration sich wesentlich unverändert Jahrtausende hindurch erhalten hätten. Hiefür fehlen jedoch jegliche Belege und die empirischen Grundlagen. Die Kulturkreisschemata wurden von fast allen Vertretern der historischen Ethnologie, soweit sie sie überhaupt angenommen hatten, schon seit langem aufgegeben. Mit der Ablehnung des Konstanzbegriffes im Sinne der Kulturkreise ist jedoch nichts gegen die Möglichkeit langdauerenden Zusammenhaltes von Elementgruppen gesagt. Ebensowenig wird, wie W. Koppers immer wieder darauf hingewiesen hat, durch die Ablehnung der Kulturkreise die grundsätzliche Berechtigung der historischen Methode in der Ethnologie mit ihrer Beziehungsforschung tangiert.

Im Zusammenhang mit dem Beharrungsmoment sei auch auf die sogenannten Universalien hingewiesen, jene grundlegenden Elemente also, die allen Völkern gemeinsam sind. Hiezu zählen z. B. Familie, Inzesttabu (das nur bei wenigen Völkern für bestimmte Belange institutionell aufgehoben wird), Eigentumsbegriff, ethische Grundnormen, Religion, Feuergebrauch u.a.m. Diese Universalien haben aber mit dem Konstanzprinzip eigentlich nichts zu tun. Da sie für die Realisierung der fundamentalen physisch-psychischen Wesensart des Menschen unerlässlich sind und existenzielle Bedeutung haben, ist bei ihnen die Frage nach Konstanz und Wandel, wenn wir von ihrer konkreten Ausformung absehen, irrelevant.

Zur methodischen Erfassung des Beharrungsmomentes im Kulturleben kommen meines Erachtens folgende Wege in Betracht: 1) Verbreitungs- und Vergleichsstudien. 2) Lokalgebundene Kulturanalysen. 3) Ethnohistorie. 4) Studium der Wechselwirkungen zwischen Hochkulturen und sogenannten Primitivkulturen. 5) Korrelation zwischen ethnographischen und prähistorischen Daten.

1. Bei Verbreitungs- und Vergleichsstudien handelt es sich darum, festzustellen, ob analoge Kulturerscheinungen auf historisch-genetische Zusammengehörigkeit beruhen. Dies nach den Worten von John Bennett (1944, S. 165) ausgedrückt: "... if two sets of phenomena, widely separated in space, show significant formal and/or functional similarity, it can be assumed that they have a common origin." Oder wie R. Lowie (1947, S. 377) formuliert hat: "The point is ... to group together what is alike in essentials and not to be captivated by outward appearance." Können also Kulturerscheinungen in ihrer regionalen oder interkontinentalen Verbreitung auf einen gemeinsamen Ausgangspunkt zurückgeführt werden, so wäre ihnen grundsätzlich betonte Konstanz zuzubilligen, zumindest in ihrem Wesenskern oder in ihrer Grundidee. Im einzelnen muss jedoch auch auf Charakter und Funktion des betreffenden Elementes, die Art seiner Verbreitung und andere Momente Bedacht genommen werden. Eine Sonderstellung kommt bei der Frage nach Konstanz dem Gesellschaftsleben zu und zwar im Hinblick auf die Wechselwirkung zwischen Sozialformen und Sozialstrukturen einerseits, ökonomischen, demographischen und biologischen Faktoren anderseits. Es eignen sich daher hier weiträumige Vergleichsstudien zur Erfassung von Konstanz nur im beschränkten Ausmasse. Eine weitere Frage ist, inwieweit die Beharrung von "trait-complexes" nachgewiesen werden kann. H. Manndorff (Wien) z. B. hat eine betonte Konstanz von Saat- und Ernteriten halbnomadischer Brandrodungsbauer Indiens, Stämme, die weit getrennt von einander wohnen, wahrscheinlich machen können. Die Ausbildung dieses

Ritualkomplexes muss in die Zeit vor den Einwirkungen indoarischer und dravidischer Sprachträger zurückreichen. Milovan Gavazzi (Zagreb) wies neuerdings auf die jahrtausende lange Beharrung des Wanderhirtenkomplexes, der Transhumantes, auf dem Balkan hin, von dem eine Reihe von Einzelzügen bis heute weiterleben.

2. Ein weiterer Weg zur Erfassung des Beharrungsmomentes sind lokalgebundene Kulturanalysen, d.h. ein intensives Studium von Einzelkulturen mit eingehender Berücksichtigung der Strukturen, Funktionen und Wertsysteme, des Spannungsverhältnisses zwischen Individuum und Gemeinschaft sowie zwischen den Generationen. Dabei ist auch zu achten, wie Innovationen jeweils aufgenommen werden. A. J. F. Köbben (1956) hat bei den Agni und Ashanti Westafrikas beobachten können, wie zäh an der traditionellen matrilinealen Erbrechtsordnung festgehalten wird, obwohl sie speziell unter den gegenwärtigen ökonomischen Gegebenheiten nur mit grossen Schwierigkeiten und tiefgehenden emotionalen Spannungen aufrechterhalten werden kann. W. Fenton (1953) wiederum konnte zeigen, wie sich bei verschiedenen Indianerstämmen Nordamerikas die Beharrung an kulturellen Grundhaltungen und Strukturprinzipien auf den Prozess der Akkulturation jeweils verschieden auswirkte.

Eine besondere Art des Konstanzproblems bilden die Survivals. Darunter versteht man bekanntlich Kulturerscheinungen, die aus früheren Epochen der Volksgeschichte erhalten geblieben sind und gewissermassen wie Fremdkörper aus dem Rahmen des gegenwärtigen Kulturgefüges fallen. Es bedarf jedoch, wie schon F. Graebner hingewiesen hat, eingehender Untersuchungen im Sinne lokalgebundener Kulturanalysen und Vergleichsstudien, um zu erheben, ob es sich im Einzelfall wirklich um Survivals handelt und nicht um übernommene Fremdelemente, die noch nicht integriert wurden.

3. Ethnohistorische Untersuchungen zur Erfassung des Konstanzproblems beinhalten im wesentlichen die Auswertung schriftlicher Nachrichten oder datierter Quellen über Völker und Kulturzustände aus verschiedenen Zeiten sowie die kritische Verwertung von Traditionen. Eine wertvolle Quelle stellt in dieser Hinsicht z. B. Herodot dar. Manche der von ihm berichteten ethnographischen Daten aus dem pontisch-westasiatischen Bereich geben interessante Hinweise auf kulturelle Beharrung. Um von verschiedenen ethnohistorischen Studien aus der letzten Zeit nur ein Beispiel anzugeben, sei auf das Werk von A. Paul, A History of the Beja Tribes of the Sudan (1954) hingewiesen. Hierin konnte bei den Beja-Stämmen im Osten Aegyptens in wesentlichen Kulturbelangen betonte Konstanz festgestellt werden. Dabei sei bemerkt, dass diesen Stämmen für die Beurteilung der Kamelnomaden Nordafrikas mit ihren maternalen Sozialtendenzen besondere Bedeutung zukommt.

4. Das Studium der Wechselwirkungen zwischen Hochkulturen und Primitivkulturen stellt in Bezug auf das Konstanzproblem einen weiteren wichtigen Weg dar. Hier ist zu klären, welche Einflüsse von Hochkulturen auf Völker mit geringerer zivilisatorischer Ausrüstung ausgegangen sind, ferner, welche Elemente von primitiveren Substratkulturen sich in den Hochkulturen erhalten haben. Es bedarf wohl keiner besonderen Erwähnung, dass bei Vorhandensein von Schriftquellen und datierten Denkmälern die Erfassung der Konstanz wesentlich erleichtert wird. Wenn nun ein- und dieselbe Kulturerscheinung sowohl in bestimmten Phasen von Hochkulturen als auch in Primitivkulturen des näheren und weiteren Umkreises vorhanden ist und ihre Entstehung in der betreffenden Hochkultur nachgewiesen werden kann, dann

erscheint nicht nur der Verbreitungsweg des Elementes klargelegt, sondern es wird auch die genauere Fixierung seiner Beharrungsdauer ermöglicht. Es sei nur auf einen konkreten Fall hingewiesen. W. Hirschberg (Wien 1955) hat gezeigt, dass der für die Negerstaaten weiter Teile Afrikas so charakteristische Zwangstod des Königs (der sogenannte rituelle Königsmord) von Meroe, dem wichtigen Kultur- und Handelszentrum im Süden Aegyptens ausgegangen sein dürfte. Die in Frage stehende Institution erscheint jedenfalls für Meroe, einem Ableger der altägyptischen Hochkultur, schon in vorchristlicher Zeit bezeugt. Die Ausbreitung dieser Einrichtung wird zusammen mit einer bestimmten Staatsidee wohl nach dem Untergang von Meroe im 4. Jahrh. n. Chr. erfolgt sein. Die prinzipielle Konstanz der rituellen Königstötung in Afrika erscheint somit in einem gut belegten chronologischen Rahmen. Wichtige Aufschlüsse über die Frage der Beharrung spezifischer Hochkulturelemente und ihre chronologische Fixierung brachten die Studien R. Heine-Geldern's über die vorkolumbischen Kulturbeziehungen zwischen Alter und Neuer Welt.

5. Die Korrelation ethnographischer und prähistorischer bzw. archäologischer Daten ermöglicht in besonderer Weise, Beharrung und deren Dauer wenigstens für bestimmte Kulturbelange zu erfassen. Wichtige Untersuchungen liegen dies bezüglich besonders von Nordamerika, Nord- und Zentralasien und Australien vor. Ein seltener Fall für die Möglichkeit einer Korrelation von Traditionen mit prähistorischen Straten bietet sich bei den Papago-Indianern des südlichen Arizona. Ihre lange Ursprungsmythe, in der historische Reminiszenzen enthalten zu sein scheinen, kann mit der fast lückenlosen prähistorischen Schichtenfolge des Gebietes, die vom Endpleistozen bis in die voreuropäische Zeit reicht, in gewisse Beziehung gebracht werden. Es scheint sich daraus zu ergeben, dass die besondere Prägung der Kulturheroengestalt der Papago mit dem Aufkommen der prähistorischen Hohokam-Kultur um Chr. Geb. zusammenhängt und sie somit eine Konstanz von rund 2000 Jahren beanspruchen könnte. Wie hypothetisch diese Deutungen auch sein mögen, die aus dem besonders gelagerten Fall des Papagogebietes sich ergebenden Indizien sind jedenfalls im Auge zu behalten.

Abschliessend seien noch einige Hinweise auf Faktoren gegeben, die Beharrung und Wandel bedingen oder bewirken können. Herskovits (1955, S. 451, 453) äusserte sich hiezu folgend: "Conservatism and Change in culture are the result of the interplay of environmental, historical, and psychological factors." ". . . it is these ever-differing historic streams that at once reflect and shape the attitudes and points of view of societies that, in the final analysis, determine the degree to which each will be hospitable or hostile to innovations." Die psychologischen Beharrungs- und Wandlungsfaktoren liegen im besonderen in der Wirksamkeit markanter Persönlichkeiten, in den Spannungen zwischen Individuum und Gemeinschaft, zwischen alter und junger Generation sowie in den Wertsystemen. Zur Beharrung können auch führen einfache Kulturartung mit betonter Selbstgenügsamkeit und innerkulturlicher Spannungsarmut, wie sie vor allem Jäger- und Sammlervölkern eigen sind, ferner organisierte Traditionsübermittlung, Ritualisierung von Einrichtungen und schliesslich Momente, die im inneren Wesen von Kulturerscheinungen liegen. Im besonderen Ausmasse trägt die Gerontokratie zur Beharrung in verschiedenen Kulturbelangen bei. Doch können anderseits gerade die alten Männer kraft ihrer sozialen und rituellen Machtstellung die Tradition unterbrechen und Neuerungen einführen. Ein stark stabilisierend wirkender Faktor ist schliesslich bei Vorhandensein einer Schrift gegeben, doch erfolgt, wie wir wissen, in

Hochkulturen die integre Weitergabe der Tradition auch ohne schriftliche Fixierung.

Es wäre an der Zeit, dass das Konstanzproblem bald eine ähnliche systematische Behandlung erfahre wie sie bereits H. G. Barnett (1953) für die Innovation durchgeführt hat. Konstanz und Wandel sind die beiden Momente, die den historischen Grundcharakter der Ethnologie bestimmen.

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A MICROCULTURAL ANALYSIS OF TIME[1]

Edward T. Hall, Jr.

During my career as an anthropologist I have been continually impressed by the extent to which culture patterns and molds behavior in unconscious ways.

I am sure all of you have had, many times, the experience of talking about culture to the layman only to have almost every word you have said misunderstood or distorted. This experience, repeated enough times, poses very real problems for the anthropologist. He finds himself feeling somewhat as Columbus must have felt when he was trying to persuade his crew that the world was round when their experience and senses told them it was flat. I began to have to face this problem in a very real way when I directed a training program for Point 4 technicians going abroad. The culture concept taken seriously made them anxious; instead, they wanted to hear about the "strange customs" of the foreigner. They also wanted to be told how to avoid offending—simple rules: like not handing Moslems things with the left hand. Quite correctly they did not want to hear about culture in the abstract.

At that time we were having great success teaching foreign languages to these same Americans, using techniques developed by the anthropologist or descriptive linguist. The high degree of success of the linguists and the rather poor showing of the rest of the anthropologists raised the question as to whether there wasn't something to be learned by comparing the methodologies of the two fields. Implicit in this was the great progress that had been made in recent years by those who followed in the footsteps of Sapir, Bloomfield, and Whorf.

If languages were a part of culture, and could be taught so that people could speak with little or no accent, why could not the rest of the culture be analyzed in such a way so that people could learn by doing and thereby remove the accent from their behavior? The key was the isolating out of cultural systems which could be treated analogously to language.

Another contribution of linguistics was the very great stress laid on paying attention to details of structure and the importance of studying very small things first. The work of Benjamin Lee Whorf is an excellent example of how a properly detailed analysis can produce startling results in the interpretation of over-all patterns.

Analysis of the smallest details of culture, involving discrete cultural systems, has been termed the study of microculture. Briefly, a microcultural analysis differentiates between three classes of events: sets, isolates, and patterns, which occur in three different contexts: the formal, informal, and technical.

Time permits only a perfunctory description of these concepts. Sets are comparable to words and morphemes; they occur in very large numbers and are by definition those things which people readily observe. Isolates go to make up sets. Like phonemes, they are abstractions and are limited in number. Sets are arranged in patterns, which are also limited in number. Sets out of the context of the pattern have little or no meaning.

Formal culture is traditional and often equated with human nature. Informal culture is largely out of awareness and is thought to have no rules (patterns) governing it. Technical culture is in full awareness and is highly explicit. There are sets, isolates, and patterns for each; that is, there are formal sets, isolates, and patterns, informal sets, and so on.

As a rule, the most difficulty in the cross-cultural experience is had with the informal, as well as with the isolates and patterns. I have chosen "*time*" to illustrate a few of these points—first, because like language, it can be handled; second, it is not too complex at first; third, you can get good comparable material from other cultures; fourth, Americans have trouble with both temporal relationships overseas as do foreign visitors to the U.S. Time represents the type of discrete and basic cultural system that lends itself to analysis and is ultimately reflected also in all other systems of culture.

Time for the Western European is something that is quite concrete and which can be handled. It can be measured, scheduled, earned, saved, spent, and wasted. It is highly valued, important, and geared into our individual society in such a way as to make any other way of handling time seem unnatural and at odds with the system.

The Sioux, on the other hand, have an entirely different way of handling time. I am informed that they have no word for *time* as we know it, no word for *waiting* or *late*. Things happen not when they are scheduled but when there is a state of readiness.

The Trukese have a rather remarkable way of structuring time. On Truk, time does not *heal*; it stacks up like a deck of cards. Long-past events are presented as though they have just happened, and the investigator soon learns to pinpoint unknown events in relationship to known events.

The Navajo's concept of time would seem to fit our treatment of space more closely than anything else. The future is not something tangible, real, and certain and for which you plan: the future can be anywhere. One of the results was that during sheep reductions instituted as a part of a range control program some years ago, the Navajos were not impressed when they were asked to make sacrifices in return for *future* rewards.

Whorf recognized the very different way in which time is handled by the Hopi. Likewise, Bohannon commented on the Tiv and Evans Prichard on the Nuer; both were impressed by the very different way in which time is structured by these two groups as compared with the European.

All of these references have to do with observations on the over-all pattern of time as it occurs in different cultures. Missing from the literature are descriptions of the formal isolates and patterns, as well as the entire range of the informal. Since it is in these two areas that most of the trouble seems to occur, I have chosen a few examples from our own cultures (urban and Eastern seaboard) as well as that of the Middle East as represented in the urban cultures of Syria and Lebanon.

Both Americans and Arabs have run afoul of each other on a number of points where there is conflict between their formal patterns or else where there is little or no overlap. In such cases the other fellow's system just doesn't make sense.

We tend to characterize the Arab as being rather sloppy in the way he handles time. So it may come as a surprise to some of you to learn that we also appear to him to be recklessly unconcerned about a good many things which happen in time—so much so, that large portions of what we say in certain contexts is discounted, and whole situations distorted because of the loose and sloppy handling of our own time references.

On the formal level we take it for granted that time is duration, and that duration can only be measured in terms of two points. Ours is bipolar duration.

Sometimes the two points are not mentioned but they are always there. Sometimes they are rather vague, or on the other hand, they may be stated explicitly. The technical clock provides us with something to hang our informal and formal time behavior on. So that when a man says, "I'll see you at 10:00 o'clock," the person he is speaking to will calculate the projected duration of visit, the relative positions in the business hierarchy, the importance of their business, as well as a number of other items, and will come up with a range of time within which he has to arrive (say 3 minutes of 10:00 to 5 minutes after). If we tell someone to come over for drinks in the evening at about 9:00 o'clock, we are surprised if they arrive at 9:00 and we do not expect them much before 9:20; after 9:40 we find ourselves saying, "I wonder what happened to the So-and-So's?"

The Arab, on the other hand, has a mono-ordinate system, which operates from one clearly defined and very definite point. We face forward and try to structure—that is, schedule—the future as rigidly as possible; they face backward and are more technical than we are about the way certain events are reported to them, and the future is something that is almost impossible to schedule accurately. Man can try but events always trip him up. Our daily schedule, by contrast, calls for appointments that are sealed off from each other by immovable compartments of fixed relative size. With us it's a sign of instability to keep shifting schedules and it makes us very anxious when we have to change appointments around or alter people's places in a projected schedule. The Arab, on the other hand, does not take the schedule as something sacred. He feels it is not right to hold the barriers on a schedule too rigidly. If you are meeting with someone and your business is not yet completed, you prolong it. We do this too, but our pattern emphasizes the *schedule* and their pattern emphasizes the *contents* of the schedule.

As nearly as I can tell, there is a language of time for all people. It requires as much learning as does spoken language. Our pattern, for instance, includes a highly elaborated system of setsmorphotemps (like words) that have certain meanings in different situations. If a boy calls up a girl at 6:00 o'clock and wants her to go out at 7:00, this communicates that either the occasion or the girl or both are not very important to the boy or that they are very close. That is, with us there is always a certain amount of lead time below which you cannot go; otherwise you will be insulting. Also, as you all know, the proper advance notice will signal the importance of the occasion. Students dating for a senior prom will have everything sewed up months in advance. The minimum here seems to be about a month.

The Arab does not make the same distinction as we do along these lines. At the present stage of my knowledge it would appear that lead time as an indicator of the importance attached to an event is much less significant than it is with us and is more a function of how long it takes the two parties to get together. Even for marriages, which we try to schedule well in advance, lead time is more a function of what has to be accomplished by the two families than anything else. One of the things that happens here is that the families have to get to know each other. If cousins marry, less time is required because the families know all there is to know.

In addition to the fact that lead time simply does not mean the same thing to the Arab, their informal pattern places a restriction of about one week on the setting up of all sorts of events; any longer period ahead is likely to be forgotten.

Informally, we have four isolates that enable us to tell the difference between one informal set and another. That is, there are four different components that go to make up the sets on this level. Two of these isolates are not present in Arab culture in the same way that they are in American.

Our informal isolates are urgency, monochronism, activity, and variety. This will become a little clearer if you consider that on the set level we distinguish between eight different degrees of duration, going all the way from *immediately* to *forever*—through very short duration, short duration, long duration, etc. So that a person says, "It took me forever to finish packing"; "I hardly had time to get my things packed before the taxi arrived," and so on.

If one looks at these different distinctions he discerns that informally we measure time with a rubber yardstick but that the divisions are always the same in relationship to each other.

One also discovers that it is such things as the degree of urgency—whether we are doing or trying to do more than one thing—and whether we are active or passive that determines which part of the scale we feel we are on.

In regard to informal time, we do not seem to share the monochronic isolate with the Arabs. Theirs is a polychronic culture on the low end of the scale. (The Chinese impress one as being polychronic but very *high* on the scale; that is, they value or enhance the value of doing many things at the same time.) One of the results of our monochronism is rigid scheduling: we get very specific in regard to when it is we want things finished; so that a publisher will say, "Can you complete your book by July 1, 1947?" or a tourist will say, "I've got to have my car by 5:00 o'clock Monday afternoon"—what's left unsaid is, "otherwise I can't get to Dubuque by Tuesday morning and my whole schedule will be shot."

Monochronism and its resulting scheduling tend to put us in the position of constantly reminding ourselves and others of deadlines that have to be met. The deadline therefore becomes a common item in the vocabulary of Western time. This item is the equivalent of a "dirty word" to someone who has been raised according to Arab time. To tell an Arab mechanic, "I have to have my car by 5:00 o'clock tomorrow afternoon," is like backing him into a corner and holding a stick over his head. The best way *not* to get your radio fixed is to pinpoint the time when you expect to have it done. In the U.S., to get technical about the specific time a given job has to be finished is a way of increasing the emphasis and urgency. In the Middle East such specificity communicates something quite different. There are ways of getting things done by a given time, but they do not include getting technical about the point in time that the job has to be finished. These differences are largely traceable to the differences in the isolate level between the two cultures.

An examination of informal patterns in the two cultures also provides us with some interesting contrasts. In the U.S. there are two principal patterns of informal time. Participants of each look down on the other. The difference between the two patterns can be seen if you observe the behavior of individuals in such situations as arrival at offices where the target time remains constant—shall we say, 8:30. Given this time, participants of one pattern will not consider they are late until about 15 minutes after the appointed hour and will feel they are being quite prompt if they arrive, shall we say, at 8:35; 8:30 is OK and 8:25 a little early. I have called this the diffused-point pattern because the target time has been spread out, as it were.

In the other pattern you are late if you arrive at 8:31 and just barely on time at 8:30. These people will, as a rule, arrive considerably before the appointed hour. This pattern has been called the displaced-point pattern, for obvious reasons. It is best known in connection with evening visits and calls in the urban East of the U.S.

The diffused-point people think that the displaced-pointers are unduly obsessional, rigid, strict, or controlling in their handling of time. The displaced-pointers, on the other hand, feel that the participants of the other pattern are sloppy, at times irreverent, and not quite to be depended upon. Both are loathe to recognize that there are really two or more patterns in the U.S. and that some people learned one and others, another.

In the Middle East, I am told, promptness at offices is a function of the degree of security of the employee. If he is very secure he will be somewhat irregular; if he needs the job he will be on time, which means that he will arrive before the appointed hour. Appointments in this part of the world use one point of reference in an over-all pattern; so that a person will say, "I will see you before one hour or after one hour, before one week or after one week," and so on. Because of the way we handle our points we tend to be somewhat cavalier with the Middle East fixed point. This is a mistake. On the other hand, their references sound impossibly inexact and vague because we don't know what is meant by "after one hour"— which can mean anything to us. Besides, we are used to two points, either stated explicitly or implied.

One has to make a special point of the fixed point in this part of the world because of our own tendency to diffuse points at least part of the time. If an American is asked how long he has been home from the office and he says, "Since 5:00 o'clock," and someone saw him on the street outside his apartment at 5:05, he will wonder what the American is trying to hide and ask himself, "Why is he lying?" Likewise, if one of us is asked, "How long have you been in Damascus?" and we reply, "Two years," when in reality it has been only 22½ months, the hearer will again wonder what devious schemes we have up our sleeve—because otherwise why would we lie?

Since it has been possible to give only a few examples of the results of a microcultural analysis, it is hoped that enough has been given to communicate the general pattern of the work. These studies are in their preliminary phases, and much that has been reported here will have to be considered as tentative in nature, particularly some of the Middle Eastern patterns.

It would, however, seem that a microcultural investigation and analysis properly conducted can provide material which can be compared in the same way that phonetic and phonemic material from different languages can be compared. The results of such studies are quite specific and can therefore be taught in much the same way that language can be taught. Such analyses require considerable time to conduct because of the small size of the structure points one is working with. The highly specific nature of these data have proved to be more acceptable to the operator than some of our more generalized formulations about culture.

Washington, D.C.

Notes

1. Originally this paper was titled *A Microcultural Analysis of Time and Space.* It had to be drastically cut to fit into the schedule, so only *Time* will be dealt with in this presentation.

THOUGHTS ON METHODOLOGY FOR COMPREHENSION OF AN ORAL LITERATURE

Melville Jacobs

Anthropologists and folklorists have collected quantities of myths and tales, often with exacting standards and awareness that the principal value was the advancement of scientific knowledge. However, comprehension of oral literatures remains largely descriptive reporting. A science matures as it arranges and classifies its reported facts, and as it formulates and tests hypotheses concerning the weightings of causal and reinforcing factors. Folklore has progressed little toward such maturity because it has not advanced significantly in the direction of performing revealing operations upon its descriptive data. Since it has not set up a structure of theory it has been unable to arrange and test reported data to demonstrate segments of a theoretical system. Recent emphases by adherents of C. G. Jung and the group of New Critics upon archetypal themes and supposed origins in rituals are of slight value as guides to recognition of the many important features which should be investigated in oral literatures. These writers have failed to offer arrangements of types of content and traits of style, and theorization about process within each type has been lacking. It is timely to inquire about procedures which effect usable arrangements of all folkloristic materials and accordingly lead to examination of theory. I comment summarily in this paper upon attempts to use a few fresh methods in which I engaged when studying Chinook myths and tales.

THE RACONTEUR

Many writers have stressed the creativity of the public narrator of folktales. The seminal role of the community in its year-round discussions of stories, and the awareness which a recitalist possesses concerning audience feelings during his performance have been largely ignored. Notations on such matters were rarely obtained from informants and involved difficulties in field research. The dogma of signal creativity of the rare genius may have deflected research away from quest of processes which reside in community manipulations of stories and in impact of the populace upon narrators. The recital has fascinated folklorists, although the principal dynamics which shape performance utterances may reside elsewhere. In many cultures there is as much or more creativity by persons who are infrequently or not yet narrators, but the factors which may be discerned in their discussions of story content and forms remain almost unknown. Folklore's progress toward a theoretical system, and its capacity to weigh the many causes of content and style, hinge upon developments of methods for analysis of the needs and behavior of the people of communities rather than citations of inexplicably exceptional men.

CONTENT OF MYTHS AND TALES

A number of familiar premises offer a foundation for methodology in study of folklore content. An oral literature should be recorded in the native language with a close translation, maybe excepting only the infrequent instances where an informant can provide a sensitive and precisely translated version directly in a European language. The sociocultural setting in which the literature was nurtured and maintained must be well known.

Plot, motif, and actor content cannot be isolated from traits of style. Both content and style have to be subjected to classification in contrastable types of features. An inventory of classes of features of content includes the following for a Chinook literature. I cite emphases in story content, social relationships, male personality traits, female personality traits, traits of child actors, explanatory and origin items, humor-generating stimuli, value ideals explicitly or implicitly present, items expressing the supernatural, traits exhibiting world view, and items referring to foods and technology. Each of these and other classes of content is further divisible. A principal aim of such ordering is to facilitate determination of probable sociocultural causes for their articulation in recitals. The task is to so analyze the culture and the content of all the stories that one may ascertain for each class of story content its special cluster of causes.

In the compass of this paper attention is given briefly to each mentioned category of features of content.

The first is that of principal emphases. Simple counting of frequencies of content items in Chinook literature shows that broadly social and narrowly kinship relationships, such as of child to grandmother, receive more stress than actor delineations, humorous situations, or moralistic presentation of value ideals. Although identification of kinship relationships as units is subject to a margin of error, it is clear that outstanding emphasis is in expressions of tensions and releases such as arose in narrator and audience identification with actors in their social relationships. In only one of over sixty stories is there a total absence of reference to a social or narrowly kinship tie. Humor is missing from about half, personality depictions and value ideals from many of the stories. Why this patent accent upon relationships? Although a plausible answer presumably will cite multiple factors, the following starting suggestion is offered. Myths and tales constitute a kind of screen. Upon it appear actors and situations identifications with which effect emotional releases for which the societal structure did not fully provide. Since Chinook society apparently failed to offer outlets for various feelings such as those which developed in child-grandmother relationships, myths and tales presented escape valves. Tense feelings which were poorly or not at all resolved in community participations were directed onto the screen of a dramatic art.

Itemization into tentative units of such unfulfilled sentiments gives the following classification for Chinook. Feelings about siblings were projected in twenty-five of a total of more than sixty stories. Feelings arising in Oedipal and marital relationships, including child-grandmother bonds, were expressed in seventy-nine instances. Feelings about children received expression only twenty-eight times, oldsters thirty-five, social inferiors twenty-three, non-villagers twenty-eight. Feelings derogatory to women were expressed twenty-four times.

A simple arithmetical method of this kind helps to point up problems about some aspects of story content. A suggested hypothesis is that the society failed its people most notably in Oedipal and marital relationships. Seemingly it exhibited a degree of success in handling relationships toward children, oldsters, inferiors, outsiders, women, and siblings. But tensions in such matters were so considerable as to result in channelings onto the story screen. Support for this conclusion would have to be found in ethnographic analysis.

A second category of story content is personality depiction. A method is required for its study. Dr. Margaret Lantis lately published a treatment of personality content in Nunivak Eskimo literature, with the purpose of deducing Eskimo features of personality. Like Eskimos, Chinook narrators cited extremely few traits of personality for story actors. The paucity of explicitly phrased features is like the limitation in anatomical traits carved by abstractionists in sculpture. In an oral literature intimated feelings of actors are a reservoir for examination of additional traits of personality. Characteristics thereby established for male and female actors of various age grades may be compared. Constellations of traits displayed in the behavior of principal actors may be contrasted with traits inferred for other actors and people of the living culture. An actor's lineaments in the several stories in which he appears are also assembled, because his behavior in any single story must have been partly shaped by the concept of him which society had concerning his whole personality. Procedures employed to identify personality traits in actors give types of traits and their frequencies in the literature. These point to unresolved stresses in society. Such stresses contain causes of literature content. Although single traits of personality expressed in stories probably occurred in living individuals, trait clusters which characterize actors may not all tend to be mirror reflections of people. Sorties may emphasize needs and values not the totality of traits of personality found in a member of the community.

Humor is a third category of content. It is virtually unexplored in oral literatures. Means of studying it include identification of fun stimuli; their arrangement in types such as slapstick, incongruity, antifeminine, immaturity, old age anxiety, mutilations, and verbal slips; assemblage of instances of these into clusters of factors; and ethnological analysis of reasons why they stimulated laughter. A folklorist should supplement field recordings and translations of stories with annotations about raconteur and audience responses to humor. He should also record many examples of wit and humor apart from the literature.

Chinook myths contain much humor. Tales contrast sharply. They are prevailingly tragic. Myths are less ominous because they deal with personalities of fanciful kinds and are dated in an ancient era. Therefore they are richly embellished with fun situations.

In the analysis of humor I again resorted to an arithmetical approach. I found that in a group of over one hundred fun-stimulating situations in stories, causal components which I recognized averaged over seven items in each situation. That is, humorous responses were usually complexly determined. Laughter in general seems to be consequent upon multiple stimuli which are quickly woven. Some stimuli, perhaps a minority, are consciously selected by a storyteller or jokester. The role of community discussions in creation of humor needs to be assessed against the work of the recitalist.

The next category of content, also little understood, is values. They may be well represented in explicit phrasings in some oral literatures. In others like those of the Pacific Northwest States, ideals infrequently received formulation because communities utilized pedagogical and other structured means for articulation of values. Moralistic notions and feelings about the good strait-jacketed stories, but were not verbally set forth in them. The problem of method is, then, to identify, classify, and weigh values upon bases of ethnographic research and deductive analysis of social behavior and actors in stories. A related problem is to determine how an environment of such values functioned in creation, shaping, and transmission of stories. If ethnographic information is spare, analysis of values implicitly expressed in a verbal art may suggest points of tension in the society, not a full inventory of values.

Another type of content, cosmology or world view, has long interested folklorists. An annoying problem arises when collections are made in dying cultures whose few survivors fail to recall origin or cosmological narratives. I wish to mention only one aspect of my findings on world view as expressed in Chinook myths. I believe that it can be shown that concepts termed trickster and transformer, long employed by folklorists of the Pacific Northwest, improperly represent actors so designated. The Northwest Coyote, denoted both trickster and transformer, is less a cosmological figure than a projection of culturally pressured needs to advance from immaturity and unreliability to wisdom and responsibility. Changes in the era of myths were more often due to unnamed people than to Coyote. He and other actors are primarily announcers not manipulators, of things to come. They are projections of informed elders, headmen, or persons of unique insight because of possession of potent spirit-powers. Mistakes which folklorists made in interpretations of Northwest cosmologies show that concepts such as trickster, transformer, and culture hero deserve reexamination in each literature. Again, deductions from ethnographic data and from probing analysis of the words and behavior of actors will shape delineations of the cosmologies which function in literatures.

Stories always contain mentions of technology, economic production and distribution, and similar matters which some writers have termed material culture. There may be mentions of rituals and religious behavior. A question arises regarding the utility of analysis of such story content. Dr. Franz Boas explored this subject more fully than others, in his *Tsimshian Mythology*. Citation of material culture, social life, and religion may be employed not merely as evidence of culture traits but, again, as indication of spots where feelings were intense and fantasy ventilation was needed. Mentions of various culture traits also operate on a stylistic level. The portions of *Tsimshian Mythology* which present features of the culture are potentially contributions to a study of style. They also direct attention to emotional needs of the people.

Embroidery of visual images, so noteworthy in stories of northerly Coast groups, contrasts with the minimum of such supplementation in stories recorded in southerly Coast groups whose compactness, terseness, rare notations of material culture, and suppression of mentions of ritual are striking features of their style. One may deduce that rituals in the southerly district served the people well. I think that it can be demonstrated that major anxieties of northerly Coast peoples resulted in distinctive forms of literary expression and that southerly Coast communities had other literary content and forms because of lack of those anxieties and presence of different ones.

STYLE

Studies of stylistic features of oral literatures have been few. We possess a small list of examinations of the epic form, stylized introductions and closings, pattern number, and explanatory elements. Many additional traits of style require study. Progress in knowledge depends upon our choice to seek them. Each of the entire range of stylized devices and motifs should be treated independently and subsequently exhibited in its connections with the others. If ethnographic data are at hand and a collection of stories is sufficiently large, well told in the native language, and sensitively rendered in translation, important advances can be made in knowledge of creativity, factors in stability, and community handling of stylized components.

A principal feature of folklorists' orientation which has escaped criticism, except by a very few writers, is the concept that myths and tales are analogues of the West's short story and novel forms. I suggest that myths and tales resemble the West's drama rather than its stories and novels. If the analogy is of value, it follows that concepts such as prologue, epilogue, one-Act play, two-Act play, skit, and Scene may serve as frames to show an aspect of structuring in a major verbal art.

Many features of style may be manipulated with more exactitude by recitalists in their shorter performances. I think that it can be shown that longer stories often witness faster internal and stylistic changes than shorter ones, because of more community debate about them. Comparative researches might provide documentation that would support a thesis that a community and its storytellers tend to examine creatively the content and style of long stories while it inclines to leave short compact ones alone. Long ones lack unity of content and simplicity or manageability of features of frame. Short ones harden where long ones remain pliable.

Stylized devices such as connectives, pauses, and vocal mannerisms, to effect transitions from Scene to Scene or Act to Act in a longer story, are invariably discernible in its dictation in the native language. But publications infrequently if ever preserve evidences of these devices. Tape recordings, more perceptive field observations, and linguistic analysis jointly permit building a descriptive body of knowledge about this facet of style.

Formal beginnings, introductions, or prologues are often so stylized that only a recording in the native language allows their scientific manipulation. The same applies to titles or other devices by which a community and its narrators refer to stories. In a paper to be published in the *Journal of American Folk Lore* I have explored each feature of content and structure in story titles rendered by a Chinook informant.

Story endings or epilogues are frequently complex in content and structure. They too may have to be studied on the basis of their utterance in the native language.

Magic or pattern numbers which folklorists have long recognized sometimes display variability within a single collection. For example, where five dominates a verbal art explanations should also be sought for instances of two-, three-, and four-patterns in the same corpus of stories. Folklorists have accounted for pattern number mainly with surface explanations based upon evidences of diffusion and rituals. Causation and maintenance of pattern number may not be so simple. I think that among Pacific Northwest peoples four- and five-patterns were culturally and psychologically maintained, subtly so, by feelings generated in sibling relationships. Factors in the phenomenon of pattern number may include feelings of rhythm, pressure of adjacent literatures, rituals, and, maybe paradoxically, nuclear family relationships.

The topic of style contains other classes of formal features. Some which I regard as of major importance may be termed repetitive formulae and recurrent themes. Examples from Northwest literatures include numberless instances where the recitalist must select from a variety of terse indicators of location which need not be presented to enhance meaning. When he refers to a person of poorer station he must say, although everyone knows it, that "she lived in an end house." When an actor approaches a village the raconteur must remark, "He saw the village below there." When he introduces a headman he must say that that worthy "lives in the center of the village." When an actor travels, he must "cross five mountains." Occasions when forms of such kinds are required should be assembled, classified, and connected with the daily life of the people.

A comparable category of demanded utterances offers datings. An actor always leaves on the following never the same day. Or, he does something "for so long a time," without further specificity in timing. Or, he does something "all the time" or "all day long," when the meaning is simply durative or action that is repeated a mere few times.

Folklorists have regarded as story content recurrent themes or plot devices. These comprise another and large class of features which are primarily stylistic. In Northwest stories they include announcers of the future world, oldest stupidest, youngest smartest, progressive superiority or viciousness in a sequence of siblings, the concept of a bride-to-be traveling unchaperoned to the man she desires, the breaking of an important artifact as a portent of tragedy, stylized depression, vanquishing of an ogre by causing him to overeat, killing an ogre from within him, supernaturally effected bad weather, proscribed direction, miraculous growth, external heart, arrow ladder, various life tokens, and others too numerous to list. Methodological problems include determination, for each literature, of their frequency, stylistic role, and sociocultural or other factors which pressure their recurrence.

For generations folklorists have been intrigued by explanatory elements. These too often operate stylistically. In the Northwest States few plots or actor depictions appear to have been shaped by need to explain the world. Explanatory items in that region are patently a diverting embroidery and only secondarily explanatory. They are digressions, asides, peripheral supplements. Usually they lighten tension. Their origin seems to lie in community theorization within small districts, for they are rarely diffused over a large area. Their content, frequency, forms, and occasions for resort to them should be studied.

Features of style which are of utmost importance and especially difficult to particularize are those which effect speed of delineation of situations and actors, and selection of their characteristics. It is not enough to generalize that actions are described tersely or that actors are depicted in a few strokes. The folklorist needs to inventory traits which the recitalist tends to phrase in order to contrast items which he implies and which his audience perceives in context. Written literatures exhibit numerous descriptive details because their readers are unfamiliar with plots and actors. An oral literature contains few or no surprises because its audience is its co-author. Recital calls primarily for a stylistically adroit presentation, featured perhaps by abstractness, succinctness, and a limited permissible selection of descriptive details. Auditors fill in as they listen. Their familiarity with, year-round discussions, and manner of transmission of verbal art account for the style of recital utterance. Each trait of style must be traced to such community orientation, participation, and authorship, and to special needs which shaped the etiquette of the recital situation. A principal task of research, then, is the onerous one of learning how a community discusses and reworks its literature between recitals. Processes and dynamics of verbal arts are sometimes futilely sought in analysis of the stark words which storytellers dictate.

SUMMARY

In any oral literature, as in other facets of sociocultural life, careful analysis of classified portions of the material displays numerous structurings and multiple determinants of varying weights. Patterns enclose patterns. Structurings intersect, contain, and shape one another. Few have been accorded recognition. Because folklore has confined its scholarly efforts to collecting and to study of presences and distributions of a few categories such as plots, motifs, and actor characteristics, it has not matured as a scientific field of inquiry. It has not dealt with other kinds of units of content and style. The opportunity in folklore to recognize and arrange its many kinds of phenomena is obvious. The goal is comprehension of the dynamics of the entirety of the verbal arts. Exacting classification and intensive analyses of all aspects of a literature, following their identification, will show the way, if the research is done without minimization of sociocultural factors.

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RECREATIVE BEHAVIOR AND CULTURE CHANGE

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Scattered materials in the literature of cultural dynamics suggest that the "recreative" facets of behavior—those connected with relaxation, leisure, play, entertainment, and similar activities—tend under some circumstances to show marked persistence, but for the most part are notably open to innovation and to crosscultural transfer. Under intervention pressures where, so typically, economic, political, and perhaps other areas of choice and expression may be blocked by external power, recreative outlets may become focal rallying points for self-motivated activity and morale.

The purpose of this paper is to draw attention to the need for more systematization in this field, both as regards culture change studies, and more widely in terms of general social anthropology theory. As compared, say, with social structure, or child rearing, or religion, behavior that is "recreative" gets at best an unobtrusive corner in standard monographs. Except for occasional items such as Firth's notable analysis of "the dart game in Tikopia" (1930), the reporting of child and adult games, entertainment, and the like, tends to be formal, with little of the rich psychological, social and cultural texture one suspects is really there, both for participants and for spectators.

Behavior which by human criteria is inferred to be "play" in subhuman animals receives wide but rarely more than passing mention by physical anthropologists. Fortunately Kroeber has discussed with insight the links between "organic play impulses," such as are notably characteristic of the young in mammals, and the dynamics of human culture. "In rechanneled form," he says, "(they) have motivated great areas of human behavior and important achievements in culture . . . not only games and sports, but the influence of curiosity, of desire for variety, of mental restlessness in the arts and sciences and fashions" (1948).

Malinowski, to take another of the rare high points in theory, also speaks of the "creative element" in recreation. "In primitive civilizations," he states, "the vanguard of progress is often found in works of leisure and supererogation . . . Advances in skill, scientific discoveries, new artistic motifs (may) filter in through the playful activities of recreation, and thus they receive the minimum of traditional resistance which is associated with activities not yet taken very seriously." By contrast, he adds, many types of games and other amusements have rather the function of establishing "social cohesion," as where a "complete sociological recrystallization" may take place during big public games and ceremonies. "In civilized communities," he asserts, "the type of national pastime contributes effectively to the national character" (1931). Malinowski, Slotkin and some others note how competitive sports may channel conflict and aggression into rule-defined, playful behaviors.

The most elaborated theory developed in this field within anthropology to date appears to be that of Bateson, centered on "play." This, he says, is "one of the great creative fields of human communication." Its distinctive mark is a logical type or frame of reference in which fictional premises hold sway, by contrast with those in which truth, reality, dominate, e.g. as with "work." "The participants (in a game)," he points out, "set up as fictions the rules of that game," including perhaps assumptions of opposition and competition, also codified symbols of gain and loss; but, "As we say, 'It's only a game'" (1951). Bateson and his associates, in a Stanford sponsored research project, have been following up these leads in psychiatric contexts, and by making films of animal play.

Recreation, by and large, is a behavioral zone which is very much an "open" system. It is strongly marked by fictional premises, by elective variation, by novelty, by risk-taking, by the super-utilitarian, by the non-serious, by relative freedom from demanding goal-orientations and strong sanctions. It is far-ranging, with many possible types of structured group activity, yet also much that is informal and personal, as with humor, fantasy, or even "just sitting around." Like magic it has what might be called "white" facets of public, approved behavior, and also "black" facets of private, subversive behavior as with salacity, pornography and obscenity. In terms of function, a particular kind of recreation may activate in varying degree such elements as (1) "pleasurable" or "hedonistic" affects, which are always likely to be stressed; (2) organically "relaxing," "energy restoring" results in the busy or tired person, or an "outlet for excess energy" in the case of the zestful; (3) "integrative" or "reinforcing" influences upon individual or group, making for stability, cohesion, high morale; (4) "therapeutic" or "sublimative" results as often channeling off conflicts, aggressions, hostilities; (5) "creative" or "reintegrative" tendencies, as offering fields for nimble innovation and self-expression; (6) "communicative" functions, as in learning and habit formation, notably among children, but also among adults; and (7) a frequently "symbolic" significance, as in "playing out" important cultural values and premises, e.g. as with many toys.

Most of these broad characteristics are also shared by recreation's close cultural relatives, art and religion. Both of these behavioral zones also have frames of reference different from those of everyday "reality" activities, though with art subordinating the playful to the aesthetic, and religion using it in the interests of the sacred. Titiev sees "games of chance" as notably like religion in challenging men to find out about the unknown (1954). Moreover, for the individual, perhaps any activity no matter how serious, as with work or worship, may in a given situation become invested, if not with fictional premises, at least with the mood of the relaxing, the playful, the entertaining. Inversely, recreation may transmute into the serious: not only do we sometimes say "This work is like a game" but "That game was hard work." Fortunately, if a clearcut definition of the "recreational" in activity is somewhat elusive, the field worker can apparently always find in the action categories and in the vocabulary of any people he is studying these zones which they count recreation. No Samoan has difficulty in recognizing a *fiafia*, an occasion for play and festivity. The English *Thesaurus*, whether we start with the French root of recreation ("giving fresh life") or the Anglo-Saxon root of play ("quick motion," "frolic") branches out into dozens of words categorizing such behavior.

With this all too brief background discussion, we may turn more specifically to recreation and culture change. The anthropological literature has scattered materials on the development and diffusion of particular games (Tylor published two papers on the subject as early as 1879), and also of dances, songs, and other relevant items. So-called acculturation studies also frequently contain at least passing reference to the retention or abandonment of old recreational forms, the adoption of new ones, and perhaps some reformulations of old and new which occur in the local contexts. Modern Hawaiians, and emulating whites and Asians, maintain or revive the cult of flowers, dancing, canoeing, surfriding, though with modifications which in some instances come via Hollywood or Broadway. American Indian "Fairs" may include exhibitions (with an admission charge) of lacrosse, archery, dancing, gay with modern pan-Indian regalia based on what white people expect Indians to be like. Applied anthropology reports may have particularly useful glimpses into this behavioral area; discussions, for example, of cultural loss through government or missionary expurgation of older recreational outlets; or again of attempts at substitution as where in Papua government anthropologist Williams gave out soccer footballs as an alternative to intergroup war feuds.

Any field worker could add detailed observations to the published record. In 1951, the writer's jeep bumping down the Kokoda trail in Papua was stopped several times by villagers wanting to use the tire pump to blow up their footballs. Maori villages in New Zealand, after traditional canoe races and dance exhibitions, may turn to a rugby football game or a tennis tournament. In Samoa, song, dance, and drama are creative traditions being added to constantly, and the village cricket pitch may have the adolescent boy and the dignified titleholder practicing together. In Western Samoa, during an anti-government movement of the 1920s, the locally adapted form of cricket was made the excuse for covert assemblies forbidden by the government, and, when men were prohibited by law from playing, their wives ostentatiously played cricket —and politics—instead.

Recognizing the great need for further systematic ethnographic data and crosscultural comparison, a few more generalized hypotheses may be tried out here to show the kinds of theoretical leads which are open. First, a traditional recreative activity appears likely to persist when it has continuing functional relation to social structure, child training, religion, or other behaviors pervaded by focal values. It might, for example, have learning or symbolic functions. The lacrosse that the writer has seen so-called "pagan" Menomini Indians play at a burial ground in memorializing the dead has a vastly different context from the lacrosse of their tourist exhibitions. The latter, however, illustrates how formal elements of old recreative behavior may become meaningfully integrated into a changing context, and so show partial persistence. Stability may also show where an old activity still affords "recreative" satisfactions, as, say, with a pattern of humor or a story, so that its maintenance is a matter of conscious preference over alternatives. The observer usually finds here, however, that changes can occur freely in the details of the activity, just as also in its general context where the wider milieu is changing. A festive occasion along acculturative frontiers is likely to be a kaleidoscope of old and new.

Granting selective tendencies to persistence, the recreative zone of culture appears likely to be well in the van of change, as with new sports, card games, popularity of movies, construction of "playgrounds." To the extent that recreative outlets involve elective, idiosyncratic, self-expressive, competitive, and other behaviors characteristic of an "open" system, change tends to be nimble, with innovation and culture transfer likely to occur freely (Keesing, 1949, 1953). Correspondingly an agency of intervention is likely to find recreation to be an area of great vulnerability within a cultural system. Governments, for example, may deliberately use holiday entertainments, school sports, exhibitions, and other activities as means to wider ends such as rallying political loyalties or reducing tensions. The "model village" of an overseas development scheme will surely include a sportsground. Here, too, as a new recreative element enters the fresh cultural context, it may readily undergo form-function modifications, just as "football" has differentiated into a number of game types.

In the initial paragraph, reference was made to how, when economic, political, or other areas of choice and expression may be blocked by intervention of an external power, recreative outlets may become rallying points for self-motivated activity and morale. Such behaviors may here become focal, even if to use Bateson's concept the "fictional" premises might seem to represent a compensatory or escapist substitute for a "reality" frame of reference. Studies by Herskovits and others of Afro-American recreative behaviors appear to provide data of relevance here. The emphasis often given to such outlets in the course of movements involving cultural reformulation, as in historical settings of colonialism and of nationalism, can also be profitably studied, noting both the "integrative" and the "therapeutic" functions which recreative classes of activity may serve. These may range from public recreative and related artistic activities to the subrosa political joke counted subversive by a dominant regime.

The writer once suggested years ago that such a people as the Polynesian Samoans may have the "highest standards of leisure" known to date in the worlds of human culture. With the machine doing more and more of the work of our own society, and with many of our basic premises in flux, recreation bids fair to become much more of a focal area in our value orientations than our present social dogma admits. "Play," which by and large seems to have pertained most fully in human societies to the statuses of youth and of the old, now battles increasingly against the preoccupation of young and mature adults with what we call "work," even with the "serious." A future society pervaded by the technology of automation might have to give recreation a prominence which even a Samoan could well wonder at.

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THE USE OF TYPOLOGY IN ANTHROPOLOGICAL THEORY

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Whitehead was right when he said that classification is only a half-way house in science. On the other hand, I think L. J. Henderson was equally correct in saying that in science any classification is better than no classification. A classification is useful to the degree that it sheds light on the relation between one set of facts and another. By a "typology" I mean precisely a classification that is explicitly theoretical in intent as opposed to one intended purely as a descriptive categorization.

Anthropology has had both typologies and empirical groupings for convenience. There have been the asserted evolutionary stages, posited with varying degrees of refinement. There have been categorizations based on selected criteria that were immediately observable: phenotypic "races"; culture areas; agglutinative, polysynthetic, and isolating languages. Categories based on a single feature whether technological, social, or psychological, have also been used. Thus we have spoken of plough or matrilineal or Dionysian cultures. Quite recently, certain typologies have been presented that exhibit some scientific precision or logical rigor or fairly adequate microscopic attention to the complexity of the data. I cannot give an exhaustive listing, but will limit myself to examples from publications in English. One may instance the classifications by Boyd[1] and others of genetically similar populations; Greenberg[2] and Voegelin[3] of linguistic typology; Steward[4] and his associates on types of multilinear evolution; Linton's[5] typology of nativistic movements. Redfield's work for many years has had a focus on an essentially typological problem: that of the folk society or little community. His theses have provoked much controversy[6] but are gradually illuminating many issues of the theory of types. It would be generally agreed that the British social anthropologists and certain Americans, such as Murdock and Eggan, have significantly advanced the typology of kinship and allied aspects of social structure. Lévy-Strauss[7] has proposed a typology of orders (kinship, social organization, and social stratification) and of types of connection between spatial arrangements of residence and kinds of social organization. He has likewise boldly suggested that social anthropology, linguistics, and economics can all be grouped into the single master field of communication.

Such concepts as "antagonistic acculturation" constitute a beginning for one typology of process. Vogt[8] has sketched a performance typology. Wallace[9] has sketched some types of events. Last year two typologies[10] of South American social structure were published. We have had at these meetings a typology of Indian cultures of California and are to hear a paper on the typology of the Japanese family. One could give many more examples, of course.

Steward's theory seems to be the most general and thoroughgoing to date within cultural anthropology. That of Trager and Hall[11] is also extremely interesting and more along the lines of the present paper. Steward distinguishes between culture types and classifications based on areas or value systems. He elaborates concepts for determining cross-cultural types.[12] His own central concept is based upon two frames of reference: cultural features derived from synchronic, functional, and ecological factors; features represented by a particular diachronic or developmental level.[13] He asserts[14] that cross-culturally valid categories will apply only to similar cultural types—i.e., types that recur in a multi-evolutionary scheme will be distinguished by unique categories.

The publications of the past decade make it evident that anthropology is moving toward more ramified and more sophisticated typologies. But much remains to be done. Most classifications are still either crudely empirical or grossly impressionistic. We are relatively rich in content categories but poor in conceptual or relational categories. Steward[15] agrees that we shall "have to distinguish innumerable culture types, many of which have not yet been recognized." Anthropologists of all branches need to study the elegant work of their linguistic colleagues and also the related fields of mathematical logic such as set theory. In spite of Gödel's Proof, Russell's Theory of Logical Types[16] deserves the close attention of anthropologists, for perhaps the most frequent and serious error in anthropological theory continues to be that of confusion of levels of abstraction. Many anthropological typologies are not firmly based upon the postulate that there must be a discontinuity between a class and its members. In general, anthropologists dealing with the theory of typology could well study the careful work of contemporary philosophers upon this subject. Ryle,[17] for example, says of the determination of types:

> It has long been known that what a proposition implies, it implies in virtue of its form. The same is true of what it is compatible and incompatible with. Let us give the name "liasons" to all the logical relations of a proposition, namely what it implies, what it is implied by, what it is compatible with, and what it is incompatible with. Now, any respect in which two propositions differ in form will be reflected in differences in their liasons. So two propositions which are formally similar in all respects save that one factor in one is different from a partially corresponding factor in the other, will have liasons which are correspondingly dissimilar. . . . The operation of extracting the type of a factor cannot exclude the operation of revealing the liaisons of a proposition embodying it.

There are two principal kinds of problems where typology is relevant to anthropological theory. The first is less abstract. This is that of assigning a biological or cultural specimen (or a piece of behavior) to the group to which it belongs. This is pre-eminently the task of biological anthropologists, of archaeologists, of museum ethnologists. Is a tooth ape or human? Does a pot most nearly approach "the ideal type" of Wingate Black-on-White or a similar but different pottery category? Here statistical procedures are often appropriate. The discriminant function[18] should determine whether a tooth is ape or human, *assuming it belongs to one of these groups.* The S-function is used to test whether the set of measurements is such as to make it likely that it is a member of a particular category. Since one cannot—or should not—measure everything, one is still faced with discovering what measurements are crucial or most economical to obtain such sortings. Moreover, while it is often essential to obtain the critical matrix of dimensions, measurement in and of itself may be either insufficient or actually misleading in localizing an object or event in a time or space or time-space category. Bronowski and Long say: ". . . even when no single dimension shows a significant variation from its group mean, the total configuration may still be wrong."[19]

The second class of typological problems is that of establishing groups or orders rather than that of assigning an individual entity to such a category. So far as typologies based on content are concerned, the principles involved are relatively simple, however difficult the detailed execution may become. One makes certain that the criteria chosen are actually relevant to the purpose or purposes at hand, and one investigates the inter-correlation or inter-association of these criteria so that one is not merely compounding the effects of one or two criteria. One makes one's *fundamenta divisionis* completely explicit, and one follows them consistently. One keeps one's levels of abstraction straight.

Typologies of relations are more complicated and less explored, save, to an increasing degree, in the areas of linguistics and social structure. Although I am profoundly convinced that linguistics supplies invaluable models, I shall for the rest of this paper limit myself to cultural anthropology in the narrow sense and indicate some directions that I believe might profitably be followed. These center on ways of developing typologies of cultural structure. This implies "models" rather than empirical generalizations. The "models" must embrace as much empirical fact as is convenient to the conception proposed or the hypothesis to be tested and must not be contradicted by any *pertinent* datum. But "models," by definition, represent relationships regularly prevailing between strictly selected assemblages of fact; they do not and cannot encompass a total cultural inventory.

A typology of cultures, it seems to me, should be directed toward such questions as the following:

What is apparently incompatible with what else? For example, are patrilocal bands never found among sedentary peoples who depend primarily upon agriculture for subsistence?

What is extremely likely to be found with what else? For instance, is culturally approved aggression against distributive minorities found mainly in an "atomistic social order,"[20] whereas a channeling toward segmental minorities is characteristic of more centralized social organizations? Are, as Murdock[21] suggests, the features ascribed to folk societies particularly associated with peoples practicing local endogamy?

Are some concatenations of cultural features or elements indifferent as far as minimal necessary coherence of the system is concerned and hence found associated or not associated merely as a result of the accidents of the historical process?

Enough information is now available to make possible the construction of a first approximation to an analogue of the chemical table of atomic elements.[22] Which combinations of cultural elements are, apparently, "impossible"; which are very rare and probably due to exceptional circumstances; which are so frequent as to be statistically predictable; on which is no guess justified one way or the other?

Such enquiry, exposing the principles of cultural structure, would take us some distance toward ranging cultures in an orderly way as to their respective similarities and differences. It would also help us to isolate wherein rests the distinctiveness of each particular culture at a given time level—the "without-which-not" of that culture. We require, of course, to deal with flow as well as fixity, typologies of process as well as of form. Seen through time, another dimension would be opened up. When a cultural structure assumes a radical alteration in form, what changes first? Is Murdock's thesis that shift in the rules of residence most often initiates other major shifts in social structure an invariant property of human social organization generally or only an important statistical generalization which will turn out to be less valid for some types of cultures than for others? In short, systematic work on the typology of cultures should reveal what Kroeber[23] has called: "... the larger configurations inherent in the multitudinous data: configurations or classes that carried hidden in themselves their derivations, their historical relations."

This refers first and foremost to the implicit culture. And, if I am correct in thinking that the key to selectivity in the implicit culture is the value system, it means the devising of standard operations for exposing the value system with its hierarchy. My assumption is that every such system could be parsimoniously described in terms of not less than about ten nor more than about twenty key values and their relations of interdependence and especially superordination-subordination. Perhaps by a kind of scaling technique the number of "essential" or "distinctive" values could be reduced to a few, but, even if plausible on logical grounds, this requires empirical demonstration. Although the methods should, I think, be quite different, the objective resembles that of factor analysis. One wants to find out what value "loadings" give recognizably distinct character to each cultural structure. One avenue to this end is the cross-cultural comparison of what values are found in complementary, coincident, incorporating, and overlapping distribution.[24]

We continue to be plagued, of course, by the twin dilemmas of what constitutes "a culture" and what units within such cultures can properly be compared. On the first point, I would slightly paraphrase Lévy-Strauss[25] and say:

> A culture is a set of patterns of and for behavior prevalent among a group of human beings which, from the point of view of the research at hand and of the scale on which it is being carried out, presents, in relation to other such sets, significant discontinuities.

As to the second point, it remains unfortunately a fact that the conceptual apparatus of cultural anthropology still does not supply culture-free elemental units comparable to the phoneme and the morpheme. I do believe that some workers in social structure and folklore[26] (Goodenough and Sebeok, for example) have almost formulated the procedures for isolating such units. I suspect it is significant that Sebeok is a linguist and that Goodenough has been much influenced by linguistics. Much in the history of science in this century (physics, neurology, information theory—to name only a few fields) suggests that the dichotomous oppositions or principle of complementarity applied by the linguists with such signal success deserve sustained trial in the realm of culture in general. Niels Bohr[27] says:

> ... the viewpoint of "complementarity" forms indeed a consistent generalization of the ideal of causality ... the trend of modern psychology can be characterized as a reaction against the attempt at analyzing psychical experience into elements which can be associated in the same way as are the results of measurements in classical physics. In introspection, it is clearly impossible to distinguish sharply between the phenomena themselves and their conscious perception, and although we may often speak of lending our attention to some particular aspect of a psychical experience, it will appear

> on closer examination that we really have to do, in such cases, with mutually exclusive situations. . . . The main obstacle to an unprejudiced attitude towards the relation between various human cultures is . . . the deep-rooted differences of the traditional backgrounds on which the cultural harmony in different human societies is based and which exclude any simple comparison between such cultures. It is above all in this connexion that the viewpoint of complementarity offers itself as a means of coping with the situation. In fact, when studying human cultures different from our own, we have to deal with a particular problem of observation which on closer consideration shows many features in common with atomic or psychological problems . . .

In my opinion, proper units for comparison will be arrived at along linguistic lines by determining contrastive categories rather than by any amount of measurement. As Lévy-Strauss[28] remarks, ". . . there is no necessary connection between *measure* and *structure*." Meyer Schapiro[29] has reminded us that the history of art shows that one can sometimes actually get greater precision by dealing with qualities. Anthropologists should avoid the mistake made by many American social psychologists and sociologists of putting a naive faith in numbers and especially in statistics.

The investigations by the botanist, Edgar Anderson,[30] constitute a dramatic paradigm of warning: graphic representation of a few features enabled biologists and many non-biologists to differentiate correctly two species, even when they were not told the number of species involved. In contrast, analysis of variance and regression techniques yielded inconclusive or much less efficient results. Anderson comments:

> If one sets out to analyze the difference between two species, the actual data are individual plants or animals, each individual a multiple-sense-impression of size, shape, color, texture, etc. . . . To analyze the nature of these differences we need to make a *selection* among the thousands of sense-impressions which come to us from each specimen . . . the two species may be completely separated by the resultant of seven variables even though any single variable would not suffice when used singly. . . . An impressive proportion of the best discriminators refer to *pattern* . . . In problems involving multiple sense impression, such as differences between species or varieties, where from each individual a seemingly infinite number of numerical facts could be derived . . . the customary methods of biometry are still inappropriate and ineffective. . . . Pointer readings are *not* more exact than any other kind of precise record . . . species are differentiated by combinations of characters more certainly than by single characters . . . (Emphases mine.)

I submit that there is a suggestive analogue here for the anthropologist trying to discriminate cultural "species" and sub-cultural varieties. The issue is that of discovering means for selecting the significant—and perhaps representing these features along the lines of the very interesting graphical techniques proposed by Anderson. If operations are firmly specified, "qualitative" judgments can be as systematic and as rigorous as quantitative ones. In another paper,[31] Anderson reminds us:

> Biology has advanced most rapidly when appropriate qualitative measures have been developed and used with precision. In Genetics, for example, the fundamental data are qualitative. Once obtained they are treated with such

precision that most geneticists probably think of their work as purely quantitative. But the fundamental categories, "vestigial" vs. "non-vestigial," "scute" vs. "non-scute," "forked" vs. "non-forked," etc. are quite as qualitative as the fundamental categories of taxonomy. . . . If the methods of Drosophila genetics were purely quantitative, the flies would not be classified in qualitative categories but their wing lengths, eye diameters, etc. would be laboriously measured. Imagine the difficulties of conducting a Drosophila experiment in which the only available data were the lengths and breadths of the wings! Genetics has been able to advance because it was willing to take the Mendelian recessive (a qualitative unit about whose ultimate significance relatively little was known) and to use that unknown but recognizable entity as a basic unit.

For typological models of structure and of process we need to abstract from immediately visible "reality," disengaging the accidental by including in the models only those aspects of the observable that are relevant to the model being constructed. This means, among other things, as Goodenough[32] has shown in a brilliant example of the kind of analysis I am advocating, carefully distinguishing those phenomena which do tend to be associated with a particular category from those which are essential criteria for membership in that class. Only thus can we trace the intersection of systems with formally independent structure. Only in this way can we isolate the organizing principles that determine both the character of sectors of a culture and the patterning of the whole. We must, as the linguists have done, identify the significant structure points and classify accordingly, isolating the units at each succeeding level of complexity.

Eventually we can, I believe, describe the compositional pattern of each culture and construct a comparative grammar and a comparative syntax of cultures. All grammars limit freedoms and control choices. A comparative grammar of culture would delimit the necessities in cultural development: what features must precede or be associated with what others? Only the intensive and systematic study of variation, and variation through time, revealing the latent structures and latent concepts and incomplete paradigms, can make a grammar of cultures possible.

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Notes

1. See, for example, his chapter in *Anthropology Today*.
2. "A Quantitative Approach to the Morphological Typology of Language," pp. 192–211, in: Robert Spencer (Ed.) *Method and Perspective in Anthropology*, Minneapolis, University of Minnesota Press, 1954. See also: Paul Menzerath, "Typology of Languages," *Journal of the Acoustical Society of America*, 22 : 698–701, 1950.
3. "On Developing New Typologies and Revising Old Ones," *Southwestern Journal of Anthropology*, 11: 355–361, 1955.
4. *Theory of Culture Change*, Urbana, University of Illinois Press, 1955.
5. "Nativistic Movements," *American Anthropologist*, 45 : 230–239, 1943.
6. For a thoughtful discussion, see F. G. Friedmann in *The Peasant*, A Symposium Concerning the Peasant Way and View of Life, Number 7 (May, 1956). Mimeographed; Department of Philosophy, University of Arkansas.
7. His chapter, "Social Structure," in *Anthropology Today*.
8. "The Southwestern Fiesta System," *American Anthropologist*, 57 : 820–839, 1955.
9. "A Science of Human Behaviour," *Explorations*, 3 : 127–137, 1954.

10. Kalervo Oberg, "Types of Social Structure among the Lowland Tribes of South and Central America," *American Anthropologist*, 57 (1955), pp. 472–487. E. R. Wolf, "Types of Latin-American Peasantry," *American Anthropologist*, 57 : 452–471, 1955.

11. "Culture and Communication," *Explorations*, 3 : 137–249, 1954.

12. *Op. cit.*, Chapters 1 through 5.

13. *Ibid.*, Chapter 5.

14. *Ibid.*, p. 81.

15. *Ibid.*, p. 24.

16. This is not itself a "typology" but has far-reaching implications for the construction of typologies.

17. "Categories," in: *Logic and Language* (second series), Oxford (England), Basil Blackwell, 1953, pp. 79–80.

18. Cf. J. Bronowski and W. M. Long, "Statistics of Discrimination in Anthropology," *American Journal of Physical Anthropology* (1952), 10 : 385–395, 1952.

19. *Ibid.*, p. 389.

20. Cf. John Honigmann, "The Testing of Hypotheses in Anthropology," *American Anthropologist*, 54 : 429–432, 1952.

21. "Changing Emphases in Social Structure," *Southwestern Journal of Anthropology*, 11 : 361–371, p. 365, 1955.

22. C. Lévy-Strauss, "Language and the Analysis of Social Laws," *American Anthropologist*, 53 : 155–164, 1951.

23. *Method and Perspective in Anthropology*, (*op. cit.*), p. 275.

24. Cf. C. Kluckhohn, "Toward a Comparison of Value-Emphases in Different Cultures," in: Leonard White (Ed.), *The State of The Social Sciences*, Chicago, University of Chicago Press, 1956.

25. *Anthropology Today*, p. 536.

26. Stith Thompson's much earlier work on the classification of motifs was an important precursor of the search for elemental units in folklore that are relatively culture-free.

27. "Natural Philosophy and Human Cultures," *Nature*, 143 : 268–272, 1939.

28. *Anthropology Today*, p. 528.

29. *Ibid.*, p. 290.

30. "Efficient and Inefficient Methods of Measuring Species Differences," in: *Statistics and Mathematics in Biology*, (O. Kempthorne *et al.*, Eds.), Ames, Iowa, Iowa State College Press, 1954.

31. "Hybridization in American Tradescantias," *Annals of the Missouri Botanical Garden*, 23 : 511–525, 1936.

32. "Componential Analysis and the Study of Meaning," *Language*, 32 : 195–217, 1956.

ARCHEOLOGICAL TYPOLOGY IN THEORY AND PRACTICE

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This study will be concerned only with methods of analyzing material remains recovered by archeologists. The remarks pertain only to trends among New World archeologists. The writer is not familiar with any recent publications by Old World scholars which clarify or formulate their opinions on the meaning of the word "type," but it is believed that these observations will apply equally to Old World archeology. In both regions, "type" has been applied in a multitude of ways, usually without definition.

While many disciplines can be pursued without recourse to a typology, in archeology as well as biology it is absolutely necessary to achieve a solid basis for organizing vastly complex material objects, if only to discuss their meaning intelligently in terms that can be generally understood. Without a typology, the results of research in these fields cannot be effectively compared with one another, nor can any individual possibly remember the details of structure, distribution, and interrelationships of the forms which occur, even in limited regions.

In 1952 Julian Huxley stated that anthropologists are at least forty years behind biologists in devising effective methods and procedures for organizing their data and observations. If this statement is true for anthropologists in general (and I think it is), it applies even more so to archeologists, whose work is, in the main, with material objects which are as suitable for objective treatment as the enormously complex organic world studied by biologists.

A common explanation that archeologists give—if they give any at all—is that the artifacts, houses, graves, settlement plans, and artistic works of man are more complex, more subtly intergraded, and more difficult to analyze than the products of nature; that *species* are controlled by laws of genetics, are more rigidly predetermined than the products of human activity, and are therefore more easily discovered. Such an attitude not only reflects ignorance of the incredible complexities which have faced biologists in their search for a workable taxonomy, but it in no way justifies the fact that archeologists even now, in 1956, have no particular common techniques for creating typologies, or even agreements on what the term "type" should connote.

The question of what typological problems archeologists and biologists have —or do not have—in common has been extensively reviewed by Brew (1946: 44–66). While Brew has attacked the whole species concept as full of unworkable defects, it is inconceivable that biological science could have progressed as far as it has without this concept. Brew repeatedly stresses the point that there is no "natural" or inherent classification in nature or in the products of man; but he neglects the other side of the question, namely, that biological research has long been directed at discovering *consistency* in the combination of structural features through time and space distributions. While one may admit that the search for "basic" or "most important" criteria as the basis for a taxonomy leads only to confusion and frustration—both in biology and archeology—

this does not disturb the fact that, with intensive study, it has often been possible to discover that a *combination* of morphological features can be shown to occur again and again to the extent that this combination can be recognized in practice by almost anyone.

One has only to look at the many manuals that have been produced by biologists, for example the handbooks on birds, mammals, reptiles, insects, fishes, flowers, etc., to realize that, despite all defects and unsolved problems, they are infinitely farther along than archeologists are in the discovery of types. Among biologists, a *variety* is a deviation from "normal" which may occur at any time at any place within the general distribution of a species; and a *subspecies* is a debatable concept, applied to a deviation which may or may not have the status of a species or a variety. Whatever the individual worker's attitude toward the placement of individual specimens may be, it is clear that a typology or speciation is *not an end in itself, but a framework within which one carries on research.* Within such a framework, anyone can discuss his opinions as to what should or should not go into previously recognized species.

What has archeology produced that is comparable in concept? It is not that archeologists fail to apply themselves, for they certainly do in their field methods. Field methods may be said to have been virtually perfected for the last decade or two; not only does the modern excavation involve infinite care and labor, but it reveals a fine sense of obligation to scientific principles.

The same cannot be said of laboratory principles or procedures. While everyone realizes that artifacts must be classified, if only to reduce repetitious description, it is not at all well understood what the act of classification is supposed to accomplish beyond this. If the ultimate purpose is to define "units of culture" which may be compared with one another and used to understand the growth and diffusion of culture, then it is strange that archeologists can be satisfied with "types" that are devised in scores of different ways. Even within the same region, different workers follow different assumptions and procedures, so that where one worker sees seven "types" of figurines, let us say, another may see as many as sixty "types." The same degree of divergency in results applies to the study of pottery, projectile points, and all other classes of material; yet attempts are constantly made to place these results in tabular form to show degrees of relationship between levels, sites, and areas. Again, some workers freely use the word "type" for *parts* of specimens, or even as a synonym for *specimen* when we read of "unusual types" when the author means "unclassified specimens."

It will be well to note that there is nothing particularly new about man's desire to classify objects and phenomena of all kinds. Man has probably been devising words to indicate distinctions between different kinds of animals, plants, fruits, fish, weather, etc., as well as differences in size, dimensions, weight, temperament, and the like ever since he has had language. Anthropologists have often been struck with the extensive vocabularies of even the most primitive societies, resulting from some basic urge to designate differences between all manner of natural phenomena as well as objects produced by man himself—that is, to classify.

The principal new element is that science demands greater and greater precision, and needs to know by what criteria various units, kinds, or "types" can be recognized, so that different workers can have maximum assurance that they are talking about the same things and comparing phenomena that really are comparable. Probably only the "pure sciences" of mathematics, chemistry, and physics may be said to be approaching optimum agreement on units of observation and measurement. The earth sciences and biology have been somewhat less successful but far more so than social sciences. Statistical treatments are now a common research activity in the latter disciplines but there is still a great deal of argument about how raw material should be organized—and for what purposes—before statistical methods are to be applied. In archeology, any conclusions based on statistical methods are open to criticism as long as the "units of culture" (traits or types) are attained by guesswork or purely personal inclinations.

EARLY CLASSIFICATION SCHEMES

Before examining the literature, I should like to repeat a distinction made some years ago (Krieger, 1944) between *classification* and *typology*. The two terms have, of course, generally been regarded as synonyms, and probably few will agree that they can or need be distinguished. However, I prefer to think of *classification* as any act of sorting or designating, and of a *typology* as a more orderly system of actions, obeying certain laws or principles. Thus, anyone can classify in any number of ways, but a typological (or taxonomic) system can only be attained in a limited number of ways, must have a clear aim, and requires considerable knowledge of how the material occurs in space, time, and context.

Many "classification schemes" have been proposed for archeological material. When accompanied by explanation (many are not), the purposes of such schemes are usually said to be "convenience in remembering" or the discovery of "universal relationships." In America they have been directed mainly at two classes of artifacts: chipped-stone (principally projectile points) and pottery. Whatever the author's intentions, methods, or assumptions, the results are always called "types." Few authors have bothered to explain what a "type" means to them; the sorting comes first and the result is then automatically a set of "types."

Perhaps the earliest classifications of stone artifacts in America were those of Rau (1876), Fowke (1896), and Wilson (1899). These took the form of charts —that is, a system of cubbyholes into which specimens were to be placed. In the 1920's and 1930's a great deal of labor went into creating some hundreds of pottery "types" in the southwest United States, using for the first time a system of *naming* rather than designation by numbers or symbols. These were compiled in the manuals of Hargrave (1932), Hawley (1936, revised in 1950), and Colton and Hargrave (1937), while many additional descriptions appeared in the publications of Colton, Kidder, Shepard, Gladwin, Haury, Sayles, McGregor, and others.

Although these Southwestern pottery types were accompanied by extremely little explanation of the methods or philosophy involved—and recently a reaction has set in which questions how many of them are really distinctive types and how many were too hastily concocted from minor local variations in technique—Southwestern archeologists appear to feel that experience has supported the reality of most of them: that is, they can be recognized by many different workers, and they have value in historical reconstructions.

The principal of *naming* Southwestern pottery types was an extremely important advance over charts designated by symbols, for, with it, new types could be added or obsolete ones dropped, without disturbing the others. It must be noted, however, that even the Southwestern archeologists have continued to use classification charts for objects other than pottery. Meanwhile the naming of pottery types (commonly called the "binomial system") has spread to the Southeastern states, Northeast, Middle West, Plains, Middle America, and South America, roughly in that order, and in recent years a similar system of describing and naming projectile-point types has appeared in Texas, the Mississippi Valley, and the Plains.

Going back to the creation of classification charts, Gifford and Schenck published, in 1926, portions of a projectile-point scheme devised by Strong, who later published it in full (Strong, 1935). The Strong chart immediately became widely adopted among United States archeologists as "the official system," so that projectile points throughout the land which more or less fitted the outlines shown on the chart were automatically called "types," each designated by a code of letters and numbers. It was mentioned above that some sections of the country now follow a more realistic method in outlining types (or at least groups) which are described for their own sake and labeled with names. However, in the western states, the Strong chart is still widely used and it is still used in the Plains area for points which are relatively late in time, while the oldest ones are named!

In the 1920's and 1930's many other classification charts, mainly for projectile points, were offered by Deuel (1927), Greenman (1929), Nelson (1929), Renaud (1931, 1934), Kidder (1932), Black and Weer (1936), Cole and Deuel (1937), and Finkelstein (1937). Deuel (1927), Nelson (1929), and Renaud (1931) were unpublished manuscripts. Of all these authors, only Nelson discussed at any length the theoretical grounds on which the "types" were to be based, stating that artifacts could be arranged in a set order similar to the "natural classification" of zoology, and that archeologists should make every effort to determine the "primary, secondary, and tertiary" characteristics—that is, to classify artifacts according to features arranged in a sequence from "most important" to "least important." All or most of the other authors believed that subdivisions on their charts could be arranged in some order of descending importance, a point which Brew (see above) rightfully attacked as an illusion, both in biology and archeology.

The classification of stone artifacts by Byers and Johnson (1940) broke away from this obsession for finding the "most" and "least" important characteristics of artifacts. They showed that some combinations of morphological and technological features could be demonstrated to have more *historical significance* than others; thus each type could be built up in accordance with historical knowledge, independent of any over-all charting.

In addition to groupings called "types," most publications also present separate descriptions of artifacts, particularly those of infrequent occurrence. Thus archeologists now proceed to classify and describe in at least four different ways:

(1) With charts arranged in some "descending order of importance," each grouping designated with a code of letters and/or numbers.
(2) With improvised groupings designed to facilitate description and illustration, regardless of what other authors may do, even in the same area.
(3) With entities that are not dependent upon one another (as in charts) but which may be formed in a variety of ways and designated with names.

(4) With individual description, not grouped unless by some assumed function (functions may sometimes be determined through ethnographic data).

LITERATURE ON THEORY OF TYPES

The first published paper which, to my knowledge, may be said to have contributed to a definition of typological concepts was that of a Russian, Gorodzov, which appeared in the *American Anthropologist* in 1933. This was described by Kluckhohn (1939) as "a tentative and fumbling consideration of the implications of the typological method," but Kluckhohn himself did not attempt to explain what this method is, or should be, only emphasizing that "Meanwhile typologies are proliferated without apparent concern as to what the concepts involved are likely to mean when reduced to concrete human behaviors."

At the same time, Rouse (1939) published what was certainly the first concerted, systematic effort to make archeologists conscious of the inner problems of artifact typology. In addition to a careful, detailed definition of terms, Rouse explained how the descriptive properties ("attributes") of artifacts could be combined in a great variety of ways to formulate types of proved significance in the dimensions of time and space, as well as in their cultural context. He further discussed the highly significant point that "attributes" can be invented and borrowed apart from the typological wholes to which they belong, leading to new combinations of further significance.

In 1944 the writer published "The Typological Concept," which summarized most of the works cited above and attempted to formulate a set of premises and procedures for the definition of types with demonstrable historical meaning. This study was aimed at laboratory practice as well as theory (the writer has a bias against theorists who are unable to demonstrate their recommendations with an actual run of material). Five years later the practical aspects of laboratory procedure were discussed in more detail (Newell and Krieger, 1949: 71–74). A brief definition of early American projectile-point types, following the same procedures, had meanwhile been published (Krieger, 1947); and in 1954, Suhm, Krieger, and Jelks described some eighty types of pottery and seventy types of projectile points in their "Introductory Handbook of Texas Archeology."

It must be emphasized that in all these applications of the methods advocated by the writer, *no types have ever been regarded as final.* They have all been formulated, rather, as reasonable entities with the hope and expectation that other archeologists would try to recognize them and follow their distributions as far as possible into surrounding areas—so that eventually their ranges of variation could be more accurately defined. This process, which necessarily takes work, time, and patience, should also result in proving some types invalid and in revealing new ones as information increases.

Several recent students of typological methods have stressed the artificial nature of all types, stating that they are inventions of the analyst, created for his convenience in analyzing cultural products, and that they are not "natural" or "inherent" in the material. Without being able to observe first-hand what patterns of manufacture were considered desirable in the culture being studied, it must be admitted that "types" are arbitrary. On the other hand, it may be assumed that in any culture one generation learned from its predecessor that things were done in certain ways in order to achieve certain acceptable patterns of form and aesthetic quality. An attempt by the archeologist to devise types which may conform to "concrete human behaviors" is therefore far from hopeless, although it can never be achieved perfectly. Such a procedure has been very well stated in Phillips, Ford, and Griffin (1951 : 61–69):

> "Thus, we have in mind the concept of a continuously evolving regional pottery tradition, showing a more or less parallel development in and around a number of centers, each of which employs a number of distinct but related styles, each style in turn being in process of change both areally and temporally. With this remarkably unstable material, we set out to fashion a key to the prehistory of the region. Faced with this three-dimensional flow, which seldom if ever exhibits 'natural' segregation, and being obliged to reduce it to some sort of manageable form, we arbitrarily cut it into units. Such *created units of the ceramic continuum* are called *pottery types . . .*"
>
> ". . . To a certain extent, the characters we select as criteria for type definition, however dictated by expediency, not to say necessity, are bound to correspond to characters that might have served to distinguish one sort of pottery from another in the minds of the people who made and used it. We should, of course, make every possible effort to increase this correspondence. In the course of time, with increased information . . . our types will be redefined in ever closer approximation to cultural 'realities' . . . The limits of the variability of the types will then no longer be wholly arbitrary decisions of the classifier, as is now the case, but will bear some correspondence to ethnographic distributions in time and space."

These authors follow this discussion with an explanation of "methods of sorting." One of them, Ford, has performed great labors in striving for precision in the counting and plotting of pottery-type frequencies in order to attain a sensitive scale for control over culture change. By use of bar-charts, Ford plots percentages for the pottery types from excavation levels, and he goes so far as to assert that such charts can be used to *predict* the chronological position of a new collection of unknown (or at least debatable) age (Phillips, Ford, and Griffin, 1951 : 219–234). While the degree of "fit" will probably serve to predict chronological position among nearby sites, I believe that Ford goes too far with it in predicting position for collections made as much as 200 miles apart. At any rate, he clearly outlines all his assumptions and the limitations of his conclusions.

In Ford (1952) the role of personal experience in defining types is freely admitted. Thus, years of work in a limited area will eventually reveal a great deal about how "attributes" tend to combine in time and space dimensions. While this paper contains many statements that are puzzling to me, there can be no question that Ford is fully aware of historical processes and that he has been able to demonstrate subtle changes in culture by use of pottery types. Pottery, however, is only one aspect of culture.

Spaulding (1953*a*) has recommended that artifact types be "discovered" by simple statistical methods such as the four-cell frequency combination of attributes, and probability equations. Doubtless it is high time that archeologists employed such methods. My main point of criticism of this paper is that, with the methods Spaulding describes, a different set of "types" would result for each site or run of material treated. It appears that something else is needed, namely the element of personal experience with the manner in which attributes cluster in time and space perspective. Such prior knowledge may be utilized to attain *trial sortings* into *suspected types*; and the formulae advocated by Spaulding are then not so much for the "discovery" of types as they are for the impersonal *validation* of results.

Spaulding (1953*b*) has caustically criticized Ford's 1952 paper on "Measurements . . ." He not only states that in Ford's paper he was unable to discover "what was measured with respect to what scale," but that this study reveals "serious methodological deficiences" and "fundamental misapprehensions of scientific method." Not only were Ford's pottery types not determined by any rigorous scientific methods, he says, but "no effective consideration" was given to negative data, adequacy of sample, or presence or absence of disturbance in the excavated deposits.

Conversely, Ford (1954*a*) comments on Spaulding's "Statistical Techniques" (1953*a*) as "amazingly naive." He says that this method "will reveal the relative degree to which the people conformed to their set of ceramic styles at one time and place, but that is all it will do. . . . Such studies could be better made after the chronology is controlled." Ford continues:

> "Patterning is not the central problem of typology, rather it is the framework in which the problem of setting up measures of time-change and geographical space-change on each unit of the pattern have to be solved."

Replying to this, Spaulding (1954) goes on:

> "I would argue that any reasonably consistent and well defined social behavior pattern is historically useful, i.e., meaningful in assessing similarities and differences between any two components . . . and if the techniques actually do what they are supposed to do they cannot fail to yield historically useful units. . . .
>
> "I should add that I do not favor setting any boundaries by legislation; I am quite willing to let Ford have his types if he will let me have mine. *The important thing is to be explicit about what kind of type one is talking about.*" (Italics mine.)

We need not decide who is correct in this controversy. Both have stimulated other archeologists into thinking more seriously about the implications of setting up types of any kind. If anyone is to be chastised, it is those many archeologists who do not know even yet that there is a typological problem.

Ford (1954*b*) states that the *type* is "the basic conceptual tool of cultural research" and tackles the problem of whether types exist or are "inherent" in cultural material. He concludes that all types are abstractions and as such may be made on many different levels of complexity: "What the classifier must do is to select a level which will serve the purposes in view." Here Ford defines four "dimensions" to the cultural type which need not be repeated here. However it is worth quoting the following:

> "In most archeological research, chance has determined the form of the typological structure to a great extent. . . . Permitting sampling chance to determine typology operates very well so long as the archeologist has only a spotty sampling of the culture history. Types are easily separable and they look natural. However, when the gaps are filled in so that the history may be viewed as a continuum through time and space, the naive typologist is certain to run into serious difficulties. . . . The artificiality of the groupings must be taken into consideration and type groupings consciously selected if working typology is to be developed."

Many of the arguments advanced by Ford for the "measurement" of culture change through seriation and graphing of pottery-type frequencies were also presented by Robinson (1951) and Brainerd (1951), although I am not aware of any exchange of ideas between these authors and Ford. Robinson, a sociological statistician, was not concerned with how the ceramic types had been formulated in the first place; he utilized material provided by Brainerd from Mayan archeology and demonstrated how *agreement coefficients* of first, second, and third order can be used to measure similarity of percentage distributions and thus to reconstruct chronological order in the material, even if such order was not apparent during excavation.

Brainerd (1951) reviewed certain aspects of archeological typology and described a new method which he called "ordered matrices" or "contouring" for a sharper, more impersonal delineation of pottery types. His Figures 95–99 illustrate "contoured matrices" for pottery from five Mayan sites. Space will not permit a review of this interesting approach; I can only say here that the Robinson and Brainerd papers seem to combine some of the Fordian principles of utilizing pottery types for the "measurement" of culture change with the pleas of Spaulding that the types themselves must be formulated by impersonal, objective, and "scientific" methods. Brainerd apparently did not consider prior knowledge of comparative distribution of ceramic features necessary, or even useful, for his method, as he does not mention it. As in the case of Spaulding, I feel that these statistical applications will result in a different set of "types" for each site treated.

The application of statistical methods to archeological material was anticipated long before the above studies by Kroeber (1940). He demonstrated use of four-cell coefficients of similarity to express degrees of relationship between *sites* or *complexes*, utilizing trait lists already prepared by others. He did not discuss typology itself except to state that:

> "The first requisite seems to be a sound comparative typology. Once we have this, the rest will follow, whether with the help of statistical devices or otherwise."

PROCEDURE WITHOUT FORMAL TYPES

Passing reference may be made to Woodbury's (1954) study of stone artifacts from northeastern Arizona. This splendid example of effective analysis and description of a very large archeological collection was written without recourse to the term "type" at all. While this may seem quite unremarkable to members of other professions, among archeologists it is phenomenal. Woodbury uses the terms "kinds," "groups," and "categories," employing functional designations when these are on safe ground by analogy with historic Puebloan Indian artifacts of known function ("axes," "shaft smoothers," etc.). When functions are unknown, he uses purely descriptive names for groupings ("cupped stones," "cylindrical stones," etc.). Woodbury states that some of these groupings will probably meet the criteria of "types" advocated by the writer (Krieger, 1944), but that he prefers to wait until more is known about them before formally designating them as such.

Daugherty (1956) is likewise cautious about using the term "type" and does so only when he feels that the groupings are on solid ground in regard to factors of comparative distribution in time, space, and context. Otherwise, he discusses the artifacts in terms of "forms" or "styles."

CONCLUSIONS

The act of classifying artifacts has been a preoccupation of archeologists for a long time. Classification, however, is not synonymous with the search for a typology directed toward definite ends and conducted according to definite assumptions, rules, and procedures. Until Rouse in 1939 began to formulate types with cultural and historical meaning, according to carefully defined principles, the study of typology cannot be said to have existed in American archeology. Since then, there has been a slowly but ever-increasing interest in the subject, but we are still a long way from attaining procedures which will be generally accepted and followed.

Many archeologists remain to be convinced that this effort is necessary. Strangely, even now, typological studies are directed primarily against the smaller objects which can be sorted on tables, hardly at all toward other cultural phenomena such as dwellings, burials, mounds, pyramids, or settlement patterns. An important exception to the last-mentioned is Willey's great study (1953) on prehistoric settlement patterns in the Virú Valley of Perú.

I have long urged that the word "type" in archeology should have approximately the same value as "species" in biology. Not that their meanings would be identical, but in both cases the term should not be loosely used for any kind of grouping. Archeologists can use other terms such as "group," "trial group," "kind," "style," etc., reserving "type" for the end result of a long process of research in which the grouping has "demonstrable historical meaning" in relation to other such groupings, and approaches as closely as possible to a reconstruction of a "concrete behavior pattern" among the cultures being studied.

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JAZZ CHOREOLOGY

Gertrude P. Kurath and Nadia Chilkovsky

COMPARATIVE ANALYSIS

Gertrude P. Kurath

The hot rhythms of jazz have set fire to most corners of the globe within fifty years. They have emerged from slave quarters and honky-tonks to respectable society and to learned circles. Only the music, however, has engrossed historians and musicologists and has engendered a voluminous literature. The indispensable concommitant, the dance, has remained neglected as a field of serious study. It deserves investigation by sociologists as well as by specialists in African, Afro-American, and jazz dance over a wide area. Someone has to get started on its choreology, its scientific analysis, albeit a specialist in the field of Amerindian dance, a jazz practitioner for the fun of it.

First of all, we need some kind of definition. Is jazz dance simply dancing to jazz music? Is it ballroom, folk dancing, or a form of degenerate professionalism? Without much ado one can answer that jazz dancing is always done to jazz, but not necessarily vice versa. Much of the pushing around to jazz bands on ballroom floors is not dancing at all. Some of it is based on Latin American styles, which are related but peripheral. Peripheral are also the dilutions taught in dancing schools, the vulgarizations in night clubs, and the creative elaborations of stage professionals. Jazz dancing is a twentieth century American product, with distinctive qualities and heritage to be identified presently, and with vast diffusion. It is primarily a dance of the people, hence a folk form, though it is often adapted to commercial purposes.

Stylistic Nucleus. To define the stylistic qualities I have notated the most important step patterns, have analyzed them and extracted essential characteristics. Most typical is the category known as jitterbugging,[1] which has many variants, antecedents, and developments, which differs by location, social and age group, and predominant color of a gathering, Negro or White. The fundamental, recurrent qualities can be identified as follows:

1. Rhythmic, often syncopated knee flexion.
2. Basic two-step pattern, in various tempi and directions.
3. Weight on heel or full foot.
4. Frequent foot twists.
5. Hip swaying and torsion, motivated by foot action.
6. Frequent sway back (Negro), forward torso tilt, or hunching (white).
7. Opposition between leg and shoulder-arm movements.
8. Syncopated hand clapping, finger snapping, or thigh swatting.
9. Aliveness of the entire body, with awareness of each part.
10. Relaxation to the rhythmic impulse.
11. Alternation of subtle and violent acrobatic movements.
12. Absence of set spatial or temporal form, with improvisation.
13. Frequent counterpoint with musical rhythms.

Jitterbugging is a couple dance, though not in the embrace position. The boy may hold one of the girl's hands to pull, push, and twirl her about.

After releasing hold, they can continue with the basic pattern of two-step and balancing, or they can string together any of the jazz steps in their repertoire.

With this brief stylistic definition as starting point, it will be easier to trace developments, to identify ethnic components, and to suggest ways of comparative analysis.

Development.[2] I have mentioned the two-step (or change-step) as the foundation of jitterbugging. The ballroom two-step of 1890–1910 was a dull step-close-step with rigid body. The one-step or turkey trot of the Irene and Vernon Castle team was little more than a smooth walk.[3] In the era of ragtime music the younger set began "ragging the two-step" with knee flexions and syncopations, and devised the foxtrot with its broken rhythm (see Ill. I).[4] Ballroom dances were performed in an embrace which was intensified into the dead clinch of the Californian Grizzly Bear.

The embrace and other restraints were shaken off after the first World War, in an epidemic of angular, foot-twisting gyrations—the Charleston, Snake Hips, Susie-Q, and Truckin'. The Charleston, after seething in the Southland as a Negro round dance, was discovered in 1923 among Negro dock workers of Charleston, S.C., and by 1926 had infected higher society.[5] The Susie-Q and Truckin' are said to have developed in New York's Negro quarter, Harlem,[6] when jazz musicians had spread from the Mississippi River and Chicago to the East. The "boll wevil" in Truckin' is a particularly entertaining and absurd step, a glide from heel to flat foot with a twist, a rear-protruding posture, and wicked shaking index finger. From these seeds sprang the Lindy Hop, named after Colonel Lindbergh's long hop across the Atlantic Ocean in 1927.[7]

In the middle thirties one of the Lindy steps, the kick, became the basis of the Big Apple. It has been rumored that the Big Apple originated with the Gullah Negroes of Georgia.[8] It seems a fact that Arthur Murray discovered it in a barn in Columbia, S.C. and popularized it, particularly in collegiate circles, by means of his national dance school. He considered it important, as the nearest approach in years to an original and native American dance.[9] It certainly is the only jazz dance which unites an entire assembly in one circle dance, with figures borrowed at times from square dancing, and which also incorporates solo or couple improvisations similar to the instrumental solos in jazz orchestras. The group figures had names like Peelin' the Apple, Kickin' the Mule, Organ Grinder, Praise Allah, Swing High Swing Low; and used all of the popular jazz steps. The soloists in the center could give free rein to their imaginations. It roused wild enthusiasm among participants, including myself; yet it has become extinct. Perhaps it became too eclectic.

Jitterbugging has, however, survived three decades, with constant modifications. After the Lindy period it was standardized into the Swing Break; during the Boogie-Woogie musical period it was known as Boogie, and it accumulated substeps, the Mooch, the dragging Sand, the Camel Walk, and the strutting Duck Walk, which recalls the Rumba in its hip action.[10] After the forties it took the name of Bop, no matter what music was used.[11] The Jersey Bounce, Detroit Jump, Huckabuck, and others came and went. Now it's the Chicken, miming rooster and hen, and Drivin' Home, with its suggestions of starting and steering a car.[12] Today the sequences are freer and more improvisatory than in the forties, while the style is more sedate.

All of these variations exhibit the same fundamental qualities. These we will now try to identify racially and historically.

Antecedents and Ethnic Components. As in the study of jazz music, we can ask, "To what extent is jazz dancing a Negro legacy from the importation of Negro slaves to the American South in the eighteenth century?" An answer requires examination of African and Afro-American as well as white and jazz dance styles, to support the traditions of Negro provenience for many of the steps.

Africa displays innumerable dance styles.[13] But certain qualities seem to predominate in the western slave coasts, among the Yoruba for instance. These are whole-bodied movements, flat feet, flexible knees, sway back or forward tilt, dynamics from pussyfooting to violent acrobatics, rhythmic complexity, improvisation, an unconcern for set structure or ground plan save a counterclockwise circle.[14] These resemble the characteristics of jazz dance. Two striking, culturally determined differences are Africa's virtual neglect of couple dancing, and its emphasis on religious function as opposed to the secular purpose of jazz.

Analogous characteristics, plus a religious motivation, survive in Haitian dancing[15] and other Negro-derived dances of the Americas. Oderigo's equation of the Carolina "shout" and the Brazilian "candomblé" also suggests jazz analogies.[16] As shown by Dr. Lorenzo Turner, the female devotees in the candomblé for the water deity Oshun shuffle with small two-stepping foot twists in a counterclockwise circle, while their leader gyrates herself into a frenzy. The Holy Dance or Shout of the Gullah and other southern Negroes proceeds in a heel-shuffle with flexing knees. In 1867 the "sperichil" was sung to a jerking shuffle, with hand clapping called "patting the juba."[17] Today this can also be observed in northern cities, Chicago, Detroit. In Ann Arbor I have witnessed at services of the Holiness church such syncopated knee flexions and hand clappings, two-steps with foot twists and low kicks, and spasmodic jerking.[18]

Recreational dances of plantation Negroes commenced with a prayer and worked up to considerable enthusiasm, even ecstasy. The slaves ragged and syncopated their clog dances, frenzied stamps with hand clapping, and less frenzied, prancing cake walks.[19] Around 1876 dances on the Cincinnati levees opened with a quadrille, borrowed from Europe, perhaps Spain. They culminated in a roar of song, stamping, patting juba, and acrobatics.[20] In new Orleans the Calinda was a contradanza in two rows (originally a stick dance). The women writhed with dragging steps, and the men leaped like savages.[21] This, the Bamboula, and other dances derived from the Spanish-influenced West Indies, especially after the Louisiana Purchase in 1803.[22] There was always a stylistic difference between men and women—observations confirmed by paintings of the period.[23]

These historical descriptions can readily be visualized by any observer of the Lindy in Harlem's Savoy Ballroom, when band and dancers warm up after midnight.

The White masters were fascinated by the slaves' antics.[24] In Cincinnati some Whites took part, appearing clumsy next to the lithe Negroes. Whites started the vogue of minstrel shows. In recreation they clung to European-derived dances, French quadrilles, English country dances with set figures and erect, proper posture, precise Scotch reels and flings, Austrian couple waltzes, and, later on, the two-steps. Quadroon balls in New Orleans imprinted a Negro flavor on the international assortment of quadrilles, polkas, waltzes, and Latin-American songs.[25] At times high society accepted one of the exotic dances and promptly dehydrated it. But one may suppose that the dockhands and their girls on Mississippi River boats performed turkey trot and tango with zest.[26]

After decades of disdain, educated Whites of the late twenties yielded to the captivation of jazz rhythms.

When they did so, they contributed their own mannerisms, exaggerating the Charleston and putting it on high heels, hunching shoulders in the Truckin'. White professionals have often created a pleasing style of their own within the medium of jazz, notably Fred Astaire and Ginger Rogers, more freely also "modern creative" dancers.[27] But the most exciting jazz dancing must still be sought in the streets and ballrooms and homes of the Negro population.

Though the obvious White contributions are limited to couple patterns, to figures of the Big Apple, and to secularization, yet jazz dance can be considered a racial hybrid, with the two-step rhythm as common denominator. The forms stem from the clash and fusion of races and cultures. Though jazz dance proper does not celebrate the economic and religious pursuits of life, it has its roots in such functional activities. Its analysis may hence prove useful in the study of seething racial problems.

Analysis through Symbols. The present modest paper cannot touch the wider ethnological or psychological implications.[28] Limited to the formal aspects, it will proceed to provide a firm foundation and to show a few tricks of the trade in dealing with forms. As in musicology these tricks consist in notation on paper by symbols, in juxtaposition of similar forms in a sort of tabulation, and in interpretation. A number of dance notation systems are available, including a convenient one of my own. In this article it is best to use Labanotation, which is widely known in many countries. Its symbols, here somewhat simplified, can reveal essential characteristics shared by African, Afro-American, and jazz dances.

The illustration aligns a few typical steps from personal observation. Always read from bottom to top and from left to right of the page, they represent the following steps:

(a) Two-step (ragged)
(b) Foxtrot
(c) Duck Walk
(d) Double Lindy
(e) Lindy kick
(f) Chicken
(g) Truckin'
(h) Step in a Holiness Negro service
(i) Step in the Brazilian candomblé

They represent the girl's part, usually only the right foot sequence, to be reversed on the other foot. Bilateral sequences are shown in (d), (f), (g), and (i).

The selected symbols show the salient characteristics noted as common denominator, as follows:

1. Rhythmic, often syncopated knee flexions (flexion shown by black symbols), in all examples.
2. Prevalent two-step, in various arrangements of short-short long and various tempi (relative length of symbols, fortified by musical notes written at the right of the column), in all except (g).
3. Shift from heel to flat foot (hooks on symbols), in (g) and (i).
4. Foot twists (oblique rectangle), in (g), (h), and (i).
5. Sympathetic hip movement (dotted line), shown for (g), also evident in (c) and others, though not shown.
6. Sway back (key signature at bottom of page), in (c).
7. Opposition of raised leg and shoulder (reversed symbols, shoulder and arm written outside three lines of staff), in (f).
8. Alternate extension and flexion (bent horizontal line and X), in (d).
9. Syncopated finger snap (here written by musical notes on right), in (c).

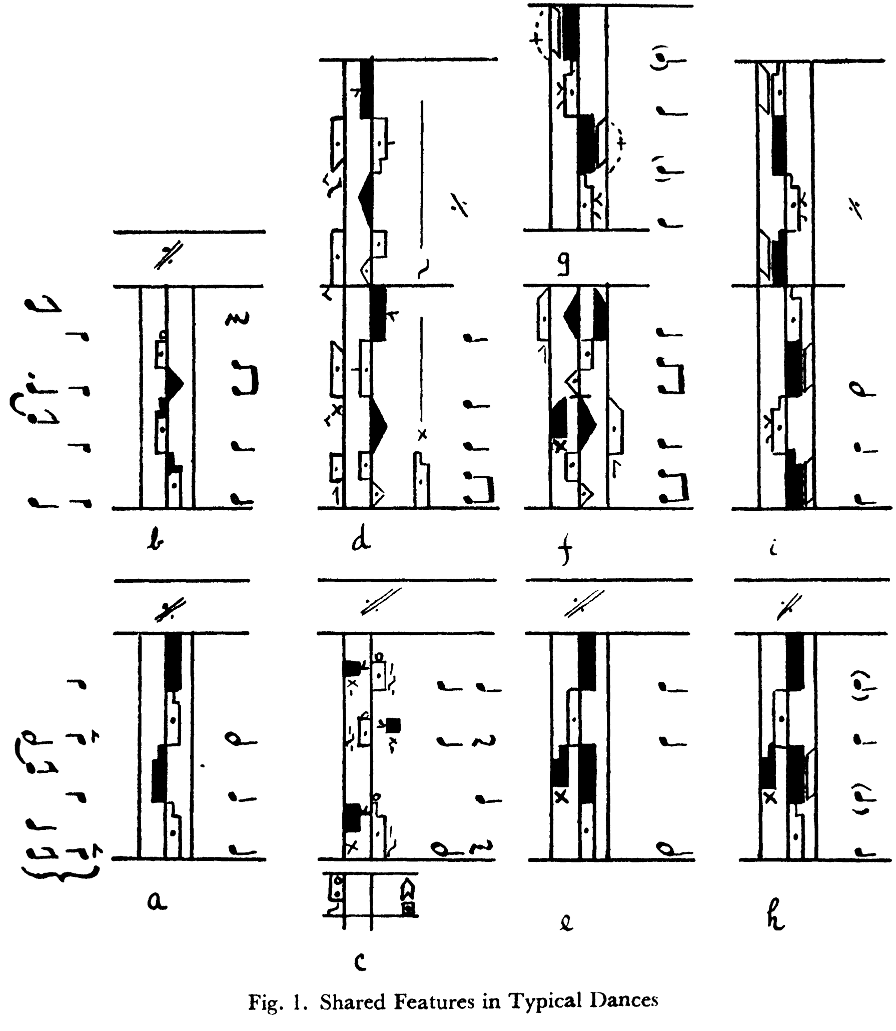

Fig. 1. Shared Features in Typical Dances

The Afro-American steps are so placed as to show the similarity between Lindy kick (e) and Holiness step (h), Truckin' (g) and candomblé (i). Other similarities and features will be apparent to a reader of Labanotation, or a student who consults one of the publications on the system, listed below. It is hoped that the general features will be apparent to all readers.

The musical accompaniment is not included with the tabulation; but some typical tunes and accompaniments are written to the extreme left. Suitable musical sources are appended to the bibliography.

Thus the illustration reinforces the remarks put forth in the written words of this article. The argument would be even more forceful when extended to a large number of dances and to the study of motion pictures, photographs, and descriptions, insofar as these are clear enough.

Ann Arbor, Michigan.

DANCE NOTATION

Nadia Chilkovsky

The ethnologist will easily recognize the value of recording not only the music and a word description of folk and ethnic dances but the movements themselves, if he wishes to study the influence of the movement behavior of the transplanted African Negro upon the music and dance of America, and, indeed, upon that of the entire contemporary world.

Almost 500 years of trial and error preceded the system of movement notation known in this country as Labanotation and abroad as Kinetographie Laban. Rudolf Laban devised an alphabet of movement notation in which there is a

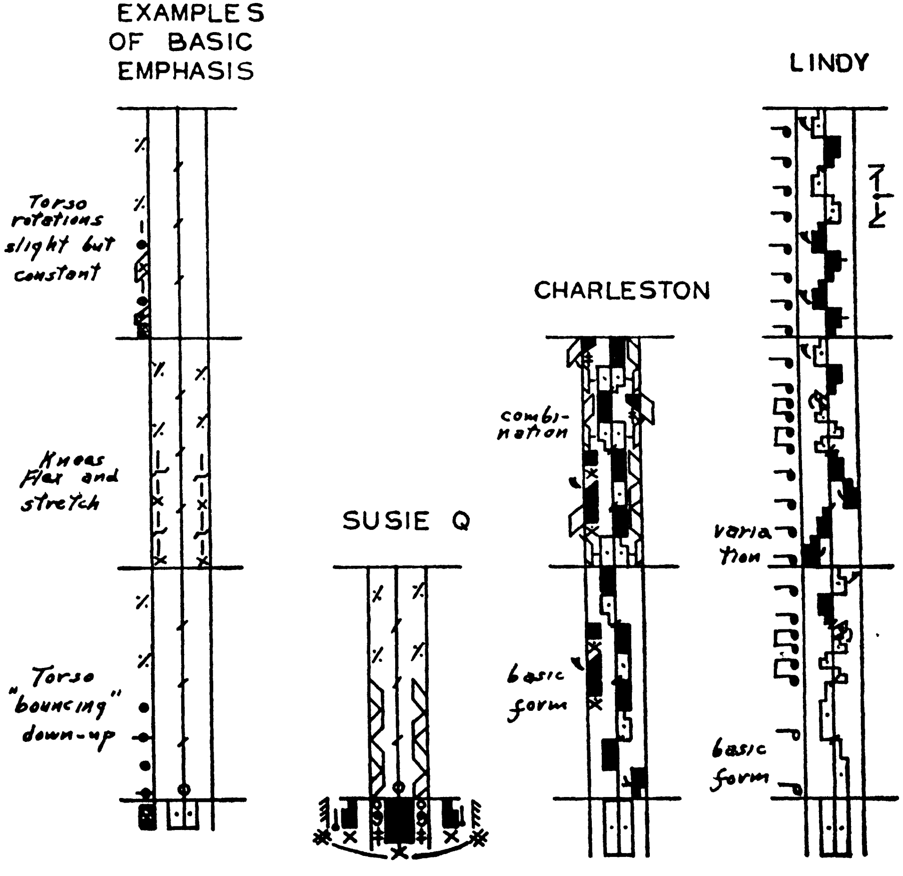
EXAMPLES OF BASIC EMPHASIS
Torso rotations slight but constant
Knees flex and stretch
"Torso "bouncing" down-up
SUSIE Q
CHARLESTON
combination
basic form
LINDY
variation
basic form

Fig. 2. Distinctive Features in Typical Dances

separate symbol for each motion within the framework of a tightly knit, logical, simple set of visual signs.[29] He discarded all previous efforts which either used letters as symbols for ideas of movement or which used symbols or words to represent combinations of movements, and set about the task of devising a movement alphabet which could record accurately the flow of time, effort and spatial path of all movement regardless of style. It can adapt itself to new ideas and needs for expression. The terminology can identify specific styles of dance as practiced by people of a variety of nationalities and languages, or codes of gesture language used by adjacent people with different languages.

Labanotation is read from the bottom of the page up, on a vertical staff of three main lines of which the center line represents the body's division of left and right.[30] On the center staff are marked off units of time which, in turn, are marked off by horizontal bar lines to represent meter. There is a separate location for each joint and body area, aided by a set of pre-signs. Eight basic direction symbols, all of them direct variations of a simple rectangle, are placed upon this staff. Each symbol indicates the duration of movement (by its length), the direction of movement (by its shape), the level of movement (by its shading), and the part of the body (by its location on the staff). These symbols are capable of infinite combinations.

The choice of illustrations has here been limited to a few of the steps known to one observer. The study can readily be extended not only to her full repertoire, but also to variants and to unique forms in many locations. For thousands of dance-literate students are now able to set down their observations and to make use of published texts. With a proved movement notation, students of culture comparisons can thus have source materials from many areas, as far apart as Nigeria, Brazil, and New York City. They can combine these with their own data towards the solution of ethnological problems.

Philadelphia, Pennsylvania.

Notes

1. Murray, 1946, p. 33.
2. See development traced in Kurath, 1949.
3. Shawn, 1956, pp. 8, 34.
4. Davis, 1923, p. 44; Murray, 1946, p. 32; Shomer, 1943, p. 26.
5. Avakian, 1956; Johnson, 1935, pp. 16–17.
6. Shomer, 1943, pp. 15–18, 21–23.
7. Avakian, 1956; Murray, 1946, p. 33.
8. Green, 1951, p. 444.
9. Murray, 1938, p. 183.
10. Dorothy Arnette and Shirley Wright, Negro informants in 1948.
11. Ruby Hunter and Horace Soward, Negro informants of 1956.
12. Ruby Hunter. Photos in Detroit News, Pictorial Magazine, June 3, 1956.
13. Murray, 1952, pp. 44–45.
14. Gorer, 1944, pp. 20–21.
15. Courlander, 1944, p. 37.
16. Oderigo, 1956, p. 317.
17. Chase, 1955, p. 256.
18. Kurath, 1951, p. 179.
19. Chase, 1955, pp. 83, 439–440.
20. Chase, 1955, pp. 436–437, quoting Lafcadio Hearn.
21. Chase, 1955, pp. 77, 312; Harris 1952, p. 49.
22. Harris, 1952, p. 48.

23. Photographic reproductions: for instance, in *Caribbean Quarterly* 3 : 1, Frontispiece (E. Bridgens' "Negro Dances"); and in Abby Aldrich Rockfeller Folk Art Collection, Williamsburg, Va., Cat. 301.29 (The Old Plantation) from plantation between Orangeburg and Charleston, S.C.

24. Chase, 1955, pp. 77–78.

25. Chase, 1955, p. 304; Terral, 1954, p. 9.

26. Harris, 1952, pp. 88–89.

27. A Labanotation record, *Better Dancing with Fred Astaire,* is in preparation, M. Witmark, New York.

28. Kurath, 1956, states various applications of choreology.

29. Hutchinson, 1954.

30. Chilkovsky, 1955, 1956.

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Other good selections in this jazz series:

Columbia Record Corp., New York
n.d. *Cakewalk to Lindy Hop* (CL782), (Wally Rose), *Charleston* I, 6, *Truckin'*, II, 5, *That Lindy Hop* II, 6.
1946. *The Great Benny Goodman* (CL820), I, 2, *Stompin' at the Savoy*. (Publ. in Murray 1946, 36–38, cop. 1936 Robbins). Suited to Jitterbug.
Jazztone Society, New York
n.d. *Jelly Roll Morton* (J-1211), *King Porter Stomp*, II, 5 and others. Suited to Two-step and Foxtrot.
n.d. *Rex Stewart* (J-1202), *Basin Street Blues*, I, 2 (Publ. *The Dixieland*, I, Melrose, 2–3, cop. 1929). Suited to Foxtrot or Duck Walk.
n.d. *Sam Price* (J-1207), *Jumpin' on 57th*, I, 1. Suited to Lindy or Big Apple.

METHODOLOGICAL PROBLEMS OF FREE DOLL PLAY AS AN ETHNOGRAPHIC FIELD TECHNIQUE[1]

David Landy

Increasingly during the past two decades anthropologists have been interested in projective techniques as devices for deriving "implicit culture patterns" or "modal personality structures" of various societies (Henry and Spiro 1953; Mensh and Henry 1953). Concurrently controversies have raged as to the validity, reliability, objectivity and practical utility of these instruments (cf. Henry 1954 and comments by Nadel, Caudill, Honigmann, Spiro, Fiske, Spindler and Hallowell).

In cross-cultural studies of socialization many techniques have been tried, usually with implicit assumptions that (1) they are more feasible and productive for studying child behavior than child interviewing or observation alone, and (2) this may be one of the few ways, aside from the difficult task of collecting accurate dream material, that the ethnologist may probe the unspoken premises underlying child behavior. The purpose of this paper is to present some practical and methodological problems of the application of free projective doll play to rural Puerto Rican children (Landy 1956) and to consider several questions relating to the validity and practicality of this mode of data-collection in particular and projective methods as field instruments in general.

So far as this writer knows, this is the first application of this specific technique in a culture outside the continental United States. In fact, the only other use of doll play as a projective technique in *any* other culture is that by Henry and Henry (1953), using dolls with detachable parts with Pilagá Indian children. The present technique was developed by Sears and Whiting and their associates at Iowa State University and Harvard. It was intended not so much for eliciting modal personality patterns as for specific kinds of child behaviors, particularly aggression and dependency. These were considered as consequents of antecedent home training conditions, which were obtained through interviews and observation. I hoped to use the technique for this purpose, but felt that it might be broadened to yield other behaviors which could not be reached by direct observation or interviewing and yield comparable data for cross-cultural testing of hypotheses.

ADMINISTRATION, SCORING AND EQUIPMENT

Four sessions of free doll play were administered to one child from each of 18 sample families systematically selected as representative of the predominant lower-class cane workers of the village of Valle Caña. The group comprised ten boys and eight girls between four and seven years of age.

The materials were constructed after we felt sufficiently familiar with the conditions of village life. They consisted of a roofless "house" formed of partitions three inches high, furnishings and an outhouse, to represent a "typical"

Vallecañese lower-class dwelling. The doll family consisted of a father, mother, boy, girl, and baby, with medium darkish skin coloring and features. The dolls were constructed of pipe-cleaners, cotton and cloth, and could easily be manipulated into almost any position by the child.

While the stimulus materials were standardized cross-culturally as to content, our attempt to make the form appear similar to life-conditions of the village detracts somewhat from the comparability of the technique. In the United States very elaborate "store-bought" equipment was set up to approximate a "typical" middle-class situation. Unlike ink-blots or nonsense words, whenever the stimulus has any form at all the anthropologist must decide whether to swap the advantage of keeping the form constant, or adapting it to the local situation. Thus, in the case of the Thematic Apperception Test, many variations have been used in exotic cultures. The same weakening of standardization must be realized.

The "act" was the unit of measurement, and was considered as any completed action, verbal or physical. Agents and objects of acts were scored. Categories of acts were: aggression, dependency, identification, nurturance, and noninteraction (an agent acting with no object in view). Content of acts was analyzed into toileting, cleanliness, sleeping, sex, and eating-feeding.

Each child was brought alone into the doll play situation. Present were the Puerto Rican field assistant, Miss Doris Díaz, previously trained in the technique by the anthropologist, who maintained the play of the child, and myself as scorer. I sat in a corner, presumably "working" and paying no attention to the proceedings, but actually simultaneously recording and scoring each act, plus notes on the child's general behavior. (In the laboratory situation in the States, only the experimenter is present, and the scorer operates from behind a one-way mirror. However, Pintler [1945] has tried the dual role of experimenter and scorer with reported success while attempting to assess the influence of experimenter-child interaction and the organization or lack of organization of the materials.) The fact that the scorer records and scores simultaneously has the advantage that scoring need not be deduced later from recorded data. But it has the disadvantage that only one scorer could be present and thus inter-judge reliability could not be tested. However, the ethnologist felt some comfort in the fact that in the United States it was feasible to use more than one scorer, and reliably high inter-judge correlations were obtained.

After a few months we began to administer the doll play, having first discovered in pretests that these children were often shy, negativistic, and reluctant to play in our presence. These first sessions proved somewhat premature and unrewarding, and doll play was temporarily abandoned. Finally, in the last two months of the field stay, we resumed with somewhat greater success, though four children cooperated to such a small extent that they were classed as non-participators and given extra intensive observation.

PROJECTION VS. REFLECTION

A question which arises in the use of most projective techniques is, "To what degree is the technique eliciting *projective* responses and to what extent are the responses *reflective* of the real life situation?" This involves the basic rationale underlying projective methods, and upon it hinges the validity and usefulness of the instrument. According to Bell (1948),

> Projective techniques derive their title from the term *projection*, which has a variety of meanings, some of which seem applicable as a partial description

of the processes involved in these techniques, and some of which are unsuitable. As yet, no clear and common definition of what is meant by "projective" has appeared among those who use these methods, although there is recognition of what is implied in the use of the term.

There will probably be consensus around the general notion that the subject will attribute to the material impulses and feelings which have been pushed down into his own unconscious. The idea, then, is that such a technique elicits socially repressed needs which cannot realistically be vented, but are expressed as fantasy: an ink-blot is a vulture carrying off its prey; a younger man "pleads for understanding" with an older man in a "neutral" TAT picture; the mother doll cooks dinner for the family or is stuffed into the trunk by the girl doll.

The difficulty is to determine how much of the derived data is projective and how much reflective, since obviously a different assessment must be placed upon each kind. As one researcher (Maccoby 1953) says,

> Evidently everything depends upon how far out on the reality-irreality continuum doll play is. If it is quite dissimilar to real life, then the inhibiting effects of punishment and restriction at home [for example] should be minimal, and the pressure of the need for fantasy should mean that the restricted and punished children will have a high rate of activity in doll play. If doll play is quite similar to the home situation, on the other hand, the punished and restricted children should show a *low* rate of activity in doll play, and should be stereotyped in their responses.

But for these Vallecañese children I am not sure that these assumptions always stand. Much of their doll play was of the very routine, very repetitive variety, and seemed to reflect the child's mundane existence. Perhaps the very fact that he took adult roles at all—behavior seldom encouraged by Vallecañese elders—indicated play at a fantasy level. This uncertainty, however, as to what extent the child's play is projection places a definite limitation on how much we can test hypotheses with it, or in fact find it useful at all. Thus, while a large proportion of the doll play seemed obviously replicative of routine home life, I could not be certain whether the remainder was actually projected or reflections of everyday life that I could not apprehend through observation.

For instance, the hypothesis that high punishment of children's aggression at home will tend to produce low overt aggression in real interpersonal situations, but high aggression in a permissive fantasy milieu like free doll play, assumes that the child will project his repressed wishes. A partial validity check here would be that if a punitive mother reports, and the ethnologist observes, low actual aggression, the child is probably fantasying when he produces high aggression in doll play. But suppose the opposite result obtains: we find low fantasy aggression associated with low overt aggression? This may throw doubt upon the projective capacity of the instrument or upon the reasonability of the hypothesis, or both. Which alternative does the investigator accept? In this instance further research indicated the possibility of a third variable, the superego development of the child. But the problem of just what it is that the child was exhibiting in doll play remains.

Furthermore, the question of how far out on the reality-irreality continuum doll play happens to be can probably not be answered in an absolute sense. It very likely is different for each child, depending upon his social experience and perceptive set. The path to the solution of this problem will undoubtedly prove a thorny one. Without some kind of answer, the displacement theory of projection is left dangling. And in a broad sense the displacement theory is *the* theory of projection, so that from this point of view all current projective techniques may be suspect.

EFFECTS OF A PERMISSIVE ATMOSPHERE

Another basic assumption of doll play, and of most projective techniques, is that the atmosphere surrounding the administration of the instrument must be completely permissive, so that the inhibitions which cause repression in real life will not be present to block free expression of needs in doll play. We did our utmost to insure permission. But certain facts of Vallecañese socialization make us feel that permissiveness alone was not enough:

(1) The child was never encouraged to act freely in the presence of adults and certainly not to interact with them on a basis of equality. So the child usually did not feel completely free in their presence. Perhaps he was consciously living up to the adult image of him as largely *sin capacidad* (with little ability to think or function autonomously).

(2) The child was not accustomed to completely permissive adults, who spoke to him without condescension and with an attempt at equality, who sat on the floor with him to play with a doll house and dolls (of all things!) in any way he saw fit.

(3) The child's response to being placed in such a situation was often negativism, a form of acting out hostility against such nonpunishing adults. This also accords with our finding that, given inconsistency of parental demands and punishment, the child tends to get away with as much as he can. Sensing that we deeply desired him to act, the child found himself in the unusual but pleasurable position of having adults wanting something of him instead of vice-versa, and being able to refuse their desires with impunity.

(4) The picture of two adults compromising themselves in such a manner must have been more fantastic than the doll play equipment itself. Our superordinate class status also must have introduced an indeterminable element of inhibition, despite our attempts to be utterly permissive.

TIME AND SPACE FACTORS

It is not possible directly to gauge the effects of the physical situation. For mountain children we used a cabin belonging to an old widow who was well known and respected, and this was probably advantageous. But because many children and adults would join us and try to look in through the windows and doors, we had to close them. In a community where this is never done in daytime this led to some suspicion as to what was going on in the cabin, even though we told everyone what we were doing. And a certain amount of fear must have entered the minds of the children inside, despite our attempts to be nonchalant and nonthreatening.

For the road children we used a room in my house. Here again certain factors must be taken into account. Seldom or never is a lower-class child invited into a middle- or upper-class home except when older, perhaps, as a servant. Suddenly to be made a guest in such a home must have created certain doubts and misgivings on the part of parents as well as children. Also it was not always opportune to remove my own child from the house, and sounds of her playing were often distracting.

We always informed a mother and child at least a day in advance of calling to take him to doll play. But as with her own interviews, a mother often forgot appointments *por la mañana*. So we frequently walked long distances to find the child was not ready and the mother would insist on washing him and changing his clothes, using much of our planned time. Sometimes the child balked at going, and his mother would request that we call back another day.

NUMBER OF SESSIONS

While two sessions seemed sufficient for obtaining productive play in continental youngsters, we often found that Vallecañese children were slow starters. So we extended the play to four sessions, assuming the first, and at times the second, as "warming up" periods. Thus we were comparing, in our cross-cultural analyses, four Puerto Rican sessions with two continental sessions, but since we used mean percentages, this does not seem unreasonable.

In an attempt to "warm up" the nonparticipators we tried some group play with the equipment, including some children who were good producers. This influenced some slow starters to produce more richly in their solitary sessions, but did not alter the performance of the original nonparticipators. However it should be noted that *they did participate to some extent in group play.* Also, especially at the mountain cabin where some space was available, we would lead group games, to relax the children before their solitary sessions.

THE BANDWAGON EFFECT

At first not only were most children reticent, but parents were not enthusiastic about letting them come. Vallecañese parents preferred their children close to home, and going off with adults, especially an *Americano* who strangely wanted them to play with dolls, was not a happy thought. Finally one mother with whom we had excellent rapport agreed to permit her child to go and we were confronted with the "bandwagon phenomenon."

Nearly every mother in the sample then insisted that her child be included, as well as those of other mothers whom we did not know as intimately. The latter would beseech us to take their children and hint that it would be a personal affront if they were excluded. (This also stimulated many of them into asking why they had not been interviewed, and they requested this, too.) Consequently we often had to take a child's friends and neighbors along, even though we permitted the non-sample children only briefly to examine the equipment.

In at least one case of a nonparticipator, we found after doll play was over that her mother, who was exceptionally punitive, forced her child to go, and this could have contributed to her extreme reticence in doll play. The final effects of the bandwagon phenomenon cannot be traced but they have been considerable. More than one mother may have forced her child to attend, unknown to us.

MOTIVATION

Presumably the child should be, and perhaps can only be, motivated in terms of his personal needs and cultural values. The prospect of playing with dolls and miniature furniture was not in itself enough to motivate these children, as a whole, to play freely. Like their elders, Vallecañese children were this-worldly oriented. So we tried candy and toys as motivators. This did raise their motivational level, but these young materialists came to want their pay in advance. There were even occasions when the shrewder children would accept a reward, play part of a session, then grin and say they would continue for another lollypop or rubber ball.

The effects of using such motivators are hardly measurable. Our feeling is that since getting paid for services is the general rule, except for class duties (for which they did not feel quite as obligated to us as to local middle- and upper-class members), perhaps this use of material rewards was not necessarily a contaminating factor. It is by no means unusual for American ethnologists, at least, to pay informants.

Another point in connection with motivation is the role of doll playing in the culture. So far as we could tell, doll playing was a normally accepted practice. However, few children in the impoverished lower class owned dolls, and on the rare occasion when one was bought, the mother often placed it out of reach, to be used only when the mother could protect it from harm. Furthermore, a good deal of stress is placed on manliness in boys, and on only one occasion outside of our own sessions did I see a boy playing with his sister's doll. Therefore, it may not be mere coincidence that three of our four nonparticipators were boys, though there are many other factors in Vallecañese socialization that could account for this (Landy 1956).

I have also wondered about the effects of trying to replicate the child's home situation. There was good evidence to indicate that the domestic environment, not only from my own cultural point of view but from that of the child himself was often perceived as drab and dull. Furthermore, as I have noted elsewhere (*ibid.*), much stress was laid upon the newness of a thing, however little intrinsic value it has. The questions then raised for the investigator are: (1) Did making doll equipment so much like real life defeat the requisite that a projective technique be dissimilar from reality in order to stimulate free fantasy? (2) Did the child feel depressed and inhibited by this microcosmic replica of his home? (3) If I had bought manufactured dolls and houses, however unlike the actual situation, would the child have been motivated to respond more freely and richly? (4) If we had ordered the children to play in the authoritarian manner in which adults usually address children in the culture, would they have responded more naturally and freely?

Space remains for one final question: Would I still use doll play in another field situation? While I by no means consider the data obtained without usefulness, I would not. It was seen that doll play was a much more time-consuming and arduous undertaking in the field than in the well-lighted sound-proof laboratory with one-way mirrors and tape-recorders in New England. In a subsequent study among the Tuscarora Indians (Landy 1955) I used a special child interview constructed by Whiting and others (n.d.) for cross-cultural field studies of socialization. While I have not yet analyzed the Tuscarora data, my impression was that the child interview probably elicited more relatable and valid material than doll play, and did not consume as great expenditure of time, energy and money. Professor Whiting has informed me that this agrees with the conclusions of the teams he sent into five societies to study socialization.

I agree with Nadel (1955) that while no projective technique currently in use should be indiscriminately administered by anthropologists, we still stand in need of such a tool. This necessarily implies that interested anthropologists will be willing to try new techniques, provided they are fully familiar with them and they have been shown to have some reliability and validity *prior* to field use. We can expect psychologists to evolve new techniques but it will be largely up to us to test their cross-cultural usefulness. Meanwhile I am also in agreement with Henry (1954) that primarily the anthropologist must depend upon the use of himself in participant observation, and must sharpen his use of the interview as the technique which most naturally lends itself to the collection of ethnographic data.

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Notes

1. This paper is based on an ethnological study of socialization in a rural Puerto Rican village. The author was assistant director of the Family Life Project, sponsored by the Centro de Investigaciones Sociales, Universidad de Puerto Rico, 1951–1953, and assumes total responsibility. The Laboratory of Human Development, Harvard University, collaborated closely and generously permitted use of its facilities and techniques.

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A NEW FRAMEWORK FOR STUDIES OF FOLKLORE AND SURVIVALS[1]

Margaret Mead

In 1952, the Wenner-Gren Foundation International Symposium on Anthropology (Kroeber, 1953) drew together scholars and scientists in the various fields of anthropology, and a new synthesis was attempted which—after a quarter of a century of divergent and uncoordinated development in the study of physical anthropology, linguistics, archeology and ethnology—has ushered in a new era of integrated research. But there is one discipline with a long and honorable tradition, with its own distinctive vocabulary and methodology, its own associations and periodicals, which, while included, showed little trace of cross-fertilization either with the other relevant anthropological disciplines or with other fields, such as modern history, dynamic psychology, ethology, and so on (Thompson, 1953). This paper is designed to present a theoretical framework within which the work of modern folklorists can be integrated with ongoing and developing theories of human evolution and cultural change.

In the period when the style of folklore research was developed, the attention of scholars was focused on macro-studies of cultural process. The aims of research were to trace paths of diffusion in order to establish historial connections, or to illustrate the psychic unity of mankind either by showing the differential receptivity of different cultures to different themes, or by demonstrating the recurrence of the same human themes where contact seems unlikely. The methods appropriate for this kind of research included the making of collections of versions of a tale or rite in different tribes, the consideration of evidence of culture contact, and the establishment of criteria for probable lines of contact and of ways of handling theme, plot, incident, and so on. This was the period of concordances; the methods have been ably summarized by Lindgren (1954). European scholars stressed change over time; American scholars stressed the spatial distribution of tales (Benedict, 1931).

In a second type of research, the themes and content of tales were used to specify the relationships between universal psychological characteristics of mankind and cultural forms. Recurrent human themes—the hero, conflict between father and son, the magic flight, animal husbands, etc.—were studied both as expressions of these universal characteristics and as particular forms expressed in the plots and themes of particular localized cultures (Roheim, 1934). Psychoanalysts (Rank, 1914) and students of culture and personality (Kardiner, 1939) drew upon folklore to establish statements about the nature of man's psyche or the character formation of a particular people.

Most of this work, whether it was done by folklorists or by those who drew upon their research, was characterized by a tendency to treat folklore in its narrowest and in its widest sense as anonymous. Preoccupation with the contrast between folk tale and myth, on the one hand, and the identified idiosyncratic products of the individual imagination, on the other, and the general tendency of the period to deal with macroscopic theoretical problems, together resulted in the recording of the tale without the teller, the rite without the practitioner, the covey of witches with no witch identified. Throughout this period, there has been a slender thread of emphasis upon the importance of identifying not merely the tribe or the village and the date but also the individual and the situation, but for the most part it has remained an inexplicit procedure buried in a developing field practice that has far outrun its theoretical formulation (Mead, 1958).

Meanwhile we have had an opportunity to explore the theoretical possibilities for an understanding of the processes of cultural change of studying fully identified individuals within an identified group followed over time. Research of this kind has been done on the Manus people of Peri Village in the Admiralty Islands, where the same individuals—fully specified—have twice been studied in a period of twenty-five years (Fortune, 1935; Mead, 1930, 1934, 1956; Schwartz, n.d.). From the study, in 1928–29, and the re-study, in 1953–54, we have gained new insight into the nature of cultural change (Mead, 1954). The change among the Manus—who were just emerging from a neolithic type of culture in 1928 and who, in 1953, had transformed their own culture and had modified the kinship system, the economic system, and the ethical system so as to accommodate the full pattern of western culture of school literacy, church, democratically functioning community council, modern medical ideas, and modern ideas of taxation, political responsibility and political membership in the modern world—was a patterned change which resulted in less individual and social disorganization than has been reported elsewhere in cultures where change was much less drastic and more partial and fragmentary. Two nativistic cults developed in the seven years since the social revolution, and it was possible to follow in detail the way in which fragments of dissociated beliefs—both from the old aboriginal culture and from partly understood, literal-minded Christian preaching—formed the nucleus for these cults which were essentially antithetical to the orderly inclusion of a people with a less-organized culture into the developing Pacific-wide culture of the twentieth century (Mead and Schwartz, 1957). The organized movement for change, on the other hand, with its emphasis upon sudden, complete change of pattern and upon the overall support needed to maintain and develop the new pattern, contained the necessary elements for integrating the small group of Manus-speaking natives and some of their neighbors into the wider polity, not only politically but also in terms of their entire system of values and aspirations.

The Manus material has raised several fundamental questions—about the extend to which rapid change may be more beneficial[2] than slow change, about the importance of self-transformation by a group which remains intact, and about the need to reëxamine the functions of survivals of belief or rite in a changing society. There have, of course, been many studies of African survivals in the New World (Herskovits, 1941), of witch cults in Europe, and of pagan ceremonies around the Mediterranean and elsewhere, and there have been many pronouncements, both scientific and partisan, as to the worth of such survivals, for good or bad. But such statements have been generalized for the whole culture or the whole society or the survivals themselves have been used as one measure of whether a society was indeed a "folk society" (Redfield, 1956), or whether a group of people should be called peasants or farmers.

It is the proposal of this paper that survivals should be studied in a new way so as to include not only localization in time and place for each tale or practice but also specification of the practitioners and of those who know and transmit the tale and of the conditions under which the survival is reënacted or transmited. This would mean that the *dramatis personae* of any rite would be fully placed in a defined social context. Are they aristocrats playing at ancient glory, or farm laborers whose symbolic dances appeal to some criminal or deviant elements from the fashionable world, or the unconscious collaborators of commercial criminals, or a small intermarrying group where the survival is the means of maintaining the too-narrow intermarriage system, or a group which because of habits of secrecy is a festering point of political subversion? The answers to such questions can come only from studies of the actual practitioners of some ancient rite and of their links with the wider world—of the conflicts and compromises with church, state, town council, the underworld, and so on.[3]

In Manus we were able to trace in the contemporary culture the present position of one small survival, one bit of black magic for killing infants. The fact that this magic survived was due to an anomaly. This bit of magic had no associated fetish objects and so, when the other practitioners threw away the material symbols of their magic powers, old Poli had no way of throwing his away. Meanwhile, the acceptance of modern medicine was not accompanied by sufficient modern medical care to prevent many infant deaths, and explanations in terms of this one existing charm began to creep back in. In one individual case after another, we were able to trace how people, who were differently placed in the rapidly transformed social system, reacted. Some had their infants precautionarily charmed in spite of the cost and the laborious taboos; some waited until their infants became ill; some maintained that the charm could not work evil unless a quarrel also was involved. Meanwhile, Poli had taught the charm—which was a source of income—to his son-in-law, who had become the support of his household when his own sons had proved to be unreliable. Concurrently, European doctors had developed the custom of telling patients that diseases which they could not cure were "something related to the place"; they meant that these were local tropical diseases with which they were unfamiliar, but the Manus and members of other tribes interpreted their statement to mean "caused by sorcery." Within this particular culture-contact situation, it will depend upon the behavior of identified individuals whether or not old Poli's one surviving charm becomes the festering center of a sorcery system of the type which so often has accompanied the conditions of culture contact—where the new more universal religion replaces the beneficent *white* magic or religious practices of a people and only the black magic survives, underground, to become hypertrophied in situations of ignorance, social submergence or social isolation.[4]

A number of careful detailed studies of personnel and situation would make it possible to reëvaluate much of the existing material and to consider what kinds of survivals, under what conditions, become the *foci* of nativistic cults (Wallace, 1956) which impede the integration of a given cultural group, which serve as felicitous reinforcements of local ethnic identity, and which tend to become so dissociated that, when diffused from one culture to another, they may traumatize generations of small children or serve to isolate further whole groups who think that, by joining some bizarre small sect, they are becoming part of one of the great religions of the world or are moving towards a respected position in the modern world.[5] It will be possible then to add new dimensions to discussions of whether blends between the liturgies of a pagan and a Christian society, or the double identification of deities—as part of a local Balinese village pantheon and as members of the Hindu pantheon, the choice depending upon whom the priest is speaking to (Belo, 1953)—are desirable and for what purposes. Geoffrey Gorer (1936) has pointed out the value for Balinese art of the way the Balinese did not believe in their religion, this dissociated attitude towards their religion being itself a by-product and a continuing condition of the very high incidence of survivals in Balinese culture. Detailed study of identified individuals in Bali (Bateson and Mead, 1942; Mead and Macgregor, 1951) makes it possible to relate the schizoid quality of Balinese character and the fragmentation of different historical traditions in Bali. Students of Middle and South America have documented the complex survivals into present Christian practice of pre-Christian elements, but it is the detailed studies of small communities, where the actual implications for individual lives and community functioning can be seen, that show the effects of clashes between the supporters of this traditionally sanctioned hybrid usage and a new type of missionary who attempts to purify the older practice, sometimes locking a large part of the community out of a church which they have lovingly adorned for centuries (Siegel, 1954).

I should like to give one detailed illustration of how one would look at the survival of European folk rites in the American children's festival of Hallowe'en in this new way. Using the older folklorist approach, we would trace this annual festival of the Eve of All Saints to its Christian and pre-Christian origins, explaining in historical terms the various traditional items—witches, ghosts, Jack-o'-lanterns made of pumpkins, their carved faces lit by candles, mischievous pranks and tricks, especially against the houses of lonely spinsters, tribute levied on the well-to-do by masked callers under threat of property damage, and a mass of divinatory practices like walking downstairs backwards carrying a candle and a mirror to see whose face appears over one's shoulder. We would note that activities that were once proper to adults are now the activities of children. We might point out the function in human societies of saturnalias, periodic festivals in which the normal rules of conduct are suspended and the normal positions of superordination and subordination are reversed. So children compelled all the year to respect property and to be polite to their elders, were given a chance once a year to express their suppressed impulses to ridicule and embarrass their elders and to destroy property.

Still using older methods, but recognizing with Malinowski that survivals have contemporary functions, we would analyze what is happening to Hallowe'en observances today as local communities through their organized representatives —Rotary clubs, chambers of commerce or tradesmen's associations—collectively buy off the potential destroyers of property with collective "treats" in the form of parades, free movies, free distributions of ice cream, and so on, while shopkeepers give prizes for the orderly decoration of shop windows which formerly would have been smeared and scribbled with soap.

Within the framework I am suggesting, the student would do something more. He would examine actual communities and identified individuals within them and would work out in detail just which children and young people went too far in their destructiveness, exactly who in the community took the lead in buying off the children, and whether, in this transformed celebration, the idea of potential mischievous destruction was still being carried along. The question might then be asked—and answered—whether this apparently innocent children's festival has not been the carrier of elements of deep hostility and of practices of vandalism which, in our present type of urban living, have been expressed not only in Hallowe'en practices but also in the waves of vandalism which have been sweeping the country. It would then also be possible to take a careful look at the latest attempt to domesticate Hallowe'en usage—that of having children go from house to house to collect funds for a United Nations' agency—and to ask whether this represents a final integration into the culture of a survival or whether this seemingly socialized behavior still retains some of the destructiveness of earlier forms.

We are entering a period of planned cultural change as millions of people start new lives as immigrants and refugees, as rural peoples migrate to the cities, as westernization and modernization are brought to the peasant and primitive peoples of the world. A series of really detailed studies of how particular survivals have functioned in particular communities as members of different generations have utilized them in different ways—for secret criminal subversive purposes, as the carriers of unacknowledged but valuable elements from an earlier culture, as a touchstone for the imagination of the artist, as the *foci* of a sense of identity—should provide a new level of meaning to the work of the folklorist and a new dimension to our understanding of cultural change.

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Notes

1. This paper was presented at the Congress in Philadelphia under the title "A New Theory of Cults, Survivals, and Culture Change," but as it is directed primarily to students of folklore, it has seemed advisable to alter the title.

2. The term *beneficial* can, of course, be used only when the value system of the commentator—in terms of such criteria as mental health, reduction of the infant death rate, reduced incidence of crime, increase in artistic or scientific productivity, etc.—is considered (Mead, 1953).

3. Margaret Murray's last book, *God of the Witches* (1953), was an attempt to do something of the sort historically, but the element of conjecture as to the repercussions of pre-Christian elements in the English court was too great to make this more than an intriguing speculation. What we need are contemporary studies.

4. I am particularly indebted to the work of Alan Lomax and to a film made last year on the hobby-horse cult, *Oss Wee Oss* (distributed by the English Folk Dance Society), for focusing my attention on the importance of studying the actual personnel involved in the practice of a survival.

5. The translation of Grimm's fairy tales and their use in English-speaking cultures with a very different character formation is an example of the first possibility, and the spread of small localized American religious sects into the West Indies, where they become identified with United States culture, is an example of the second (Métraux, 1954).

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A SYSTEM FOR DESCRIBING AND ANALYZING THE REGULATION OF COORDINATED ACTIVITY

Walter B. Miller

The analytic system I will describe here was developed in response to the problem of the ethnologist who goes to a non-European society and attempts to describe or analyze that range of data which falls under the heading of "Government" or "Political Organization." The ethnologist frequently discovers that a great many analytic categories developed in connection with European societies simply do not fit what he finds in non-European societies. The Europeans who first began to contact non-European societies in the 15th and 16th centuries were much struck by what they found, or failed to find, in the way of "government" in these societies. What they observed in many cases so obviously failed to correspond with European forms of government that they adopted the conclusion that these societies had no government and nothing political. In the strict sense of the word "political," in its original meaning, "that which refers to the socio-political organization of the Greek city-state," this was true. Most of the distinctive features of the Greek city-state, and thus of European governmental systems, were absent. These early observers generally adopted one of two apparently contradictory views: they saw the non-Europeans either as automatons, blindly conforming to tradition as if by some mechanical process, unthinking puppets manipulated by the iron hand of custom, or else as wholly capricious and without law, acting according to the whim of the moment or their passing childish fancies.

However, if one approaches the problem of "government" on a level other than that derived from the consideration of European-type governmental systems, it is evident that men in all societies are "governed." Each society has a systematic method of regulating and coordinating the behavior of groups of people. On this level, the similarities between the methods by which coordination is attained cross-culturally are far more impressive than the differences in the particular form in which these mechanisms are organized. On a reasonably objective level of analysis, the regulative systems of all societies may readily be encompassed by a common analytic rubric. Where, then, are the sources of the not inconsiderable difficulties encountered by European investigators in adequately handling the governmental or regulative systems of the so-called primitive or non-literate societies? I will cite briefly only a few.

The simple fact of belonging to a highly organized and complex society of itself induces a systematic perceptual distortion. With the specific cultural features of a given society serving frequently as a largely unrecognized baseline of comparison, characteristics are attributed to features of other societies that are primarily a function of the base of comparison and only secondarily related to the features themselves. The works, for example, of Max Weber, one of the most informed and sophisticated recent analysts of political phenomena, reflect the determining influence of his position as a European. Weber's discussion of the regulation of human activity centers around the consideration of "imperatively coordinated" systems of organization. An extremely brief section of his work is concerned with groups coordinated according to a different principle, treated under the heading "Types of Government of Corporate Groups that Minimize Imperative Powers." One of the sections under the above heading is called "Anti-authoritarian Forms of Government," carrying the implication that "authoritarian" forms constitute the norm. Weber does not call his European-type governmental systems "Types of Government . . . that Maximize Imperative Powers," an equally valid description if a different and equally valid point of reference were assumed.

Another factor making the objective treatment of governmental-type phenomena difficult is the fact that consideration of the area of government is highly charged with strong emotions. Most individuals in a given society have a deep commitment to their own society's system for regulating collective activity, and the strong influence of these values tends to impede objective study in this area.

Much more subtle and deep-rooted than the fairly obvious effects of ethnocentric bias is the influence on political thought of the Indo-European linguistic tradition and the particular way of handling phenomena it involves. As Whorf has so ably pointed out, the Indo-European linguistic system tends to divide the empirical world into a collection of separate and static entities, and to endow abstract ideas and relational concepts with the qualities of concreteness, solidity, and manipulability.

The school of thought dealing with political phenomena under the rubric "political science," "political philosophy," or "political sociology" is particularly prone to this influence. The analytic and descriptive method of this school centers on a set of artificial conventionalized entities, derived from the pattern of Indo-European linguistic systems. The core terms Power, Authority, Force, Influence, and Sovereignty are treated, with rare exceptions, as reified entities, as qualities or attributes having a real and independent existence which can move, grow, increase, decrease, flow, be possessed, be lost, be augmented, be divided up, and so on.

Lasswell and Kaplan, for example, speak of the "weight, scope and domain of power. . . . Increase or decrease in the 'amount' of power may involve a change in its weight . . ., scope, or domain. . . ." The conveyed picture of power is that of a very tangible physical entity with all the attributes of concrete matter.

In an admittedly inadequate attempt to cope with some of these problems, I have tried to develop a method of describing and analyzing a range of data relative to the regulation of human action that would be cross-culturally applicable. The descriptive purpose was primary, reflecting my conviction that the most basic need in current political theory is a method for presenting data on a simple descriptive level through a system of categories which do not reflect the values or biases of any particular cultural system. Such a set of categories would be applicable to the entire range of cross-cultural data and able to accommodate all societies, from the simplest to the most complex.

One result of my desire to minimize value considerations was an attempt to keep a firm conceptual separation between the level of objective description of observable phenomena and the level of motivation or value. To that end I have utilized a set of descriptive terms such as "coordination," "activity," "regulation," "authority role," "plan of action," that carry fewer and less intense value connotations than currently common descriptive terms such as "democratic," "authoritarian," "leadership," and "power." Secondly, I tried to employ a method that would permit a description of regulative processes involving minimal treatment of motivational factors or psychological dynamics.

A further aim was to present a "middle level" analytic system, whose categories would be close enough to empirical data to be readily applicable to concrete situations, and at the same time general enough to permit adequate relational comparisons and analyses.

The system I will describe here is a highly condensed version of an account presented elsewhere in considerably greater detail and with many more concrete examples. Thus, in addition to the inadequacies inherent in the overall formulation, the schematic and condensed nature of the present account introduces a measure of obtuseness and incompleteness not intrinsic to the system itself.

A METHOD OF DESCRIBING COORDINATED ACTIVITY

This system of description and analysis takes as its point of departure the existence of coordinated group activity. It differs from those approaches which use either concepts such as power or authority, or organized groupings such as the state or tribe, as starting points. The term "coordinated activity" will be used to refer to that type of collective human action in which the individual rates of activity of the several members of an acting group are in some way mutually interadjusted.

"Coordinated" action can be distinguished from individual action, collective action, and interaction. Individual action can be carried on in the absence of a collectivity; a solitary hunter, a lone ploughman, or a single craftsman can achieve his objective operating alone. Collective action may or may not involve coordination; in non-coordinated collective action a group of individuals may work or act in proximity and be involved in a common enterprise, but their separate actions are not temporally articulated; each works at his own rate. Examples are a scientific laboratory with a number of independent researchers, a group of agricultural workers or food gatherers working in the same field or area, each at his own rate, or an orchestra with each musician "warming up" before the start of a concert. Interaction involves responsive action; the actions of individual B are partly determined by the actions of individual A. Examples are a chess game, a tug-of-war, a duel, a round-table discussion.

For purposes of description and analysis, the flow of coordinated activity can be broken down into sub-sequences. The analytic "unit" of coordinated activity is to some extent arbitrary and to some extent clearly marked by observable boundaries. This unit, which is somewhat analogous to the phoneme in linguistics, can be called the "activity episode." An activity episode is a subsequence of activity cut from the continuity of ongoing coordinated action. As in the case of the phoneme, the activity episode is defined by a combination of two factors; one factor is the observable physical events that serve to segment the flow of activity; the other is the relation of the sub-sequence to the broader "field" of coordinated activity of which it is a part.

In practice it is usually a relatively simple matter to isolate clearly defined episodes of a more extended activity sequence. The English language contains many terms referring to specifically delimited episodes of various areas of coordinated activity. Among these are the "period" (education), the "set" (tennis), the "course" (banquet), the "shift" (industry), the "round" (boxing), the "inning" (baseball), the "session" (legislation, other activities), etc.

Episode Markers. In some coordinated activities such as a march, building construction, or a political rally, definite episodes can be distinguished which, unlike the above examples, are not specifically named. Specific sub-sequences in such activities, as well as in activities with named episodes, can be designated with considerable precision by referring to the observable events that serve to separate one episode from another. Those events that separate successive episodes can be called "episode markers."

In cases where the beginnings and endings of activity sequences are less clearly marked, the limits of the activity to be considered can be specified by designating the episode markers that serve to bound the total activity sequence. A larger activity sequence of this type may be designated a "unit activity."

Types of Episodes

In all societies recurrent sequences of coordinated activity form part of the flow of organized societal life. These activity sequences are staged repeatedly in essentially the same form. Examples of activity sequences common in American society include the concert, the play, the football game, the convention, the surgical operation, the courtroom trial, the religious service, the parade. Because they are recurrent, sets of episode markers delineating beginnings, endings and included episodes for a range of coordinated activities are furnished by the cultural tradition of all societies. The nature of particular sets of episode markers and of the events comprising the episodes, however, varies from society to society.

Once demarcated, activity episodes can be typed according to the extent to which the events comprising them have been determined in advance of their execution. Using as an analytic dimension "extent of predetermination," a dimension importantly related to the authority involved in the regulation of coordinated activity, two kinds of events can be distinguished; those which occur in accordance with a plan of action formulated prior to the start of the episode, and those which do not. All activity episodes contain both kinds of events, but the proportion of one to the other differs in different kinds of episodes. Four kinds of activity episodes can be distinguished according to the proportion of predetermined events they contain; "ritualized," "prescribed," "controllable," and "indeterminate" episodes.

The great bulk of events comprising ritualized episodes has been determined in advance of execution by a tradition-derived plan of action; the primary end of actors is to execute activity with maximum conformity with the traditionally-formulated plan of action (The Hail Mary; Military "Retreat" ceremony). Prescribed activity episodes are also executed in conformance with a previously formulated plan of action, but one which has been produced expressly by known authors (act of a play, movement of a symphonic selection). In controllable and indeterminate events the order of event occurrence is not determined beforehand. Controllable episodes differ from indeterminate episodes in that all events comprising the controllable episode can be controlled by participants (chess move, tennis shot, jazz lick), while indeterminate episodes contain some events that cannot be so controlled (dice-game throw, abandon ship operations).

Any unit activity sequence can be characterized according to the proportion of these four types of activity episodes it includes.

The Action Group. Once an activity is described and characterized in terms of its component events and included episodes, the salient characteristics of those participating in the activities can be cited. Note that the description of activity participants is logically subordinate to, and temporally subsequent to, the description of the observed events of coordinated activity. The characteristics of groups participating in coordinated activity vary widely, corresponding to the wide range of variation in types of coordinated activity. Features of the group such as size, degree of segmentation, amount and kinds of role differentiation, significant characteristics of members, and the "permanency" of the group as such may be cited.

Action groups may vary in the extent to which they are composed of identifiably separate segments and in the degree of internal role differentiation segments exhibit. At one end of the scale are groups with one or a few distinct segments and little or no role differentiation (Rockettes, sculling crew), at the other, groups with many segments and much internal role differentiation (ship's crew, construction-company workers).

THE REGULATION OF COORDINATED ACTIVITY

The method for describing and analyzing coordinated group activity so briefly outlined here, while it can serve to delineate a field of data of great interest in its own right, was developed primarily to serve as a basis of an operational method for identifying and describing the set of roles in a society by which coordinated activity is effected.

Once the question "What is done" is answered, the question "How is it effected" can be asked. Individuals engaged in coordinated activity perform a wide range of actions. Some relate primarily to the task objective of the activity, others primarily to interaction with fellow participants; many involve both objectives. In any case, effectively coordinated activity demands that there be some method by which participants may know what it is they are to do. In many activities a designated member or members of the action group perform the special job of communicating to participants what actions they are to take at given times. In other activities, participants act in the absence of such direct communication. The totality of measures by which methods of procedure are communicated to participants in coordinated activity can be called the regulation of coordinated activity. As when describing the coordinated activity and the action group, the question "What is done?" is asked before the question "By whom?"

The delineation of the system of roles in a society through which coordinated action is effected can be achieved by the use of two concepts—the *regulative function* and the *regulative agency.*

An action or set of actions performed to coordinate collective action can be called a "regulative function." Four sets of major regulative functions can be cited: *formulative* functions, *expositional* functions, *directive* functions, and *adjustive* functions. Two or more subordinate functions are included in each set.

Regulative Functions

Formulative	*Expositional*	*Directive*	*Adjustive*
Selection	Instruction	Ordering	Adjudication
Formulation	Exemplification	Signaling	Mediation
			Penalization

Formulative Functions. Formulative functions are concerned with derivation of the plan of action governing the execution of a given activity sequence.

The basic formulative function is that of selection. Selection involves choosing one alternative procedure or item from a number of available procedures or items. The extent to which selection is necessary and the duration of the procedural sequences selected depend on the degree of permitted latitude associated with the involved activity.

Where longer sequences are selected for execution at a specified time, the plan of action governing that sequence has generally been drawn up sometime prior to selection and by persons other than those who make the selection. The process of originating a plan of action can be referred to as "formulation." Formulation always involves selection and arrangement, but selection can be independent of formulation.

Collective formulation is frequently called "decision-making." Activity sequences involving collective formulation or selection may comprise included episodes of longer activity sequences (pitcher-catcher-first-baseman "huddle"), or may themselves comprise a unit activity (legislative session, board meeting).

Expositional Functions. Expositional functions are performed to impart knowledge of methods of procedure to participants in coordinated activity. Such exposition may concern the specific procedures of a given episode (briefing football players on the details of a play), or may involve more generalized rules of procedure (pre-fight instructions to boxers). Instruction is a generalized and direct form of exposition.

Exemplification is a form of exposition wherein methods of procedure are demonstrated by the actual performance of a person or group. Verbal instructions may accompany exemplification ("Do it this way"), or may not.

Directive Functions. Directive functions are exercised to initiate given activity sequences and to maintain the continuity of activity. The regulative function that operates most directly to trigger action is the order. An order communicates to the participant in coordinated activity what action he is to take.

An order may also be communicated by means of a non-verbal signal. The policeman's whistle may mean "Stop" or 'Go"; the orchestra conductor's downbeat represents the order "Start playing"; the bugle call "reveille" means "Everybody get out of bed."

Adjustive Functions. Adjustive functions are concerned with securing and sanctioning adherence to procedural rules and directives governing a coordinated activity. Non-adherence to orders or procedural rules may be intentional or inadvertent. In either case adjustive functions are exercised to minimize non-conformity to directives.

The adjudicative function is exercised to decide which of a range of alternative events is permissible during a given episode (ruling by chairman: proposed action is "out of order"; professor: "books may be used during the examination"), which of a range of permissible events has occurred (baseball umpire: "Strike"; athletic contest judge: "Runner A was first"), or whether unpermissible events have occurred (boxing referee: "Low blow"; orchestra conductor: "Wrong note"; examination monitor: "Talking").

The penalizing function involves the selection and application of penalties for non-adherence to procedural rules. Penalties may take many forms, ranging from mild rebuke to physical punishment.

Mediative functions are exercised to reconcile conflicting formulations or selections and to prevent the occurrence of such conflict. In adjudicative mediation or arbitration, an attempt is made to find some middle ground between opposing interpretations of procedure of conflicting formulations.

Regulative Agencies. The range of regulative functions necessary to coordinate collective activity is performed in all societies. Specific functions and sets of functions, however, are allocated to designated regulative agencies in many different ways. For this reason a consistent conceptual distinction between the functions performed and the agencies performing them is necessary to accurate comparative analysis of regulative systems. Two types of regulative agencies can be cited: the authority role and the authority organ.

The Authority Role. Regulative functions associated with the coordination of collective activity may be performed by incumbents of specially designated positions, referred to as "authority roles." An authority role may be defined as a conceptualized position within a system of interpersonal relations whose incumbent is authorized to perform designated regulative functions for a designated action group during designated activity episodes.

An authority role may or may not have a specific name. Most authority roles associated with American activities are named. Examples from American society are drill sergeant, coxswain, orchestra conductor, ship's captain, foreman, football coach, presiding officer, cheer leader, etc. A named role may be associated with a specific activity (quarterback, football) or may be generic (boss, of office staff, road gang, logging crew, etc.).

The range of regulative functions associated with any given activity may be performed by the incumbent of a single authority role, or may be allocated to a number of roles. Any authority role may be described in terms of the regulative function or functions its incumbent is authorized to perform. An authorized function is one whose exercise eventuates in intended responsive action by those to whom it is directed. An authority role may be thought of as a set of authorized functions assigned to a particular incumbent; the set of functions has a persisting conceptual existence independent of any particular incumbent. Individuals subject to the exercise of authorized functions accept the right of the role incumbent to perform them; the incumbent accepts his responsibility to perform them.

Allocation of Functions to Roles. The range of functions associated with the execution of a given activity sequence may be allocated in different ways. Some activities may be coordinated by the exercise of one or a few regulatory functions. These functions may be performed in the absence of a specific named authority role (activity: bridge game; regulative functions: signaling; associated authority role: none), by a single-role incumbent (activity: crew run; regulative functions: signaling; associated authority role: coxswain), or infrequently by a number of role incumbents (activity: grandstand cheering; regulative functions: selection, signaling; associated authority roles: head cheer-leader, cheer-leader).

Where a wide range of regulative functions is performed, these may be allocated to one or a few authority roles (activity: Boy Scout hike; regulative functions performed: formulation, selection, exemplification, instruction, ordering, signaling, adjudication, mediation, penalization; associated authority role: scoutmaster), or to a number of roles.

The Authority Organ. When regulative functions are performed by an organized action group, such a group can be referred to as an "authority organ." A frequent type of authority organ is one whose primary function is formulation or decision-making (council, board of directors, legislature, "staff"). Members of such a group act collectively to formulate details of activity or to draw up extended plans of action. Details of activity formulated by such an authority organ are generally executed by a different action group, although some or all of the decision-making group may also be involved in execution.

In more complex societies each specific regulative function or set of functions is generally performed by a separate corresponding organ or system of organs; in less complex societies the full range of regulative functions may be performed by a single organ (council of elders); in the simplest societies these functions may be performed in the absence of any specifically designated authority organ.

SUMMARY

The approach underlying the system I have described is based on an analytic "field" whose primary parameter is coordinated group activity considered as a sequence of events in time. The basic units of analysis are temporal units. Observable continued action is the point of departure for all subsequent description. The basic question asked is "What is done?"; the question "Who does it?" is analytically subordinate. This represents something of a departure from most prevalent methods for analyzing social systems, which take as their point of reference the organized group, considered as a systematically related collectivity.

The two basic analytic units used in this system are the "activity episode," a temporally delimited sequence of coordinated action, and the "regulative function," an act or series of acts performed to effect the coordination of group action. Both these units are units of process or activity rather than structural components of organized group relationship systems. The utility of this approach derives in part from the fact that while the form of political groupings in various societies may vary widely, the functions performed to coordinate activities are analogous for all societies. Using the "function" and "episode" as analytic units makes it possible to include coordinative phenomena from all societies under the same rubric. While these units encompass data sufficiently specific to permit cross-cultural comparison, the distinctions they make are not so fine as to involve a proliferation of data on a highly specific level.

The field of interest delineated by this approach is not coterminous with that of any existing disciplinary field. It cross-cuts the existing rubrics of "social control," "law," "political organization," and "government." It focuses on a range of data which all of these areas deal with and none subsumes entirely. It selects and organizes data around the process of coordinated group action, in whatever segment of society this may appear—from the nuclear family to the whole-government regulative system. The merits or defects of this approach rest on an evaluation as to whether the range of data so selected is a useful one.

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TYPOLOGY IN THE AREA OF SOCIAL ORGANIZATION

George P. Murdock

The first service of typology in any science is that of bringing order into masses of descriptive data by classing together phenomena possessing common characteristics that suggest similar scientific explanations, and by differentiating such categories from others on the basis of unlike characteristics which suggest the intervention of other factors or principles. Just as the proof of a pudding is in its eating, so the test of a typology is in its use. If research oriented by its categories and their criteria proves consistently productive of new knowledge, a typology is validated through its utility. As an example of such a useful typology we may cite the periodic table of chemical elements proposed by Mendelyeev, the empty cells of which have, I believe, now all been filled by newly discovered elements possessing essentially the predicted characteristics.

Innumerable typologies have been proposed, of course, which have proved unproductive because of the selection of common and differentiating criteria irrelevant to scientific problems. Unproductive typologies are a stock in trade of philosophy as opposed to science. Some possess sufficient esthetic or intellectual appeal that they gain temporary standing even in the sciences. Notable among these are the paired polar ideal types so popular in sociology, of which a few have acquired a following in anthropology, to wit, the cooperation-competition antithesis and the folk-urban dichotomy. None, to the best of my knowledge, has warranted general scientific acceptance by serving as the springboard to significant new discoveries.

In the area of social organization, more than in any other sub-field of anthropology, typology has proved its utility by stimulating a long and almost uninterrupted succession of major new scientific achievements. Through the contributions of men like Morgan, Tylor, Rivers, Kroeber, Lowie, Linton, Spier, Kirchhoff, Radcliffe-Brown, Steward, Eggan, Levi-Strauss, Goodenough, and many others, we now possess a truly remarkable series of viable criteria for classifying, differentiating, and interpreting types of family organization, kin and local groups, kinship terminology, and patterns of behavior among kinsmen in societies throughout the world. In a recent paper,[1] where I noted this fact, I also called attention to the marked tendency in recent research to view typology in a less static manner than heretofore, and to pay closer attention to its integration with dynamic processes like those of socialization and culture change. This is a normal phenomenon when scientific advance has reached a particular phase of development. To cite but a single example, the Linnaean classification of living organisms, which had a strongly static character when first propounded, is today much more closely coordinated with dynamic processes like those of variation and natural selection, and particularly the varied mechanisms laid bare by modern research in genetics. In order not to repeat myself, I propose here to examine somewhat more closely the specific relationship between typology in social organization and the dynamic processes of cultural change.

Any similarity between two or more societies, in social organization as in any aspect of culture, is susceptible to only one of three possible explanations or to some combination of the three, namely, migration, independent convergence, or diffusion. Chance may be ruled out as a fourth explanation, since the invocation of chance is never more than a confession of ignorance, and all chance similarities between cultures would necessarily turn out, if we knew enough about the facts, to be reducible to one or more of the three categories just specified.

A substantial proportion of the literature concerning the roles of migration, convergence, and diffusion has failed to relate them adequately to the central dynamic mechanisms by which cultures are both perpetuated and modified over time. At the risk of stating the obvious, I must summarize the basic factors involved in cultural dynamics, since some of the peculiar characteristics of typology in social organization have their origins at this level. All the factors involved can be reduced to three basic ones—man's original nature, the environing conditions encountered by the particular society at a particular time, and the culture held by that society at the time it faces the conditions in question. By "original nature" we mean the anatomical and physiological characteristics and the innate capacity and mechanisms for learning with which all members of the human race have been endowed through millennia of organic evolution. These determine man's basic needs and the range and limitations of his capacities for action. The environment presents a range of possibilities for the satisfaction of needs and a series of limitations on the range of successful responses. Learning relates the behavior potentialities of man to the resources of the environment by establishing as habits the kinds of responses which satisfy needs. Such of these as are generally accepted constitute the culture of the society. Culture, in turn, generates acquired or derived wants and techniques for fulfilling them.

Since needs and wants are never fully satisfied, and environing conditions are subject to constant fluctuation, cultural techniques do not remain static. They undergo constant variation as attempts to improve them are tested out and as changes in the environment block off old channels to satisfaction and compel efforts to find new ones. The whole process is one of adaptation, as surely as is that of organic evolution. There is no escape today from conceiving of culture as fundamentally functional. But to regard cultures as wholly functional is neither wise nor sound, since every society is at any moment in the process of trying out new solutions to old problems and of attempting to discover primary solutions for urgent new problems.

The role of the existing culture, or what is sometimes called the "cultural base," in the normal process of cultural transmission and change deserves especially close attention. Unlike the subjects of the experimental psychologist, who are invariably selected from individuals who have had no known experience relevant to the test situation or whose prior experience has been carefully controlled, human societies face any situation in which new adaptive behavior is required equipped with an enormous mass of prior experience stored up in their culture. Portions of this are certain to be relevant to any new situation, providing elements which can be synthesized to produce particular innovative responses and interposing definite blocks to the appearance of others. Thus British culture, transplanted to the New World, provided the elements from which the American Constitution could be composed and, at home, offered insuperable obstacles to the development of polygyny as an adjustment to the shortage of men created by two world wars. The role of the cultural base in innovation explains why independent technological inventions are commonplace in closely related societies, such as those of the Western world, but are extraordinarily rare in societies with markedly different cultural backgrounds. One thinks immediately of the dome among the Eskimo and the ancient Romans, and is then hard put to think of even a half dozen other fully authenticated instances.

In the area of social organization the cultural base has several distinctive characteristics not ordinarily found in other segments of culture. In the first place, it includes far more elements that are common to all known societies than is generally the case. I refer to the universality of marriage, of the nuclear family, of the common domicile of husband and wife, of their economic and sexual association, of their joint responsibility for the early care and socialization of their children, and of the prohibition of incest between parent and child and between siblings. As opposed to this array of universals, I can think of only one trait of comparable complexity in the area of technology which is equally widespread, namely, the technique of manufacturing thread or cordage.

Secondly, the interplay of universals in the field of social organization results in a sharp limitation in the possibilities of variation. Forms of marriage are limited to three alternatives: monogamy, polygyny, and polyandry. Rules of residence are limited to four alternatives or combinations thereof: patrilocal, matrilocal, avunculocal, and neolocal. Rules of descent are limited to three: patrilineal, matrilineal, and bilateral. There are only four alternative ways of designating uncles and aunts, six of designating first cousins, and so on throughout the range of social organization. By contrast, alternatives tend to be exceedingly numerous in most other aspects of culture. In the field of folklore, for example, the number of possible themes, characters, plots, and incidents is practically limitless. An important result of the limitation of possibilities in the area of social organization is that any society which for any reason modifies any basic feature of its structure can only exchange it for another which numerous other societies have already independently adopted.

Thirdly, in social organization as opposed to most other aspects of culture, societies are normally compelled to choose among positive alternatives. Every known society possesses some form of marriage and family organization, some type of kin group and kinship system, some rule of residence and of descent. There are none which completely lack marriage, kinship terms, residence rules, and the rest. Elsewhere, however, the choice ordinarily lies, for example, between realistic art, abstract art, and no art at all; between various modes of warfare and no war at all; between different types of political structure and no political as opposed to familial institutions; etc. The student of social organization is thus placed in the enviable position of not having to cope with absences or negative alternatives in constructing his typologies.

We may now return to the analysis of the three possible interpretations of similarities in social organization in different societies—migration, convergence, and diffusion. Migration presents no real problem. Any resemblance due to migration is a genuine genetic similarity, produced by the same processes of transmission as prevail within any single society. From the point of view of the dynamics involved, the similarities between modern New Zealand and Great Britain, for example, differ in no significant respect from those prevailing in different parts of Great Britain or between one generation of Britishers and the next.

In the case of convergence, however, we are dealing with the manifestations of two quite independent genetic processes which reveal points of basic similarity due either to man's common original nature, to likenesses in the environing conditions in the two societies, to common factors or predisposing features in the two cultural bases, or to some combination of the three. The two manifestations have no genetic relationship. They are merely parallel adaptations, like the bats and the birds in zoology.

Now how about diffusion? Anthropologists have been prone to assume that similarities due to diffusion or cultural borrowing, like those due to migration, bear a genetic relationship to one another rather than a relationship of parallel adaptation.

It is a major thesis of this paper, however, that, in the area of social organization at least, diffusion bears a much closer relationship to convergence in the dynamics involved than it does to migration, and that it is, in essence, only a special case of convergence. In migration, the transmission of culture is wholesale and complete; modifications appear only after the movement has occurred and consist at first exclusively of changes forced by the new environing conditions. In the case of diffusion, however, the transmission is partial and highly selective. A people borrows from its neighbors only what its cultural base is prepared to accept and, among such elements, only what its members have reason to feel will satisfy their wants better than existing practices, and, among such, only the elements which actually prove, after trial, more satisfying under the environing conditions. In actual fact, the presence of other peoples with differing cultures in the vicinity is reacted to as is any other aspect of the environment, as a source to be selectively drawn upon for innovations which may bring superior adaptation. As Malinowski long ago pointed out, cultural borrowing is a creative process having much in common with other types of innovation. We must therefore conclude that, from the point of view of the dynamics involved, similarities resulting from diffusion and from convergence form a single class, and that those resulting from migration fall into a separate class.

If this is true in general, it is particularly true within the area of social organization. An extended family, a patri-sib, or a Crow system of kinship terminology obviously cannot be borrowed, given a quick trial, and then accepted or rejected in accordance with the results in the same way that this can and regularly does happen with, for example, a steel knife, a new cultivated plant, a cigarette, an item of foreign clothing, or a new song, ritual, or folktale. A change in social organization involves a wholesale readjustment of interpersonal relationships, and can normally come about only piecemeal as individuals react one after another to the pressure of new circumstances which render traditional patterns of behavior less satisfying than formerly. It seems probable, therefore, that modifications in social organization ordinarily occur only through the normal genetic processes of adaptive change. Similarities in social structure found in different societies are thus much more likely to be due to migration or to independent convergence than to diffusion. Consequently diffusion should never be invoked as an explanation except in the presence of very strong corroborative evidence.

Does the existence of uniformities in social organization over large and contiguous geographical areas constitute such evidence? By no means necessarily. If the tribes of the area are closely related linguistically, like the Muskogeans of the Southeast, the Central Algonkians, or the Basin Shoshoneans, a genetic relationship through fission and migration is ordinarily much more probable. Even where linguistic differences rule out such an interpretation, parallel adaptation to similar environing conditions is always a possibility.

The similarity in conditions may often, of course, be attributable to diffusion in some other aspect of culture, e.g., the economic, to which different peoples have then reacted independently by modifying their social organization in parallel directions. In Africa, for example, the introduction of cattle among agricultural peoples who have previously lacked them has repeatedly precipitated, in remote as well as adjacent regions, parallel trends toward the adoption of a high bride-price, patrilocal residence, and patrilineal descent.

Occasionally the presence of alternative adjustments in the social systems of adjacent societies provides concrete evidence that they have evolved their similar structural forms independently. Thus the Navaho, who certainly borrowed their agriculture and sedentary mode of life from their Pueblo neighbors, and who equally certainly exchanged their earlier bilateral social organization for a matrilineal one of the same gross type as that of the Western Pueblos, clearly did not copy their neighbors in this respect, for the details of their social system are markedly different. In abandoning their earlier Hawaiian cousin terms, for example, they did not borrow the Crow terminology of the Hopi and Zuni but evolved the alternative and equally adaptive Iroquois pattern. What presumably happened was that their preexisting rule of matrilocal residence, given the newly adopted sedentary mode of life, produced permanent local aggregations of matrilineally related kinsmen which could now crystallize into matrilineal descent. The process paralleled that among their Pueblo neighbors, but was independent.

As a matter of fact, I can think of extraordinarily few cases anywhere in the world where the balance of probabilities seems to favor the direct borrowing of significant features of social organization. All of them involve a special mechanism, which was first isolated by Bruner[2] in a community of Mandan-Hidatsa Indians. Through genealogies Bruner was able to show that every member of the community who had adopted the Eskimo pattern of kinship terminology from the neighboring whites was himself the descendant of an interracial marriage. The pattern, in short, had been originally acquired from a parent through the socialization process and had thereafter been transmitted through the same mechanism. There was no evidence of cultural borrowing at the adult level.

There are not a few instances where diffusion in social organization has demonstrably been mediated through intermarriage. Thus the Kunta Arabs of the Timbuktu region, who have in the past often taken Berber wives, have adopted the Berber practice of monogamy in the face of the general Arab preference for polygyny and the specific Koranic sanction thereof. The Ngoni of Nyasaland, descendants of invading Zulu warriors who intermarried with the local women, have abandoned much of their ancestral patrilineal structure and have adopted many of the matrilineal features of the societies from which they took their wives. The conquering Caribs of the Lesser Antilles likewise borrowed various matrilineal traits from the local Arawak women whom they married. The matrilineal Yuchi of the American Southeast, who lived during part of the contact period in the same settlements as the patrilineal Shawnee and presumably intermarried with them, are known to have exchanged their original Crow kinship terminology for one of Omaha type resembling that of the Shawnee. What characterizes all of these cases is the fact that diffusion was mediated through the socialization process. The elements of social organization were borrowed through the very same mechanism by which they are normally transmitted from one generation to the next.

A similar situation probably prevails in those areas of the world, like native Australia and the Northwest Coast, where custom prescribes local exogamy. In such cases every marriage unites representatives of at least slightly variant local cultures, and children grow up exposed to both and capable of selecting between them. Under these circumstances, elements of social organization can, without difficulty, spread gradually from group to group to the limits of the area. The conclusion seems justified, therefore, that genuine diffusion of significant features of social organization can occur only where intermarriage places the mechanism of socialization at its disposal. When this cannot be demonstrated as probable, all similarities in social structure between different cultures must be attributed either to fission and migration or to independent convergence.

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Notes

1. Murdock, G. P. Changing Emphases in Social Structure. *Southwestern Journal of Anthropology,* 11 : 361–370. 1955.
2. Bruner, E. M. Two Processes of Change in Mandan-Hidatsa Kinship Terminology. *American Anthropologist,* n.s., 57 : 840–850. 1955.

AESTHETICS IN "PRIMITIVE" SOCIETIES

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Two of the aims of ethnology are to establish the range of variability in cultural forms possessed by the societies of the world and to discern the regularities of process and the universals, if any, among these forms. For many aspects of culture these aims have been realized, or at least the methodological procedures to be followed are becoming clear, *e.g.*, social organization. We now possess a wealth of descriptive and analytical materials on many hundreds of distinct cultural systems with which hypotheses concerning culture have been and are being tested. But in all this there is very little which makes it possible for us to speak with any degree of conclusiveness or sureness about aesthetic beliefs or standards among the so-called primitive societies. In making this statement, I use the word "aesthetics" in its dictionary sense of referring to the branch of philosophy dealing with the beautiful, chiefly with respect to theories of the essential character of the beautiful and the tests by which the beautiful may be judged. In short, though the ethnographic literature contains much about the graphic and plastic art *forms* from many primitive societies, it yields little direct information on what ideas the members of these societies hold concerning beauty or aesthetic worth on the criteria by which they judge these forms. Perhaps my complaint, and the main thesis of this paper, can be made more lucid with an analogy: if we inquire into the ethnographic literature on some such issue as disease and its treatment we can find not only a wealth of data on the cultural forms employed in various societies but also a great deal of reliable information as to what the members of these societies believe to be the nature of disease, what their philosophy on this subject is and on what premises their logic concerning it is based. The same literature, if approached with the issue of art and aesthetics in mind, yields much technical detail about the art forms, usually well illustrated, considerable interpretation of the symbolic aspect and penetrating functional analysis of art and the artist in his or her society, but almost nothing about the aesthetic beliefs which these artists had in mind while they worked or which they used as a basis of judgment of their fellows' work.

This lack is all the more surprising in view of the fact that anthropologists have long been prominent in the writing of books and articles about the arts of primitive peoples—the names—Boas, Adam, Sayce, Herskovits, Linton, Kroeber, Weltfish, Bunzel and a host of others come immediately to mind, and Inverarity lists some 60-odd titles by anthropologists for the years 1952–53–54 alone in his brief survey article on "Anthropology in Primitive Art" which appeared last year. In all of this writing, anthropologists have long since made it clear that the work of the adult artist in a primitive society is *not* to be equated with that of children in our own, or that it is not representative of an arrested state in human aesthetic possibilities, but, rather, that the graphic and plastic arts of each society, primitive or otherwise, are the result of independent developments, each of which is historically valid in its own right. Meanwhile, aestheticians, philosophers, art historians and dilettants have continued to proffer interpretations of primitive art, most of them inaccurate and some of them ridiculously ethnocentric: universal symbolism is assumed; primitive art is facilely equated with folk art of Euro-American societies; or it is regarded as a deviant or incomplete expression of human capacities. And such writings are reinforced with all the weight of prestigeful names ranging from Plato to Suzanne Langer. In short, though ethnologists have already accomplished much in the understanding of primitive art, they still have before them an important problem concerning aesthetics in primitive societies as well as the task of making their findings available beyond the anthropological fraternity.

The quality of this problem may be indicated in the following manner: we can discern that artists employ four major methods to produce emotion and evoke aesthetic responses—(1) employ symbols that have established emotional associations; (2) depict emotion-arousing events, persons, or supernatural entities; (3) enlist the spectator's vicarious participation in the artist's solution of his problems of design and technical execution; (4) employ particular combinations of line, mass, color, etc. which seem capable of arousing emotions in themselves. Usually, these procedures are employed in some combination. The first two require knowledge of the beliefs, value system, etc. if a cross-cultural understanding of graphic and plastic art forms is to be achieved. The third requires knowledge of the technology and its limitations, characteristics of the materials used, and the like, for the spectator to participate vicariously. Anthropologists, as a matter of course, deal with the arts of primitive societies with full and conscious awareness of the first three points above, and most of their writing about primitive art is cast in those terms. Non-anthropologists dealing with primitive art (and they are legion) however, approach and evaluate primitive art with some measure of ignorance concerning the first three procedures but instead judge and select examples of primitive art on the basis of the fourth—the formal aspect—and make their evaluations according to what emotions are aroused or communicated by line, mass, color, and so forth. (Parenthetically, I am sure that anthropologists do this too, not only with art forms from their own society, but also in selecting examples from others, perhaps some primitive society with which they are doing field work and are also making a personal or museum collection.)

That this should happen, that ethnologists sometimes and others frequently treat primitive art mainly or entirely as pure abstraction and with regard only to its organization of lines, masses, color, or form, the meanwhile ignorant of all or most of its symbolism, and of the techniques involved, suggests strongly to me (as it has to others) that there are indeed formal elements in the graphic and plastic arts which in themselves are capable of arousing emotions and evoking aesthetic responses. But about this matter we know very little beyond the borders of our own society, and what we know within Euro-American society is so ethnocentrically biased that it probably is not applicable elsewhere to any substantial degree.

If it is ever to be shown that particular formal elements or combinations do indeed arouse emotions and aesthetic responses by themselves, and that these are universal, it will only be done through collecting the primitive artist's statements about his fellows' work, through understudying native craftsmen, and through the pursuit of controlled, cross-cultural experiments where objects from one society are presented to members of another for their aesthetic judgments. The present ethnological literature contains a bit of such information (writings by Bunzel, O'Neale, Himmelheber or Fagg are an example), but we need far more. I chose to bring this topic up at the Congress in the hope that this audience, and the readers of the *Proceedings*, will in their future field work give attention to the problem.

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ACCULTURATION

Sol Tax

When Columbus discovered America there were a great variety of cultures very different from the European by which the people of these continents lived. If we call to mind the way of life of The American Indian that Wissler, for example, describes, it would appear nonsense to ask whether acculturation has occurred. Do Incas still hold that tremendous empire together? Do the Mexicans cut out the hearts of prisoners atop the pyramids? Do the Sioux still count coup? Which tribes are chipping arrowheads now? To answer these questions is to say that of course Indian culture has at least lost a great deal of its roster of aboriginal traits. And if we ask if some Indians are Christians, or if they eat wheat bread or ride on horses, or if some speak Spanish or English, the answers tell us that in some degree the descendants of the aborigines have also adopted a great deal of European culture.

In any ordinary meaning of the term acculturation the American Indians have undergone a great deal of it. At a recent meeting of ethnologists who study in Mexico and Central America there was considerable discussion of this point, and a sharp difference of opinion arose. Dr. Paul Kirchhoff (whose specialty is the early culture) at one point asserted that some 95 per cent of the aboriginal culture was lost. To those of us engaged in studying the culture of the present-day village Indians, this seemed an outrageously high figure. Even the most acculturated groups in Guatemala, Chiapas, Yucatan, and Oaxaca seemed to us much more Indian than European. They speak Indian languages, have a system of beliefs, values, and motor habits different from those of the Ladinos, and every aspect of their culture—from technology to religion—is a liberal mixture of Indian and European elements, and of course many novelties that arose after the contact between the two. Elsie Clews Parsons' study of the mixture in Mitla of course illustrates how much of the Indian there still is even in that relatively acculturated town.

The discrepancy in point of view may be explained because different questions are involved: First of all there is the difference between asking how much of the aboriginal culture of the 16th century is to be found among the Indians today, and asking what part of present-day village culture is Indian. Second, there is the difference in the weight given to the great cultural works of a people, like the astronomical system of the Mayas, as compared with the ordinary culture of all the people. And third, is the difference between counting culture traits, or outward manifestations of culture, however they may be weighted, as contrasted with over-riding patterns, or themes, or the basic ethos. If one therefore thinks first of the ancient Maya, and their theocracy and temple cities and astronomy and systems of mathematics and notation and the like, of course the Indians in the villages today seem quite de-culturated and Kirchhoff is close to correct. It is quite different, however, to focus on the village today, and the ways of thinking and behaving of its people. There is still another difference in point of view that I must mention. One recalls the man who said that he has used the same razor for forty years; during that period he had replaced the blade five times and the handle twice—but it was still the same razor. In similar fashion, one may or may not choose to admit functional substitutions to the acculturation picture. For example, many of us see that, function for function, the Catholic saints are often simply substituted for the earlier gods. Perhaps there is only in fact a change of name, and the change ought not to be weighted heavily. Or the fact that Indians often substitute in their ceremonies the more recent distilled liquor for their old corn or fruit ferments does not seem too important. Yet by admitting such substitutions one can easily reduce the whole matter to utter absurdity. Rifles instead of bows and arrows, or steel machetes instead of stone axes, but the culture hasn't changed. Do we also want to substitute for some hunting complex the institution of the butcher shop where cattle are slaughtered? Or for that matter, one can argue that the whole social system is still pre-Columbian, with the mere substitution of a Spanish ruling class and Roman Catholic hierarchy for the old upper class and theocracy.

In answering the question of how much acculturation has gone on, we have to recognize these choices. There is still another choice that is very relevant. There is a difference between asking how much acculturation has gone on over some long historic period, and asking how much is going on at any point in time. Oliver La Farge's study of cultural changes in northwestern Guatemala which shows that short periods of rapid change alternate with longer periods of quiet consolidation and reintegration is applicable to very much wider areas. In Middle America as a whole, most of the loss of aboriginal traits appears to have occurred during the first years after the Conquest. Acculturation isn't a matter of either steady or of homogeneous erosion. It is obvious that I did not put the question of whether acculturation occurs with any intention of giving an answer.

To this point I have used Middle America to illustrate the difficulties we face in discussing problems of acculturation. The remainder of this article is concerned mainly with the Indians of North America. The same choices need to be made in this area; but in some ways the problem is simpler in North America than in Middle America. For one thing, the typical unit is the discrete tribe rather than the large complex society with its own sub-cultures. For another thing, history is shorter in most of North America: there was the first shock of pacification and loss of land, economic means, and freedom; and then came the reservation period.

This paper confines itself to the reservation period. The first major loss of aboriginal culture has already occurred; the buffalo are gone, it is not legal to collect scalps, children go to school. The white man is all around, buying furs or beadwork or oil, or hiring Indians to work in field or factory, or simply supplying money and services. The Indians have made adjustments to new conditions, and every accommodation they have made represents a change of some sort in their culture. In other words, a great deal of acculturation has occurred. The question that is being asked for North America is whether acculturation generally continues to occur.

I shall make a very bold statement to begin with, and then qualify it. My answer is that acculturation does *not* occur. Most of us assume that these small enclaves of American Indians must soon disappear. Perhaps there is a bias peculiar to United States sociology and anthropology that is connected with the American ideal of the melting pot. Immigrants from Italy, Poland, or Ireland came with their peculiar cultures, and in a generation or two were absorbed into the larger cultural stream. We tend to assume that the same must happen to the Navaho or the Fox or Iroquois. It is only a matter of time. The Indians have perhaps been a little slow to get into the game—that's because we've kept them on reservations. As soon as we turn them loose, like other citizens, they'll be quickly absorbed.

My thesis stated in its strongest terms is that there is no reason to expect now that the Navaho, the Fox, or the Iroquois won't be with us for a thousand years—or, as the treaties used to say, as long as grass grows and water runs.

Now to some qualifications: I am thinking primarily of the subtler or inner aspects of culture, as opposed to the more superficial characters. This distinction, long labored by anthropologists like Benedict, Linton, Mead, Kluckhohn, Opler and many others, is still not clear. Nor are there stated operations by which to recognize the differences. Nevertheless, we all agree that outward forms may change while inner meanings may remain the same and vice versa. And, I repeat, my hypothesis that acculturation does not occur is confined largely to the area of meanings.

I must qualify my proposition in still another way. This involves a couple of definitions. Take as a model a society which has a culture shared more or less by all of the individuals in it. Imagine Zuni, if you wish, or the Pine Ridge Sioux, or what have you. Imagine now that all of the Zuni stay right where they are, but their culture changes so that the norms of behavior are like those of the surrounding white society. I would call that acculturation, and say that the Zuni have acculturated. If they maintain a social cohesion—in the small-town American pattern, having lost all Indian forms of social organization—and remain socially apart from the larger community, then they are completely acculturated but not at all assimilated. This of course is an unrealistic model, since acculturation could not occur without very significant social relations, hence considerable assimilation. But I set it up to define community acculturation.

Now look again at our ideal type of unacculturated Indian community. Suppose the population is one thousand. Now imagine that one by one or in small groups, individual Indians leave the community. Possibly they are forced to leave by economic circumstances, or whatever accident. As individuals suppose that they then change their norms of behavior completely from those of the Indian community to those of the white world into which they have moved. Perhaps we don't wish to call that acculturation; one of my students thinks the word should be confined to group situations. In this case, at any rate, the individual and his children surely become assimilated into the larger society.

If a hundred Indians leave the community in this manner, the remaining nine hundred are still an unacculturated community. If the whole thousand leave the Indian community, to be assimilated into the larger society, the Indian culture is of course dead.

There are thus two ways for a culture to become absorbed into another—by acculturation (meaning community acculturation) and by loss of individuals. Both processes always occur together, and are obviously related. (I have seen both in operation in Guatemala, where the situation is otherwise quite different from North America.)

My hypothesis is (1) that acculturation is not occurring in North America; (2) that Indian societies lose individuals, but the rate is so slow compared to the vegetative population increase that (3) there are as many or more Indians in communities with Indian culture than there were a generation ago. And for all we know, the number may increase rather than decrease.

Such a proposition obviously has very significant implications for government policy; these I shall not discuss. It is important, however, to establish the proposition, or discover the degree to which it fits the facts. The work that we have done among the Fox of Iowa, and at the Fort Berthold reservation in North Dakota, suggests very strongly that there is much in the proposition. Of course what we know about the Southwest supports it. But reports I have read about the Penobscot Indians of Maine, and the Iroquois of New York and Canada—which have had so long a history of contact—also seem to support it.

It is also necessary to fill in the hypothesis by describing the mechanisms by which American Indian cultures resist acculturation to our ways.

I would like now, however, to try to make sense out of the hypothesis in more general historical terms.

Let us return to our theory of the melting pot. Sociologists use the term acculturation in speaking of Italians, Poles, Irish and others; indeed, these days we talk of the middle and lower classes as if they were different cultures. It is a matter of degree. But I think we ought to emphasize what is so obvious that it is being forgotten: Western civilization is one culture, and the differences that subdivide us are utterly inessential compared to those between, say, Europe and the Far East, or between the aboriginal Americans and Africans or Australians. In the tree of culture even the Near and Middle East are so close to Europe that we can expect fewer problems of communication than with the American Indian. A globe of the earth and history will show in fact that western European culture and that of the American Indian are more separate than any others except perhaps those of Australia and Tasmania.

This is true for all American Indians in contrast with Europeans. Why should we expect easy transculturation?

But the difference is even greater between Europeans and North American Indians than between Europeans and the Indians of Mesoamerica and much of South America. In the latter places the Indians developed large societies and social classes and trade and the like, and hence there developed many parallels with Europe. But the North American Indians remained tribal.

This leads me to what I think is the most important difference. The fact is that the North American Indians are among the few people on earth who never became in any sense peasants.

I need not remind this group that some 7500 years ago there began to occur what Childe calls the Neolithic Revolution, and I think that most of us realize its tremendous effects on human social life and thought. The aspect of the change that I am emphasizing now can be dramatized as follows: you all recall the Book of Genesis, and the story of Adam and Eve in the Garden of Eden. Paradise was of course the food-gathering stage of human culture. Whether the Biblical story represents a nostalgic folk memory—the change hadn't been so long before—or not, the fact is that Adam's punishment was that he would have to earn his bread by the sweat of his brow. Indeed, Adam and other full agriculturalists did sweat for their bread. Peasant life is a kind of slavery to property and to time; this is particularly true in the full agricultural economy that developed in Asia and Europe, where domestic animals were tied into tillage. In parts of America, Oceania, and Africa both plants and animals were domesticated; but generally only in Asia and Europe did a system develop where the two became thoroughly intertwined. Maize in Mexico requires seasonal care; in irrigated field crops there is special urgency. But in the full neolithic of barns and hay and pulleys and plows with oxen, and dairy cows and all the rest—here indeed man becomes a slave to daily chores, to time. Make hay while the sun shines is a realistic adage, and leads directly enough to compulsions about time and about protecting and saving for a rainy day and for posterity. Thousands of years of development of these institutions and ideas have left a stamp on European man; our habits of work-and-save have made of them a virtue and one basis of a whole ethical system.

In all of this the North American Indian has not shared; indeed, it was with considerable shock that Europeans discovered noble red men whom they rightly saw to be free in some sense they were not, and who were rightly perceived to set a dangerous example.

I must emphasize the point that the difference was a difference in the tradition of peasantry. The agricultural Indians with whom I have lived in Guatemala are nearly as much peasants as are Europeans, and in large part share the slavery to time and property, and the ethical values which accompany it. The contrast with North American Indians is emphasized by this comparison; here there was only the beginning of agriculture, and except for the pueblos in the Southwest no great dependence on it. And nowhere is saving more important than sharing; and where there is preoccupation with property it is rather with destruction of it than with its private accumulation.

The great changes which began in the commercial and industrial revolutions of the past centuries may eventually end the slavery of the peasant. Urban life is much more independent of property and the compulsions of time and place. The mobility of the urbanite is somewhat comparable to that of the hunter. It may well be that Indians will communicate much more easily with Greenwich Village than with Main Street.

Recall again the American theory of the melting pot. Sociologists now see that even Europeans haven't melted together as it was once supposed that they would. But the melting-pot theory itself was only partial, and took little account of genuine cultural differences. The fact that the Chinese, for example, showed few signs of melting was probably put down to the color bar, when indeed the cultural difference could well have been more important. Surely the color factor in Negro-white relations has not kept Negroes from a thoroughgoing acculturation to European ways and values. But the North American Indians are surely different enough culturally to explain any lack of acculturation one finds—connected as they are to Europeans only by the most remote fork in the historic tree of culture; and divided from them by all the differences between a large society and a small tribe, and by the habits of thought and principles of peasantry and commerce as contrasted with the hunt.

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WITHDRAWAL, AN EARLY MEANS OF DEALING WITH THE SUPERNATURAL

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This paper is a humble footnote to the literature on religious origins. In teaching primitive religion I found, as I suppose many here have, that we are now ready for a much more detailed classification than has yet been made. The separating of elements which have been roughly classed together will clear up some of the confusion and contradiction found among various writers. It may also help to work out a time sequence for attitudes toward the supernatural, at least in one culture.

We often begin our speculations about such attitudes with the fact that Neanderthal Man, in common with some historic groups, painted the bones of his dead with red ochre (Linton, pp. 137). This has been taken, very reasonably, to indicate some sort of belief in a soul and an afterlife, no matter how crude and vague. Moreover, the use of red, which so often stands for blood and life, points to some conception of sympathetic magic. All we know of primitive behavior corroborates such a theory. I would suggest, however, that, compared with any form of animal reaction known, the positing of a soul seems a fairly advanced form of reasoning. Perhaps the proposition of sympathetic magic that like produces like demands less ratiocination, but neither can have been man's very first reaction to powerful and mysterious events. There must have been a long development of attitudes toward the supernatural before this stage was reached.

Even further development is indicated by such primitive beliefs as ancestor worship, animism, animatism, and even mana, which has a good many rules and functions. The point about these theories which astonishes anyone who has had long association with primitive groups is that they are all cognitive. They begin with the assumption that mankind was questioning the *cause* of phenomena which he did not understand. Doubtless he did, ultimately, or some intellectually inclined individuals did. On this point, I agree with Radin (ch. 4) that in primitive society, as in all others, there is an obvious difference between the thinkers and the unquestioning masses.

The reaction of these masses is purely an emotional one whose expression can be channelled by the thinkers into particular grooves. If we ask what it may have been in very early man, the answer we get from many sources is fear. To quote two modern statements, Hutton says, "Primitive man, 'that frail phantom and waif in an unfriendly world,' lives beset by fears of every kind. They . . . prompt him to avoidances almost as instinctive as those of lower animals" (Hutton, p. 8). Karsten puts it, "The savage fears everything strange and mysterious . . . Forest, jungle, cave, mountain, thunder, lightning. . . . All these things awaken in him a sense of something supernatural and divine. . . . It is this sentiment that is the primary trait of religion." He soon goes on to give these phenomena a concrete form (Karsten, p. 27).

My thesis is along this line. However, I would not go so fast as Karsten in assuming that the giving of a concrete form, that is, imagining a spirit of forest or lightning, was an immediate result. To imagine a spirit and to give it a definite appearance means a thinking brain. To jump from instinctive fear to that sort of explanation seems to me almost as great as that from a hand axe to a flaked point. As the hand axe served a large portion of mankind as its chief and almost only tool for thousands of years, so I think it possible that, for much of that same time, man was reacting to what I shall briefly call the supernatural simply with fear, unmixed with any speculation or even the most rudimentary magical devices.

Since even Neanderthal Man may have achieved both speculation and magic, this assumption leads us back into prehuman history. We now recognize that, even during the early Pleistocene, there may have been various forms such as Coon calls "half-brained men" (Coon, pp. 28–32), able to perceive the awesome more clearly than Darwin's dog (Rivers, pp. 10 ff.). Yet, surely, they would react to it in animal fashion. This fashion, when the very weak are confronted with the very powerful, is not combat, nor even flight. Rather, it is utter stillness, which may allow the frightened creature to be overlooked. This is the device of the rabbit pursued by the wolf or the bird by the hunter. It is akin to the system of camouflage in nature which makes the victim invisible. We can imagine that mankind, with a long history as the weak creature among more powerful beasts, had developed such an instinct, or at least such a habit, far back in his history.

These half-brained forms had speech. They would have been able to convey ideas of mysterious danger and of ways to avoid it, long before they speculated as to its cause. I suggest that at some half-human stage the instinct to remain passive in the face of the supernatural, to "play dead" as it were, may have developed a set of rules which have spread to various situations. Such rules include seclusion, not looking at the sun or at fire, not touching the head or sometimes, the lips, those important parts of the body. Often there should be little or no talking, little sleep, little food. Perhaps the person in danger may have to sit in one position.

Numbers of reasons are given for such rules as that looking at the sun will cause blindness, scratching the head will make the hair fall out, talking will make one a chatterbox. There is no integrating theology. In fact the reasons, which vary greatly from place to place, might even be later accretions. The sum of all the rules is simply a complete passivity, a reduction of vital processes, almost a simulation of death. Frazer and, after him, Durkheim have classed such behavior as negative magic. I would question whether it can even be called magic, since magic, in Frazer's own definition, is a means of manipulating the supernatural, and this kind of shrinking can hardly be called manipulation. May we not here be on the borderline between magic and mere animal reaction?

My next point is the question what situations produce such a very elementary response? Tylor (pp. 428–30) suggested death as providing man's first introduction to mysterious power. I agree that it must have been one of the first but, if so, the action of withdrawal must have been associated with it long before there was any theology concerning the soul and the afterlife. To this subject I shall return, for all death observances which we know of now do include this kind of theology.

That statement does not apply to birth. In numbers of cases which we know of (Frazer, pp. 207–10), forms of withdrawal are practiced without any reference to a belief in souls, such as we find in the case of the dead. I have often wondered, in fact, whether birth might not have been more impressive to the primitive man or primitive post-ape than death. The abundance of female statuettes, like the so-called Venus of Willendorf, give proof of an early preoccupation with maternity. We have got past the stage of thinking that these indicate matriarchy or the worship of a female goddess. My own opinion is that they are charms to aid fertility and help with a safe birth. Leaving that controversial point aside, I would suggest that the recurring fact of birth, with its dangers and uncertainties, was a pivotal point in the life of early man and a natural place for the first reactions toward the unknowable to crystallize.

Primitive cultures provide taboos for the parturient at least as often as for the mourner. These are of two kinds. The other sex must avoid her as a vehicle of supernatural power. Also she herself must often refrain from activity, from touching her head or looking at the sun. Often the newborn child must be kept out of sunlight until a ceremony has been performed, removing taboos from it and the mother. The couvade which included the father in the withdrawal practices is not so common. I suppose most of us disagree with the Freudian proposal that the father's inactivity indicates male jealousy of female creative power. Except in extreme cases, the prescription for the father is refraining from his daily work. He must not hunt, fish, or fight because he would have bad luck. True, a great many practices referable to sympathetic magic have accumulated around the basic fact of withdrawal. These, as I have suggested before, mean a certain amount of reasoning. They should be classified as a separate phase of birth practices and perhaps a later one.

The treatment of the menstruant, and especially of the first menstruant, is, of course, to be classed with that of the parturient. Some later religions, like the Hebrew, have regarded the female in this condition as unclean. However Frazer (p. 223), and others after him, have proved clearly enough that what was later regarded as unclean was originally too sacred and therefore too dangerous to be touched. So the girls in many cultures were secluded and subjected to the same taboos as the pregnant woman. These strictures were relaxed oftener than were those for the parturient, perhaps because the danger did not seem so imminent. In time and in various cultures, the procedure for girls became encrusted with ceremony which obscured the stark fact of withdrawal. Thus the elaborate dances given for girls on their return from seclusion (Driver, p. 28) are connected with the very practical matter of marriage arrangements. The African ceremonies, where a physical operation is performed, seem an attempt to affiliate the girl's treatment with boys' circumcision, which had a quite different origin and purpose. The Apache ceremony, at which the girl's touch confers a blessing (Opler, pp. 90 ff.), looks at first like an anomaly. Perhaps, however, it comes nearer the feeling that the girl is in a sacred condition.

Withdrawal practices must have proliferated gradually to include many different situations with proscriptions against using or looking at many persons or things, from a neighbor's field to a king or a mother-in-law. At this stage, the various kinds of withdrawal have been given the technical name taboo. Sometimes taboos are on the same non-theological basis as the seclusion of the menstruant. For instance, if a Navaho fails to avoid his mother-in-law, both will go blind and jump in the fire. No spirit is concerned. No reason is given except the fact that this is so.

However, the many Polynesian taboos and also many among American Indians form part of a consistent theology. They are integrated with the idea of mana, the all-pervading supernatural force which may be focused in any object, organic or inorganic. This concept has appealed to many students as a basic one beyond which we cannot go. Yet mana has its theology. It is sometimes said to come from a powerful spirit. It can be obtained from heredity or by effort. It can be lost. It is not the uncontrollable phenomenon seen in birth and death. It would be possible to imagine the mana and the taboo concepts as slow developments after long periods of withdrawal.

Even when further theologies were developed, including a belief in souls, ghosts and spirits, withdrawal continued to be an accepted response. In the treatment of the dead, already noticed, we find there are extreme cases where mourners are subjected to all the taboos enjoined at pregnancy and childbirth, with a further one not necessary for the secluded woman. This is sexual continence, enjoined on all mourners and corpse handlers. Moreover, in this case, we find an incipient theology. Instead of a mere statement that some bodily ill will befall those who neglect the taboos, there is usually some myth about the spirit and the afterworld.

One can well regard them as part of an incipient theology. But why do the mourners also practice acts of withdrawal? Why should it please spirits to have men abjure the food which keeps them alive or the sex intercourse which keeps their race alive? The statement that this means purity seems a civilized one, for primitive people do not think such acts impure. Nor can we say that such abstinence makes the devotee more like a spirit which neither eats nor marries. Such a conception of spirits took a long time to evolve, and even some present-day primitives do not have it. Is it not possible that here we have two layers of religious practice? The earlier one is the instinctive animal reaction of withdrawal, which began long before Neanderthal man painted bones with ochre. The later indicates some rudimentary theological concepts which would later people the world with different classes of supernatural beings.

In the class of events subject to spirit influence, we may place not only natural death but murder, the killing of an enemy and the killing of fish or game. Frazer has already grouped these together, and later study of New World practices goes to confirm this statement. The element of danger here is that the soul of the dead man will haunt his slayer or the soul of the animal or fish will turn his kind against human beings. The explanation is reasonable and so are the ceremonies and offerings made. Again, there is withdrawal, in whole or in part, with sexual continence a prominent element. Frequently the reason given is that the deer or other animals do not like women. To explain this, we would have to fare into Freudian depths unless we consider that hunting is subject to chance, and therefore, to primitive thinking, to supernatural power. It should therefore be treated by the well-known ancient means of withdrawal.

These procedures involve not merely a theology dealing with ghosts, souls, and other worlds. Often they use what Durkheim calls positive magic, such as hunters' amulets (Speck, pp. 227–230, pl. 36), or a scalp which has been purified and become a rain charm (Bunzell, pp. 674–689). Here I would like to bring up the matter of classification, mentioned earlier. Positive magic, which looks toward handling and controlling the supernatural, is purely a more sophisticated tool than negative magic which may come to no more than hiding. We can well imagine it to have been a later development, leading to elaborate theories of correspondence between objects or between present events and future ones.

The forms of magic which I have mentioned so far are more or less democratic, practiced by anyone as a matter of tradition. They may all have existed before the days of specialists. To go further, we should have to follow fine distinctions among specialists who are taught, inspired or hereditary, with all possible permutations. Although all of these wield an armory of devices for dealing with the supernatural, they make some elements of withdrawal a basic practice. It is my suggestion that, having been first used as an almost animal reaction to danger, they have continued into far different situations and have been glossed with theological explanations, having no connection with their origin.

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SUBSYSTEM TYPOLOGY IN LINGUISTICS

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In order to identify the kind of typology which is restricted to "subsystems", I list below, and discuss, three groups of papers concerned with (1) classifications of languages; (2) identification of subsystems within whole systems; (3) domains—subsystems within the dictionary—as related to the problem of selection.

CLASSIFICATIONS OF LANGUAGES

Edward Sapir's *Language* had as its primary objective the revival of whole language typology which fell into bad repute because its 19th century propounders (a) ranked their types according to an asserted evolutionary development; and (b) weighted their types according to a scale of ethnocentric values. Stated purely in linguistic terms by Sapir, the whole language typology became accepted as legitimate but not exciting, respectable but not revived. It failed to arouse the research interests of any group of workers.

In fact, the only fresh attempt to develop further Sapir's typology is found in Greenberg's recent paper, *A Quantitative Approach to the Morphological Typology of Language*, which obtains a derivational index, an inflectional index, and the like. This quantification is all to the good, but the theoretical framework for the various morphological indexes is a bit wobbly. It is not its concern with "the morphological structure of the word" which may or may not include a clue "of fundamental importance to the over-all characterization of a language" that makes a whole language typology what it is. One might just as well be concerned with phonological structures and seek clues therein for the same purpose.

The argument of my paper, *On Developing New Typologies, and Reviving Old Ones*, is that Sapir succeeded all too well, all too fast; Greenberg's quantified revision of Sapir's whole language typology can be predicted to obtain a similar speedy success. Whether few, or quite a few, criteria, are used, whether based on counting instances in a text or based on the known inventory of the morphology, it is certainly possible to classify languages irrespective of their genetic relationships. In fact, a single worker could be given the task of classifying all the languages of the world as whole systems; and having accomplished this, he would be hard pressed to find a next related task.

IDENTIFICATION OF SUBSYSTEMS WITHIN WHOLE SYSTEMS

In a recent issue of the *American Anthropologist*, most papers were concerned with cultural typologies of one sort or another. Though references to the matrix of the special typologies—to the whole culture—were commonly made, no one except Evon Vogt succeeded in identifying, with sureness, the whole culture or whole system to which he referred; this I have indicated in some detail in *Subsystems Within Systems in Cultural and Linguistic Typologies*. Perhaps the reason why whole-system typology preceded subsystem typology in linguistics is because any one whole language is distinguishable from any other as a result of the language barrier, the place where communication or mutual intelligibility fails; analogous boundaries within a language, as between phonemes and morphemes, are obvious neither to the speakers of a language nor to non-linguists, but only to the analysts of the language.

Subsystem typology often treats the phonemes alone, and not the morphologies of the languages concerned. So, for example, in the *Preliminaries to Speech Analysis* by Roman Jakobson and others; since no direct attempt to classify languages is made here, one is at first apt not to see that this encompasses a genuine instance of subsystem typology, as John Yegerlehner and I have shown it to be. It is clear from this example that while all whole-language typologies are classificatory, some subsystem typologies are not.

In his *Manual of Phonology*, Hockett seeks to by-pass arbitrariness, which haunts all work in typology—as opposed to historically oriented research—by including in any one of his subsystems only what is strictly symmetrical. The result is that he obtains much less than all the phonology in his numerous subsystems, less, even, than all vocalic phonemes and less than all consonants—for consonants, in fact, only that selection which makes for symmetrical contrast, with the remaining consonants set aside as left-overs.

I have devised a procedure somewhat similar to Hockett's, but one in which matched *linear phonemes* are abstracted from *additive components* in order to arrive at reasonably few and reasonably non-arbitrary—but highly comparable—sets of consonant types and vowel types. One interest in this and other subsystem typologies is that we arrive—little by little, but nevertheless surely—at linguistic perimeters: in respect to the subtypes treated; also in respect to a linguistic area or a continental area; and finally in respect to all languages of the world. It can already be shown that, so far as consonant and vowel types go, the perimeters are relatively narrow in South America, relatively far apart in North America.

DOMAINS—SUBSYSTEMS WITHIN THE DICTIONARY—AS RELATED TO THE PROBLEM OF SELECTION

Two recent papers, one by Lounsbury and the other by Goodenough, extrapolate operations commonly used in linguistic analysis to the analysis of one lexical domain: kinship terms. The rest of the literature, from Morgan and Rivers to the modern British social anthropologists, pays less attention to the terms themselves, but characteristically treats the kinship system as a closed corpus—one of several corpora, to be sure, but the one most clearly isolable of all the domains concerned with interpersonal relations.

Some attempts have been made to isolate and treat separately other domains concerned with other fields than social organization—perhaps most successfully in ways of counting or quantifying, as the use of single morphemes from one to five, or one to ten or one to twenty, with morpheme combinations for other numbers in the given subsystem. Examples of less spectacular success are the papers which appeared in a long run of the journal, *Wörter und Sachen*. And general discussions on meaning as derived from domains are also common; perhaps the best of all is by Hallig and Wartburg. The difficulty is always that one set of words for one domain—one word family—intersects with another; hence the domains set up are never strictly isolable, one from the other.

Or almost never: the one great exception is the kinship system which is at least treated as though it were isolable from other domains. If kinship terms were assembled from a bilingual gloss, they might appear no more isolable than other domains. But they are, in fact, not so assembled; rather they are brought together through highly specialized eliciting, first practiced systematically by Rivers, and often called the genealogical method. In order to make other domains equally isolable, specialized eliciting will have to be devised. Any one of the three eliciting methods currently used by linguists will obtain data for the analysis of grammatical structure of any language, with varying degrees of reliability and speed. A fourth method of eliciting, listed below, is incidentally relevant to structural analysis, but primarily useful for discovering domains.

(1) The Bloomfieldian method of eliciting is restricted to texts. Leonard Bloomfield's publications reflect his practice: Tagalog Texts, with notes on the structure of Tagalog obtained from the texts; texts from Algonquian languages, with the linguistic structure from these languages still in manuscript. Bloomfield spent much time learning to speak Menomini because he would not permit himself to ask questions on structure; he preferred being corrected, when he made an error as a child-like speaker of Menomini, to asking a direct question on how do you say so-and-so to a bilingual Menomini—for fear of obtaining a false analogy. During the three summers Bloomfield and I recorded Ojibwa texts together at Linguistic Institutes, he never once asked a "how do you say" question, but once his curiosity overcame his scruples, and he asked me to elicit directly from the informant.

(2) A method which enables one to obtain information on linguistic structure without benefit of extensive prior texts is here called Whorfian eliciting because Whorf seems to have been the first scholar in the tradition of anthropological linguistics to elicit in this way—when he had occasional hours of work with a Hopi informant in New York. The small manuscript dictionary left behind by Whorf reflects his eliciting: the informant must have given the range of meanings of Hopi words, with Whorf subsequently restating the meanings in common denominator terms.

(3) Boasian eliciting, still followed by most American anthropologists today, proceeds by obtaining texts first, and by obtaining morphological variety or ancillary forms while translating the texts. Almost everyone becomes aware that it takes much longer to elicit the ancillary forms than to record the texts—that he already has, in the early part of a field session, vastly more texts than he can possibly translate in the rest of his field session, if he is to elicit grammatical information as he goes along. At the end of his field session, the bulk of elicited material is apt to loom larger in size and interest than the bulk of his texts; and this is reflected in the fact that grammars are traditionally published first, before texts.

(4) The text-domain method of eliciting begins, as does Boasian eliciting (3), with texts, but it ends with texts bulking larger than the elicited material. This eliciting maximizes the use of texts in contrast to Whorfian eliciting (2), which minimizes the value of texts. The most favorable conditions for practicing text-domain eliciting are two: the use of several different informants, and, as in Bloomfieldian eliciting (1), the exercise of some partial speaking-knowledge of the language, even though all informants used are bilingual. In translating

texts, then, one asks for ancillary utterances related primarily to the domain which the text treats, rather than to the grammatical structure. Some examples from field work with Hopi will illustrate this text-domain eliciting.

If a text included the name of a tree, the questions asked were the names of the root of the tree, its bark, and its leaves. It turns out that Hopi has two very similar morphemes, one a generic term for *root*, another for *medicine*, and a third but entirely dissimilar morpheme for *dry root* of the cottonwood tree from which Kachina dolls are carved. Likewise, Hopi has one generic term for *bark* of tree, *peeling* of apple or onion, *pod* of pea, but a dissimilar morpheme for *bark* of juniper trees. A single morpheme for color means *green* when compounded with *leaf*, but *blue* when compounded with *corn*.

Materials obtained from such eliciting make it possible to typologize: one type would include dissimilar morphemes which have overlap in meanings, as in the examples above and in the two entirely different Hopi morphemes for *water*, where again one morpheme is the generic term while the other is restricted to drinkable liquids; for example, "breast water" equals *milk*, and "urine water" equals *beer*.

Another type would include single morphemes which would be classified in two different domains because there is a wide gap or discontinuous referent—as the Hopi morpheme whose two referents are: 1, *sun* in the heavens, and 2, *clock* on the wall. In a given text, one or another of two discontinuous referents is possible in translation; this in contrast to the continuous referent range, where any referent in the continuum is possible for translation in a particular text. Thus, there is a single Hopi morpheme with a continuous range from *thinking* to *worrying*.

Another type, for example, can be established when the privileges and restrictions on morphological selection or combination are known. Thus, in the domain of the corn complex, some morphemes for corn become edible under the name of "piki" which has a wide continuous range of meaning or application to cooked foods of various textures and shapes; other morphemes for corn become edible under the name of "kʷivi" which has an even wider application to cooked foods. But the type of food classified as "piki" always includes one or another kind of corn as a constituent, while food classified as "kʷivi" may or may not include corn. Here again a type distinction is found for morphemes which remain in one domain, namely the corn complex, and those which intersect two or more domains.

By any kind of eliciting one can find obligatory categories. However, when a choice is possible between two or more channels of expression, the current methods of eliciting serve at most to find the alternative possibilities, without distinguishing between situations in which either channel may be followed from those in which one or the other are followed—though either channel might be intelligible for any situation. This is the problem of selection. Text-domain eliciting enables one to work directly on this important but elusive problem. Thus, there is in Hopi an overlap in referents for two dissimilar terms, one of which is "qaci" *life*, which hardly ever appears in conversational texts. Upon finding this rare term in one text, I proposed various situations in which I supposed it would be relevant to talk about *life*, and found that the morpheme which I had previously translated as *heart*, without appreciating its wide continuous range, was also the common morpheme for speaking of *life*—in biological rather than in religous situations; thus, the Hopi say that a cat has nine hearts.

Text-domain eliciting not only leads us into the area of selection—alternate channels of expression with overlap referents among which the Hopi speaker makes choices as he talks—but it helps a beginning learner to speak a sensible kind of Hopi, that is, to make choices between alternate possibilities of the same general type as those made by the Hopi speakers themselves.

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THE ETHNIC DIMENSION OF HUMAN HISTORY: PATTERN OR PATTERNS OF CULTURE?[1]

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It is one of the major tasks of the anthropologist to define the primary unit of his science—the ethnic unit. Sociologists base their science on the primacy of the biological family, psychologists on the concept of the individual, but the anthropologist generally, by common consent, has left the question largely unresolved. Tribe, nation, social group, community, culture—while unsatisfactory, have been applied provisionally for what we really mean, viz. the ethnic group.

John R. Swanton[2] counters the familiar question, "How many Indian tribes are there?" with another, "What makes a tribe a tribe?" He says:

> "The words 'tribe', 'band', and 'division', will be employed indiscriminately in what follows. One of the lessons resulting from any attempt to classify or 'give the number of' Indian tribes—is the fact—that there is no specific definition of such names that will apply in all cases. Sometimes a tribe is a tribe because of its political unity, sometimes because of its dialectic unity, sometimes from a mere 'consciousness of kind' on the part of the individuals composing it. A 'band' is supposedly a subdivision of a 'tribe' but, the definition of a tribe being such as it is, it is frequently impossible to say whether we have a tribe or a band. The word 'division' assumes of course, a larger unit but there are divisions which could be tribes from one point of view and divisions or bands from others. Still the *application of the name* to any group of Indians *whether by themselves or by outsiders means that they share something in common*, whether that something be a common *territory*, a common *language*, *culture*, *or* a common *government*. The common territory, language, culture or government *may*, *however*, *extend beyond the tribe*. A common territory may be shared by two or more tribes, as for instance in the case of the Hidatsa, Mandan, and Arikara Indians of North Dakota. A common language is shared by tribes bitterly hostile to each other, such as the Dakota and Assiniboin, and the Choctaw and Chickasaw. A common culture is shared by numbers of Indians in California who differ in language, and a common government is shared by the five tribes of the Iroquois Confederacy and the several tribes of the Creek Confederation."

Swanton then concludes that:

> "For all that, each tribal name means something and a knowledge of them, or at least a directory to them, with some intimation as to their geographical and linguistic position as basal ordinarily to their cultural position, is of distinct service to ethnologists and ethnographers." (Italics mine.)

This places the question squarely before the ethnologist: What are these Chukchee, Bushmen, Maya, Fijians, Samoans that we so cavalierly discuss?

The fact that they have been named by someone at some time can hardly be taken to constitute a sufficient basis for a scientific ethnic classification. What are group ethnic differences?

Even from the earliest human cultures we can trace, clearly distinguishable group differences in custom can be recognized. The stone implement maker of Europe and Africa in the Paleolithic period characteristically sought to produce a symmetrical pointed tool, while in southeastern Asia the earliest tool-producing peoples tended to make choppers with a linear edge, while in their pointed tools they seldom aimed at symmetry.[3] In North America eight to ten thousand years ago there were groups (Folsom) whose main technical aim was evidently to produce projectile points of chipped stone, while others (Cochise) produced grinders and millstones.[4] Shall we call these broad technological trends cultures, communities or what, and what constitutes our basis of classification?[5]

The vagueness of the ethnologist on the question of his primary unit is now reflecting itself in archaeology where an effort is being made to link up the data with functioning lifeways. V. G. Childe, quoted in Phillips and Willey's[6] recent summary of American archaeology, observed that in distinguishing *archeological culture* "the arbitrary peculiarities of implements, weapons, ornaments, houses, burial rites and ritual objects are assumed to be the concrete expressions of the *common social traditions that bind together a people.*" (Italics mine.) I. Rouse[7] compares the basic archeological units (components and phases or foci) to societies, communities or tribes.

In point of fact the ethnologist is equally arbitrary to Childe's archaeologist in the *peculiarities* he selects out of the whole gamut of social behavior to characterize the group he is studying. He often lays such stress on the peculiarities, at the expense of the "common human," that he ends up with a rather grotesque picture of the life of a people.

The analogy between archaeologist and ethnologist is even more pointed when we realize that the job of the ethnologist today is "social archaeology" since the life he seeks to study is that which was functioning before the spread of modern European commercial and colonial civilizations. Taking A.D. 1400 as a rough base line, the ethnologist in attempting this cultural reconstruction finds himself faced with a time depth varying from 50 to 500 years. From the present practices among ethnographic peoples, he attempts to separate out what elements are "due to white contact" and what are "aboriginal elements" and also at what time and in what order the "white" elements were added to the original cultural complex.

How do we delimit the units of aboriginal culture with which we work and, in Childe's terms, how do we derive the constants of social tradition "that bind together the life of a people"? More concretely, how do we delimit *spatially, the unit "people"* which we are assuming to be bound together by certain constants of social tradition, and what is the *time span* of such a unit?

Ethnologists have commonly employed a combination of principles as a basis for the spatial delimitation of a people to be studied. These classificatory principles can be broadly categorized into two classes:

I. Types of physiographic features combined with the technological and social arrangements for their exploitation (known as environmental or economic interpretations depending upon the emphasis of the classification).

II. Types of social, political and/or religious controls.

Actually, though this may seem contradictory, the second category offers us a more consistent basis for *spatial delimitation* than the physiographic features or their exploitation, as a people is bound together, whether for the exploitation of physical resources or for dealing with each other, by social controls. Their common interests can find effective expression only through such social controls.

For some time the concept "area" has had considerable currency—e.g., culture area, natural area, and, more recently, area study—growing out of combined operations in the war and post-war period.

Wendell C. Bennett in his *Area Archeology*[8] offering area studies as one way of uniting archaeology and ethnology as well as other social sciences, makes it clear that his concept "area" is not basically physiographic:

> "The physical and natural sciences contribute basic information about the region, but these sciences are not truly area concerned. Instead it is the geographer, historian, anthropologist, sociologist, political humanist who must pool their knowledge for sound regional analysis." (p. 8) . . . "Although natural area is an old geographic concept, the area approach stresses the suitability for human occupation." (pp. 8, 9.)

Bennett further makes the following rather significant comments about other classificatory concepts that he feels should be subsumed under his primary unit, the area:

> "Other subdivisions of the area which may or may not correspond to the natural areas, are based on multiple factors such as geography, economics, politics, ethnic composition [by which I think he means physical type], and culture. The importance for area analysis lies in the relative equality or inequality of these subdivisions, and in particular in their interrelationships. Attitudes of conflict between subdivisions are called regionalism, whether occasioned by history, by isolation, by ethnic and cultural differences, or by economic competition and are important in measuring the unity or disunity of the area. *Nationalism is in part the opposite of regionalism, insofar as it reflects harmony between subdivisions.*" (p. 9.)

It would appear from this observation that the political or social sanction of nationalism is more definitive as a principle of spatial delimitation of an ethnic unit than the physiographic area *per se*, or any of the other classificatory principles Bennett mentions.

On page 10 he states that

> "Area analysis is most applicable to archaeological regions of complex cultures or civilizations."

As an example of area archaeology, Bennett selects the Central Andes. However, we find that as Bennett carries his analysis back in time, his original area shrinks in terms of social and political rather than area considerations:

> "Today, Peru, Ecuador and Bolivia are grouped together as one of the South American culture areas. Similar unity existed during most of the Spanish Colonial period, and can be traced back to the time of the Inca political empire. However, for longer time analysis, the limitation of the Central Andes essentially to Peru is justified on the grounds that wider cultural unity to include Ecuador and Bolivia is not verified for the pre-Inca periods." (p. 11.)

And finally he observes that with the imposition of western civilization:

> "Commercial crops, mechanical transportation, and new power techniques profoundly affected the region as a whole. However, for the pre-Conquest periods, the changes which occur throughout the time period covered appear to be due to shifts in social, political, and religious organization rather than to major shifts in technology." (p. 12.)

However, despite the great importance of major shifts in technology following the Conquest, in this case there was no gradual infiltration of technological devices, but a military and social conquest upon which the technological shifts followed.

Thus while it must be conceded that natural features have, in one of Boas' old usages, a limiting effect—as a principle for the delimitation of an ethnic unit, the area concept even in Bennett's broad terms, is less effective as a basis for ethnic classification than the socio-religious-political type in a given locale.

However, while dominant social controls are valuable in delimiting ethnic units, we must remember that historically every group is a composite as the general trend of world history, even in its earliest stages, has been from smaller to larger social groupings, each of which has come about by an alliance of at least more than one family. The identity of the several groups that have been combined does not entirely disappear. Dominant social controls do achieve a certain community of practice and attitude in the group within which they operate, but they never achieve a total *gleichschaltung*. One of the most deliberate attempts in the past to unify a large body of people under a national authority was that of the classic Inca of Peru. Wendell C. Bennett[9] describes this process in the following manner:

> "The Central Andes became politically united under the dominant Inca of the central highlands. Although the subdivisions were still differentiated, the factors for national unity were emphasized. Each subarea was linked to the whole through a planned system of roads. The Quechua language was imposed and religious organization was of national proportion, and efforts were made to create cultural uniformity as well. Inter-regional conflicts were deliberately suppressed by force, and by planned shifting of the population."

The factors for cleavage, however, were not obliterated. They were simply transferred from interregional to class differences, between the local culture and the superimposed, the urban versus the rural.

Thus we are never justified in giving dominant social controls such primacy in our studies of human cultures that the existence and operation of genuine contradictions and major differences in custom within such socially controlled groups are undervalued or disregarded. The pattern of a culture must also include its dissident patterns.

I shall take as an ethnographic example of culture-historical reconstruction, through the study of local variations within a social group, Laura Thompson's work on the southern Lau islands, easternmost of the Fiji group. At the time the study was made in 1933–4, these islands had a minimum of outside population, e.g. no Indians or white settler and only a few Chinese trade stores, offering a relatively rare example in modern times of a native isolate.

Dr. Thompson's definition of Lauan culture[10] constitutes a brief resumé of the main culture-historical events:

> "... the simple culture of the aborigines, the highly organized culture of the immigrants from Viti Levu on the west and the influence from Tonga on the east met in the Lau islands. Conditioned by the natural setting of the island world, these diverse strains fused into a unique form. The combination of social organization, economic life, technology and outlook on life, found in Lau at the beginning of contact with western civilization, we call native Lauan culture. This native culture has been altered considerably in recent times by the influence of western civilization."

However, if we look into her analysis of Lauan culture in detail, the fusion has been far from complete on all levels. I shall here attempt to summarize from her work, the culture-historical events,[11] with apologies for certain liberties I may have taken in omitting some of her basic documentation.

The Lau islands are situated on the eastern rim of the continental submarine shelf of southeast Asia. Beyond them to the east are the oceanic Polynesian islands, with Tonga, the nearest group, 400 miles away.

The people of the first stage of Lau history had a

> "simple, indigenous Melanesian type of culture" (p. 196). They lived in small hamlets, had a religion based on magic and local totemic spirits propitiated by medicine men. Political power was in the hands of old men and all males were initiated into the men's society with rites carried on in a sacred rectangular enclosure, the rituals consisting of offerings to ancestral spirits, circumcision, ordeals, dancing, license, and the distribution of wealth.
>
> They were collectors of roots, fruit, and leaves in the jungle and probably also cultivated gardens on the fertile volcanic islands, but were prevented from gardening to any extent on the limestone islands because they lacked food plants which would grow well in shallow soil.
>
> By contrast with the people in the later culture stages, they were apparently a relatively peaceful group and did not make a practice of cannibalism.

How long these people inhabited the Lau islands is not known, and of course, neither is the specific version of the culture in this locality. But at the time of the report in the 1930's people with this type of culture were living in the Fijian islands[12] and traces were found throughout the Lau islands. The degree of its survival is rather remarkable in view of the following history, which includes at least 250 years of Polynesian-Tongan domination.

According to native tradition, about ten generations ago, which should be in the neighborhood of 1650, two large double war canoes, led by a war hero and his brothers, sailed eastward from the largest and westernmost of the Fijian islands, Viti Levu. They landed in several islands, one of which was Kambara in the Lau group whence they spread and established themselves as the dominant social group. They married women of the local group and their descendants became the upper or chief caste, while the rest of the inhabitants survive today as the "land" people, each group maintaining separate patrilineal sibs. Cross cousin marriages in a classificatory sense are the rule. Nevertheless the people think they can recognize the aristocratic breed by certain mannerisms.

Most villages today comprise some of both sibs, one or the other predominating in the proportion of 80–20 per cent; some villages are composed entirely of land people, but none are exclusively of the chief class. Most of the land including the fertile patches is owned by the land sibs, who take more interest in their gardens, while the immigrants are better sailors and expert spear fishers. Any land belonging to the chief class came to them as dowry when they married the local women.

The chiefs stimulated craftsmanship by attaching specialists to their courts, especially carpenters and fishermen, and extracted heavy tribute of trade articles from their subjects. Lau became known throughout Tonga and Fiji for her craftsmanship. On the debit side, bitter rivalries developed among chiefs of the different islands, violent wars resulted, fortifications that can still be seen were built high up on the cliffs, and according to missionaries and travelers, cannibalism, infanticide and strangling of widows at the death of a chief were prevalent customs. Petty chiefdoms developed as small islands became dependent upon the larger. From 1643 when Tasman discovered the islands, early voyagers reported these practices.

When the first historical records on Lau appear at the end of the 1700's, Tongan sailors had been visiting the group for two centuries seeking raw materials at three different locations in Fiji—fragrant sandalwood on the westernmost point of Vanua Levu, brilliant red paraquet feathers on another island for trade with the Samoans, and hardwood found only in southern Lau for their canoes and other fine wooden products. The Chinese traders discovered the source of the sandalwood in the early 1800's and soon exhausted it completely.

When the first European missionaries to Fiji landed at Lakemba in 1835 the Tongans had gained a great deal of political power. As Tongans in Lau were converted, they began to challenge the power of the high chief who complained about them to the Tongan king George. He sent his warrior nephew, Maafu, with a fleet of war canoes, manned by Tongans and armed with cannon, to rule the Tongan colony in Lau. By political strategy Maafu extended his power westward through the islands, including most of Vanua Levu. He secured the backing of the missionaries by promising them converts and credit from the traders by promises of valuable trade items from vanquished enemies.

Meanwhile on the island of Mbau off the east coast of Viti Levu, a petty chief had gained power over all his rival groups with the help of firearms and western military tactics. European trade interests were threatened by constant bloody raids and wholesale cannibalism in Fiji, and the chief of Mbau was held responsible by the United States government for damage to life and property of American citizens to the sum of $45,000. Threatened by Tongan power on the east and European and United States interests, the chief of Mbau negotiated with the British consul in Fiji. Annexation followed in 1874, Fiji becoming a British Crown Colony.

The power of Maafu, the Tongan chief, was ended when foreign trade interests forced him to make peace with the Fijian chiefs in order to protect the bêche-de-mer trade on the north Vanua Levu coast. Tongan political power in Fiji thus came to an end after more than 200 years. Many cultural features, of course, survived. In language, technology, economics and psychological temperament, Tongan and Melanesian features are combined.[13]

In the past, the political power of the dominant Polynesian chiefs was embodied in an official religion. War heroes were deified and placed in a hierarchy according to the deeds they performed while alive, and their degree of relationship to the original warrior-leader. These ancestor gods were propitiated for power or mana in order to enhance social prestige, the highest social value of the invading Polynesians. Hereditary priests carried on the rites in small oval or rectangular temples with high thatched gabled roofs built on round or oval mounds. By contrast the older Melanesian religious practices were carried on in the open among the populace by individual non-hereditary medicine men.

Direct European influence was first brought in by missionaries, then political officers and traders. The ancestor gods were demoted to the status of devils, but naturally they continued to be feared. Changes appeared first in the religious, then in the social, and finally in the economic life.

During World War I the copra trade ended abruptly. People had been neglecting fishing, gardening, canoe building, and inter-island trade in favor of work on sib coconut plantations. They returned to the older economic pursuits.[14] In 1933–34 the natives imported only a few types of goods, e.g., axes, bush knives, cooking pots, trade cloth.

In religion, the outer forms of Christianity were incorporated into the native ceremonial life, but the inner conflict caused by weakening of the ancestor cult had not yet been satisfactorily solved. This conflict tended to "sap vitality from the culture." According to Thompson's estimate at that time (1933–34) it was to a great extent responsible for the restlessness of the natives, in spite of their growing economic and social stability.

What are the lessons to be learned from this case of a Polynesian-dominated Melanesian people, later conquered by the Europeans? First, that after ten generations of domination the Polynesians did not succeed in obliterating the older stratum of culture despite the fact that it was apparently simpler and politically less highly organized, and, second, that after a century of European dominance, with all of Britain's military might and the contrasting pattern of European culture, they have not succeeded in erasing either of the older cultural layers. Both older patterns survive, dispersed throughout the population as varying practices in different localities and among different individuals.

Further, that a considerable degree of political and social domination can be established by a relatively small group of men who marry native women, leaving the actual physical composition of the population largely unaltered, despite the fiction carried by the people themselves; and finally, that religious and political sanctions are likely to assort themselves within the consciousness of individuals and to survive as parallel systems of anxiety.

Today in times of trouble the commoners offer kava and food at the base of their ancestral totemic tree; while they formerly participated in the outer form of the ancestor cult they never accepted its inner content. The ancestral gods of the upper class were incarnated in a species of tree, a species of fish, and a species of bird. It was tabu for clan members to eat or harm them and "this tabu is upheld by some Lauans today" (Thompson, p. 110). The apparent success of the Christian mission has failed to obliterate the ancestral gods entirely: ". . . a few courageous individuals still propitiate their ancestors secretly in the bush." (p. 113.) Buell Quain[15] in his detailed psychological study of a Fijian village in the large western island of Vanua Levu states that:

> "The dissonance between theoretical hierarchy . . . and the random and confused determinants of status has a historical background. The system of chiefly titles and hereditary status is Tongan and the prestige of Tongan invaders spread and buttressed it in this region of Fiji. . . . The whole complex has been accepted without complete obliteration of an earlier ethos. For each region of Nakoroka there are two sets of ancestors. The first are "owners"

> of the land, sometimes identified with animals; the second are chiefly immigrants who built an empire at Flight-of-the-Chiefs and then dispersed to found chiefly houses throughout the province.
>
> "Tongan influence has not been so strong in all parts of Fiji as it has been Nakoroka.
>
> "This structural flaw in the ethos of Nakoroka works itself out in the life and character of all men in the village. They are uncertain of themselves; they are touchy about their status."

Tongan influence was not equally strong in every locality. Dorothy Spencer[16] reports that in Tholo West, Viti Levu, chiefly titles imply little authority. There is not the hierarchal subordination that there is in Quain's village of Nakoroka.

> "Dr. Spencer describes the noisy quarreling in which people at Nasanthoko are constantly engaged; there is not the careful modulation of voice and gesture that is so marked at Nakoroka. Nor is Nakoroka's constant phrase *via-via-levu*, 'acting out of place,' 'acting beyond one' so common Nasanthoko vocabulary."

Laura Thompson remarks that in the Lau islands:

> "Even today the land people are less restricted in daily life by formalities than the chief group. They seem to be more modest and liberal and to have more sense of humor. . . ."[17]

Thus we see that as a result of the survival of past social sanctions in his psychology, the Fijian today is the victim of parallel systems of anxiety. He also has parallel technological methods of manufacturing bark cloth. These, however, do not cause him similar difficulties but, contrariwise, offer him added cultural resources. At a Lauan wedding the groom was heavily swathed in Tongan tapa cloth which he removed and placed at the bride's door revealing his body garments made in the old Fijian method. The bride was similarly outfitted.[18]

In his study of the psycho-physical medical problems of the Ba-Thonga people of southeast Africa, Dr. A. Liz Ferreira[19] has noted a similar accumulation of anxiety patterns. The difficulty of repressing or removing these sanctions can only be appreciated if we realize that the penalties for their violation were most commonly threat of disease or supernaturally-caused death.

Closer to the American scene we have the combined ethos of a Hopi Indian woman as described by Dorothy Eggan[20] in the content of dreams:

> ". . . the strength of tribal attitudes toward cooperative obligations toward the tribe, frequently masked by surface changes, is well illustrated in dreams, as is also the superficiality of Hopi conversion to Christianity. A survey may show that 30 per cent of a village is Christian in that they attend a missionary church; but their dreams indicate that the majority of the old Hopi who list themselves as Christian have as much respect for Masau'u, and many other Hopi deities, as they ever had.
>
> "These people sometimes come back from college to bow to the will of the old grandmother who is the head of their clan. One such woman, an excellent pianist, who lists among her possessions a piano, a refrigerator, and a washing machine, notes in an interview that she belongs to a church in Los Angeles where she lives most of the year. But may we judge accurately by these things the degree of this woman's acculturation when she says in association to a

> dream, 'Oh, I'm going to leave my husband . . . because I have my girl baby at last and don't need him any more. They (the clan) have given me my fields and my brothers will plant for me'."

Felix Keesing,[21] remarks:

> "Among the vast majority of native Christians in the tropical islands, bodily states are still considered a reflection of supernatural forces of good and evil, and treatment of sickness is approached accordingly." (p. 207.)
>
> "The investigator is likely to find, indeed, not only apparently sophisticated natives but also many persons of mixed descent and even whites subscribing to some elements of native belief and magical practice." (p. 228.)

Few, or possibly no individuals in modern America subscribe fully in their intimate psychology to a completely scientific point of view. We are likely to be carrying a considerable baggage of surviving social sanctions, viz.,

(1) archaic magical superstitions
(2) astrological notions
(3) religious idealism
(4) business or commercial sanctions
(5) the outlook of experimental science.

The individual may bring to bear any one of these disparate systems to determine his attitudes or behavior in a given instance. One of our basic problems today is whether we can form an effective composite of the last three systems and eliminate the earlier contradictory ones. If we are to achieve a deliberate control of our own psychological and social processes, the development of a procedure of "historical unravelling" is essential.

A single pattern of culture is an unjustifiable fiction with relation to any group—an administrative hope, perhaps, but not a social reality.

From these brief observations it is evident that our history is always with us[22] and unless we unravel our past, step by step, and look at each stage out of its present context, we cannot understand our lives today. The means for this study are given in a detailed consideration of variations in the present, coupled with a study of past records.

With regard to the cultures of Europe, an extremely interesting study is that of Hugh Hencken, "Indo-European Languages and Archeology."[23] Hencken has furnished evidence through a correlation of archaeological and philological materials for a number of composite cultures that apparently preceded the now dominant Indo-European. I want to mention especially his analysis of Ligurian as Indo-European-modified-Celtic (pp. 33–34) and the history of the cuneiform Hittites who between 2000 and 1200 B.C. apparently carried an Indo-European social structure and language into central Asia Minor and there combined these features with an oriental material culture and religion (pp. 39–42). Boas' general principle of the independence of race, language and culture in the stream of history is receiving added support from this material. No one of these factors can alone define the ethnic unit.

I would conclude that:

(1) A dominant system of social controls best serves to delimit an ethnic entity at any time,
(2) that nevertheless, the identification of current ethnic groups cannot be made by a characterization of the dominant system of social controls alone;

(3) since within the orbit of any dominant system of social controls there are a number of surviving elements of previous ethnic entities which may maintain their identity to a considerable degree;

(4) and that the functional significance of these surviving entities can be evaluated only after they have been isolated by a process of historical analysis.

There was no time in human history when an ethnic group did not include a number of ethnic patterns from previous ethnic groupings. Whatever system of priorities one may use in evaluating the various groupings within a social entity, an attempt to attribute an all-embracing character to the pattern of the dominant social mode, to the exclusion of the divergent social elements which it must include, will materially hinder the growth of a scientific ethnology. To accomplish an adequate ethnic analysis of human cultures, a combination of all the techniques of historical reconstruction—ethnological, archaeological and linguistic[24] must be utilized. Only by the refinement of our techniques for deriving the ethnic unit in its many socio-historical contexts will we develop a solid base for the general science of anthropology.

New York, New York.

Notes

1. See also G. Weltfish, 1956.
2. Swanton, 1952, p. 613.
3. Movius, 1949, p. 408.
4. Marrin, Quimby, and Collier, 1947.
5. Possibly these should be called archaeological "techniculture."
6. Phillips and Willey, 1953, p. 617.
7. Rouse, 1955, p. 718.
8. Bennett, 1953, p. 7.
9. *Op. cit.*, p. 14.
10. Thompson, 1940, p. 26.
11. Thompson, 1938.
12. Spencer; this account is based on field work in interior Viti Levu, 1935–36; Thompson's report is of 1933–34.
13. See Thompson, 1940; language, pp. 13–14; barkcloth technologies, pp. 56–57.
14. Thompson, 1938; on page 196 the return of the older economic pattern is described in the following terms: "Neglected gardens were cleared and replanted, native crafts began to flourish, trade between the islands revived and the whole daily routine resembled olden days. The hereditary master-fisherman regained control of the communal fishing. The first fruits of the harvest were again presented—now to the old chief and the native colonial official jointly, while in the earlier periods they had been offered to the gods and in the middle period to the chiefs. Large single sailing canoes with improved rigging due to western influence have replaced the cutter, but the double canoe has disappeared. Wooden bowls, but with less skill than in olden days, are made in large quantities. Tapa and mat making industries are again flourishing and native rope, fish lines and fish nets are replacing imported articles. See also Thompson, 1940, and Keesing, 1945, p. 135 on a parallel instance in Samoa in 1934.
15. Quain, 1948, pp. 433–434. This is a report of field work in 1935–36.
16. Spencer, 1941.
17. Thompson, 1938, footnote 40, p. 186.
18. Thompson, 1940, pp. 55–56.
19. de Liz Ferreira, Mss.
20. Eggan, 1952, p. 479. See also pp. 475–476 for the significance of an old woman's dream.

21. Keesing, revised edition, 1945.
22. See Weltfish, 1956.
23. Hencken, 1955.
24. I do not mean to slight the possibilities for historical reconstruction through the use of physical-anthropological techniques. But they have been so heavily slanted on the medical and physical-genetic side that their interpretation for deriving ethnic units requires more intermediate steps than the cultural disciplines. See the highly significant article of Frederick B. Thieme, "The Population as a Unit of Study." "From what is known about the structure of human societies, it is hard to imagine any human population that practices random mating, particularly if short intervals of time are considered. We know that the question of 'Who mates with whom?' will be answered largely in cultural terms. . . . However, we need to know the answer to this question in terms of the genetic structure.

"If the interaction of these various factors is kept as the goal, the arrival at a solution will depend upon the degree in which they are related to the basic unit of study, namely the population, or the breeding unit. . . . Not only do we need to know the frequency of characteristics in population, but we must need to know about the formation of isolates (sub-groups) within larger populations. . . .

"In terms of the mating system of a single social unit, endogamy or exogamy are crucial. . . . How big a social complex is involved for a particular area, before the single exogamous patterns combine to give area endogamy? Closely related to this question is the important problem of assessing the reality of the mating structure in contrast to the expressed ideal. . . ." (p. 506.)

"In a complex society, the problem of defining the endogamous unit is most difficult. Religious, caste, racial, economic, educational and class groups, as some important examples, may, more or less, constitute breeding isolates.

"As a process of evolution is seen only in reproducing populations, the characteristics of fertility are important. Which segment of a population enjoys high effective rates of fertility determines future gene frequencies if the genes are not evenly distributed in the population. Related to this are group attitudes toward the proper size of family, economic status, individual and group attitudes concerning participation in the society, and a host of other culturally mediated mechanisms. It seems that the knowledge of human fertility at the present day suffers more from ignorance of the cultural effects than of the biological. It is the attitudes and role of the individual in his society that seem as important as his biology in determining effective fertility." (p. 507.)

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SECTION III

CULTURE CHANGE AND CULTURE HISTORY

SOME SIMILARITIES BETWEEN ARDS OF THE BALKANS, SCANDINAVIA, AND ANTERIOR ASIA, AND THEIR METHODOLOGICAL SIGNIFICANCE

Branimir Bratanič

The center of the Balkan Peninsula is characterized by a special type of ard[1] which differs entirely from other types of ards used eastward and westward from there (fig. 1).[2] This difference consists not only in the construction of the implement as a whole and the particular shapes of its constituent parts, but also the central type has a nomenclature quite dissimilar to that of the other ard types[3] which we shall call, because of their geographical situation, marginal ones. All of these marginal types have certain formal qualities in common, and their nomenclature, too, is congruous. The question before us evidently implies two entirely different traditions in the use of ploughing implements. Nevertheless, both types of nomenclature are of Slavic origin, and both can be traced back to other Slavic peoples. But an ard type corresponding entirely to the central Balkan ard does not exist among any other Slavic people, and also nowhere else in the world. It is therefore interesting that a considerable number of its characteristic features are found in ploughing implements in the Scandinavian area, especially in the most distinctive type of Swedish ard, *grindårder*

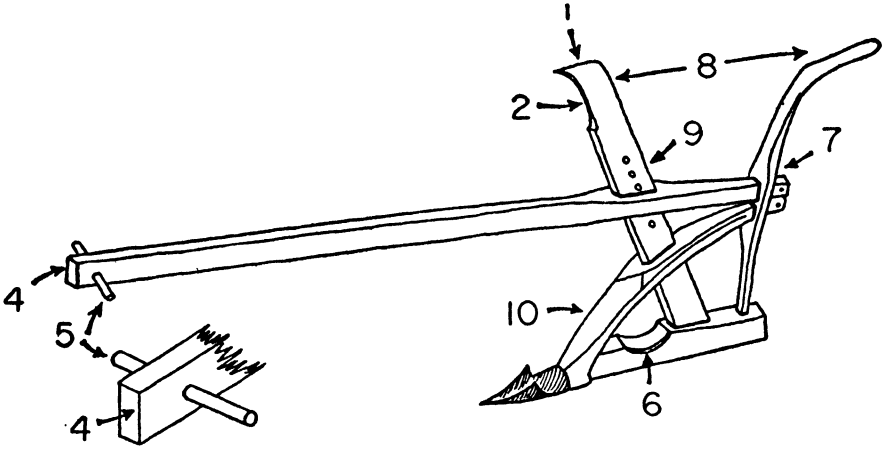

Fig. 1. Yugoslavia. Drawing somewhat schematized: all relevant qualities assembled in same specimen, which very rarely, or hardly ever, occurs in reality

(fig. 2).[4] Both kinds of ard, the Balkan and the Swedish type, show the same basic construction of their frames: both are "four-sided." But this is not decisive. "Four-sided" ploughing implements are spread very widely, and many reasons make it probable that this particular constructive trait has grown out of several different older constructions independently, so that this would be an instance of cultural "convergence". On the contrary, a set of minor specific distinctive traits, by which both areas are joined together, seems to be more significant.

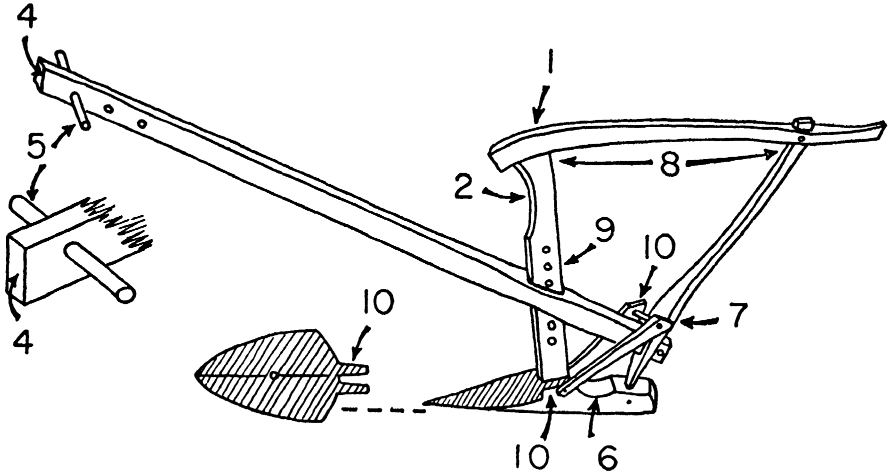

Fig. 2. Sweden. Drawing somewhat schematized: all relevant qualities assembled in same specimen, which very rarely, or hardly ever, occurs in reality

Here they are: (1) the high sheath which frequently serves as a second stilt for guiding the implement (in Scandinavia, moreover, the sheath is joined with the stilt by a special stave so as to form the characteristic "frame-handle"); (2) a cut-out on such a high sheath for the hand of the ploughman (or, in Scandinavia, a special handle on the fore-end of the horizontal stave joining the stilt and the sheath); (3) the position of the ploughman (he walks not to the rear of the implement, but at the side of it, often grasping the implement with both hands, one upon the stilt, the other on the sheath, or on the "frame-handle"); (4) the shape of the plough-beam which is squared so as to form, in cross-section, a parallelogram set on edge; (5) the yoke-pin put through the beam in horizontal position; (6) the rather high sole, bevelled on its fore-end, and frequently cut out, in a roundish fashion, upon the top-side of it; (7) contrasting with this, often a very slender and slim stilt, widened only at the place where the beam is mortised into it; (8) frequently the characteristic appearance of the whole implement because of the strongly diverging top-ends of the stilt and of the sheath; (9) sometimes a row of holes on the sheath for managing the depth of the furrow; and (10) a certain similarity is shown thereby in that the Scandinavian share sometimes embraces the sheath with a fork-shaped tang, and that it is placed somewhat obliquely, in several cases even somehow prolonged, by the obliquely fixed mouldstrokers, to the place where the beam and the stilt join, which, all taken together, reminds one of the characteristic oblique mouldboard of the similar Balkan ard.

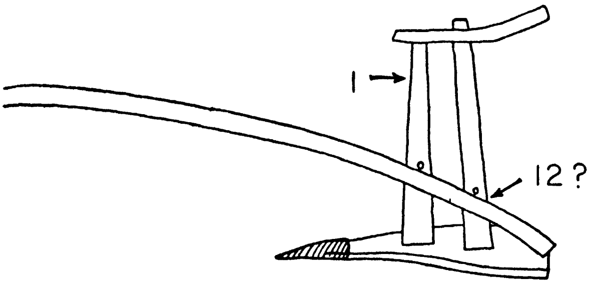

Fig. 3. Azerbaidjan. Free after Feilberg, 1936, fig. 7

A number of the formal peculiarities in question[5] appear also in a part of Anterior Asia, comprising Armenia, the Caucasian countries, and northwestern Iran, similarly on ploughing implements which can be regarded as "four-sided". Moreover, there are some formal features that connect ploughing implements of this area with those of Scandinavia, although not occurring, generally, in the Balkans. It has long ago been pointed out by various authors that the Scandinavian "frame-handle" and the characteristic position of the ploughman had their analogues in these parts of Western Asia. To this, some further constructive particularities can be added: (11) in both areas the plough-beam is frequently not mortised into the stilt (construction *a*), but the stilt passes through the beam (construction *d*); (12) sometimes the beam and the stilt are not joined by mortising at all, but by scarfing or by tying together (construction *n*); (13) in some cases the plough-beam is put into a large aperture at the lower end of the stilt, where its height (and, consequently, the depth of the furrow) can be regulated by the aid of several small holes (much as is done otherwise at the sheath); (14) a particular shape of the beam, which is, in substance, straight and only bent twice at its rear end so as to render this rear end somewhat lower, but parallel to the whole beam (shape *H*, cf. fig. 4). On the other hand, the most important part of the central Balkan ard, its characteristic middle-placed oblique mouldboard, can be manufactured (15)

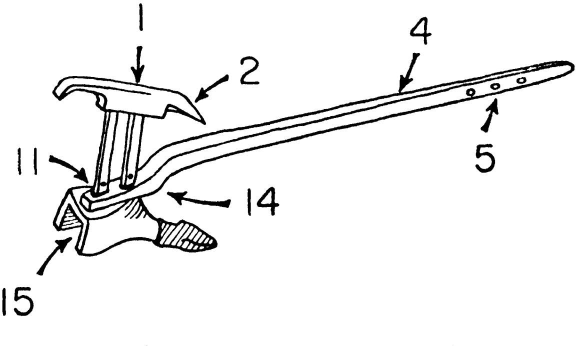

Fig. 4. Southwestern Shore of the Caspian Sea. Free after Leser, 1931, fig. 215

of a huge, hollowed-out-from-below, block of wood, and a very similar shape (formally and functionally) of the sole is found in some ards of Transcaucasia, thus forming another congruity between these two areas.

None, or almost none, of the aforementioned peculiarities are required by the purpose of the implement or by the material of which it is made. They are not found in large parts of the agricultural zone of the world, where similar results have been reached by other means. It is not likely, therefore, that they might have originated independently, and they are too many so that their existence might be accounted for by chance. Consequently, some historical connections of one kind or another are to be assumed, joining together these three centres where a greater or less assemblage of the distinctive traits in question occurs on local ploughing implements. But the distance between them is great and they are populated by different peoples, with very different languages, while no direct historical relations ever connecting them are known. Still, the region intervening between these three areas is not entirely devoid of all traces of some possible connecting links between them. Some of the ard types resembling as a whole the central Balkan ard can be traced northwestwards, across Slovenia and Austria, to central Bohemia, analogies including partly also the typical Slavic nomenclature of the central Balkan ard. Further, some detached instances of the formal qualities and names in question can be found, in quite different ard types, in some other Slavic areas (parts of Ukraine, Poland, and White Russia, including the territory of the western, asymmetrical, type of sokha), in (former Slavic) Mecklenburg, and in the Baltic countries. It is significant, however, that Slavic names belonging to the nomenclature of the central Balkan ard, are confined only to such implements of Western and Eastern Slavs as show, at least, also some analogies to its formal peculiarities, whereas the nomenclature of other Slavic ards resembles that of the marginal ard types of the Balkan Peninsula. This has permitted some historical and

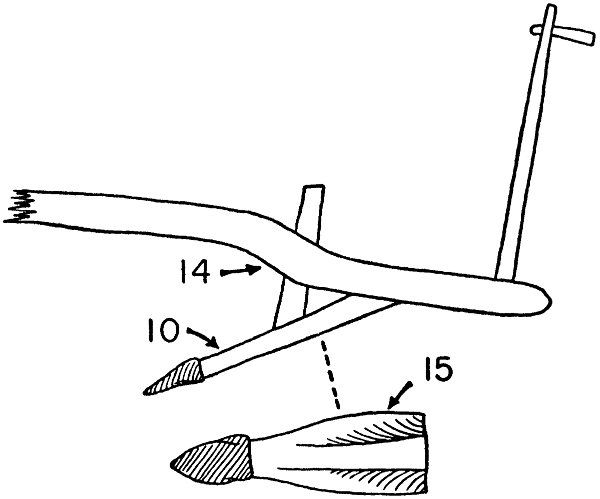

Fig. 5. Ukraina. Free after Moszynski, 1929, fig. 125

chronological inferences elucidating the existence of that special ard type in the Balkans, which have been dealt with in a former paper (Bratanić, 1951). Besides nomenclature, the chief resemblance between the Central Balkan and the Polish-Ukrainian-White Russian areas is that the working parts of the ards of that area and of the one-sided sokha, being without sole, largely correspond (both in their shapes and functionally) to the central oblique mouldboard of the Balkan ard. This agreement would be even greater if we should have more and stronger evidence for a Bosnian ard without sole (cf. Bratanić, 1953, fig. 4, 5). Moreover, some of these implements are held by both hands, having frequently two handles, shaped differently, so that the ploughman is obliged to hold one handle "off himself" and the other "towards himself,"[6] exactly as is done with the central Balkan ard, and probably also with corresponding ards of Scandinavia and Anterior Asia. The same implements often show the particular shape of beam and horizontal position of the yoke-pin (traits 4, 5) which we know from the Balkans, Scandinavia, and Anterior Asia. With the ards of the latter two areas some Ukrainian ards share the stilt passing through the beam (construction *n* = trait 11), and the particular, twice bent, beam (shape *H* = trait 14; cf. fig. 4). And a kind of "frame-handle," a stave going obliquely from one handle to the beam, is characteristic not only of a great many ploughing implements of northern Ukraine, White Russia, and Baltic countries, but it is also rather common in Scandinavia (besides the ordinary "frame-handle") on different ard types, and reaches even to the Shetland Islands.

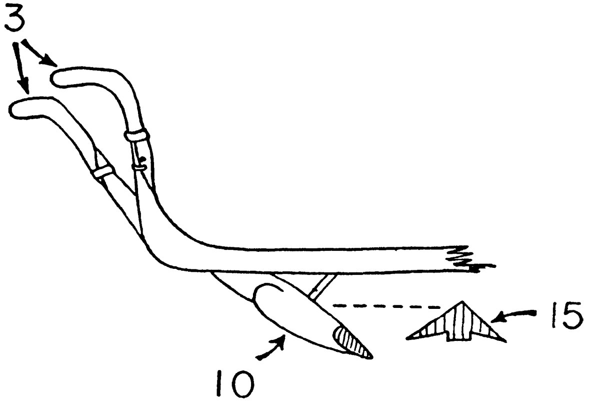

Fig. 6. Poland. Free after Moszynski, 1929, fig. 127

The special investigations which have been made up to the present have been to a certain extent adequate, unfortunately, only for the Swedish, Finnish, Esthonian, and for a part of the Yugoslav and Bulgarian areas. Still, the present evidences already seem to indicate a vast territory of similar tradition concerning ploughing implements, extending from the Caspian Sea and the central part of the Balkan Peninsula across the eastern half of central Europe and a part of eastern Europe, to the Scandinavian north (fig. 7). It must be emphasized that none of the distinctive traits in question are to be found, normally, in the Mediterranean area, in western Europe, or in the western part of central Europe. On the contrary—although this is not very relevant to the present survey—

some traces of the same peculiarities of ards seem to have spread far to the east, to the south, and to the southeast: to central Asia (traits 4, 5, 15) and Egypt (4, 5), India (1), Burma (1, 4, 5), Cochinchina (1, 15), and, perhaps, across China (9, 10) as far as Korea (1).

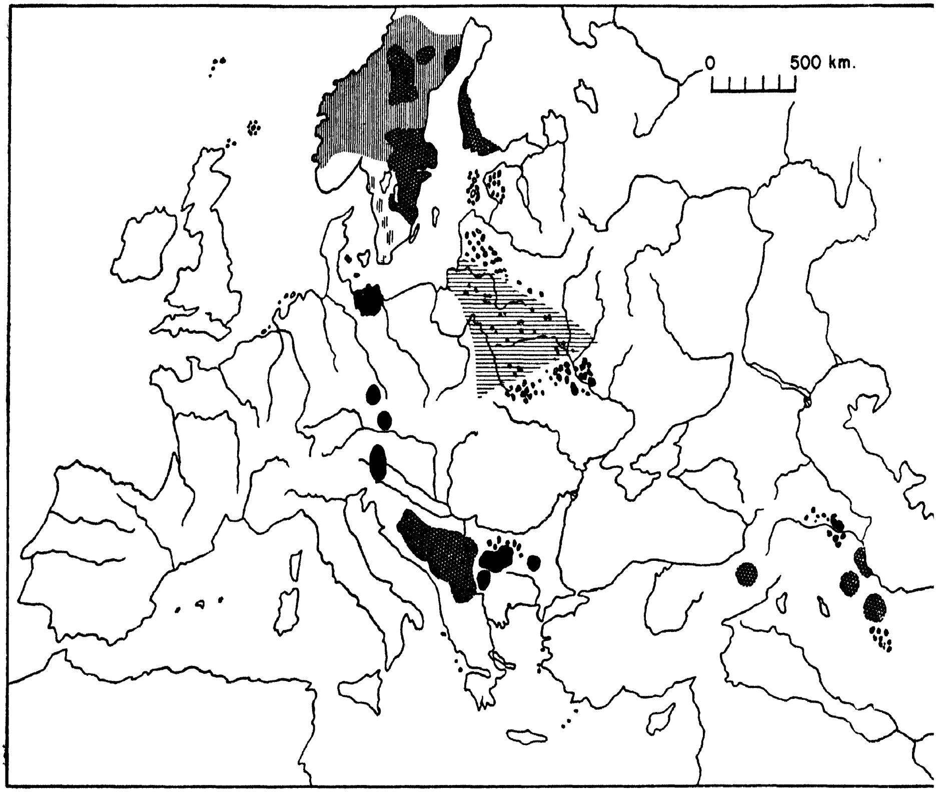

Fig. 7. Map of Distribution of Ard Types

■ A number of characteristic traits assembled on certain "four-sided" ard-types (in Scandinavia also on other ploughing implements of similar construction).

▨ Detached characteristic traits, more or less expressed, on various types of ploughing implements.

▩ The area of the western, one-sided, sokha.

The facts hastily set forth here, and their characteristic geographical spread, cry to be explained in a proper scientific way (not to be carelessly explained away). All this may be results of a variety of factors, but the main problems arising here are of historical nature. It is impossible, in a short report, to point to all methodological implications arising from the situation which has just been outlined. Of course, for the ultimate solution of such problems the coordination and synthesis of the results of several sciences, such as history, archaeology, ethnology, linguistics, and physical anthropology, are desirable and necessary. But this synthesis must be a real collaboration, not a mutual cribbing, renarration, or relying on ready-made concepts belonging to another discipline, which so often leads to a *circulus vitiosus*. It is not unknown that "we tend to try to settle things much too fast" (Forde, 1953, p. 82). We even tend to explain and to "understand" them before we know them sufficiently. Based on quite insufficient, frequently unreliable, and usually vague and unclear data from written sources or from archaeological finds, a general picture concerning a certain cultural problem frequently comes into being, and this—supported by eventual reputation of its author or by agreement of a number of those who have nothing to say about the special issue in question—can become settled, so that judgements on every single phenomenon are formed according to it, and new evidence and new opinions are accepted, discarded, or ignored respectively after this fashion. Such predilections to explain every new particular phenomenon or new question by a ready-made conception of a general cultural height (actual or supposed), of a social structure (actual or supposed), or of an economic system (actual or supposed), are not only the source of many wrong interpretations of the disposable material, but can become a serious obstacle for all true scientific work. However, if historicity is not restricted to written documents or archaeological finds, if history—and especially cultural history—is not regarded as what various authors, at various times, have thought it was, or wished it to be, but as what actually "happened in history," then it must be recognized that by far the largest part of human cultural history is recorded solely in the products of culture, these representing its only documents. If this major part of culture history is not to be entirely abandoned, it must be reconstructed by a cautious, difficult, and long-lasting procedure. Thereby the importance of a critical ethnological research, working back from the present into the past, from the known to the unknown, from the particular to the general (not inversely), becomes enormous. This ethnological research can show, it is true, various types of approach to its material and various objectives (historical reconstruction being one of them), which may require different methods and techniques of investigation. These different objectives and methods can supplement each other's deficiencies and present a mutual cross-fertilizing influence. But there is, regularly, a certain logical order in the process of scientific research, a gradual progressing, in which one stage of research always presupposes the preceding one. Various methodical procedures must never be confounded or mistaken for others in the course of investigation, and their single stages must not be skipped over or anticipated. Before the questions of "how" and "why" can be posed, the question of "what" (has actually happened) must be answered. That is, historical facts and connections themselves must be scientifically established first, and only then can they be explained in terms of causality. For this first and indispensable step in reconstructing cultural history, every detail—perhaps quite insignificant in other respects—can be of greatest importance. We cannot "understand" or judge a man by the colour of his eyes or his hair, by his stature, by the shape of his nose, by his blood group, or by his finger-prints. These traits tell us nothing about his social or economic status, about his abilities, or his human values, nor can we predict his behaviour in various situations by means of them. But we cannot identify a man in every-day life without the help of such traits, and they can render it possible to prove his paternity or, eventually, some crime of his, too. For analogous identifying and detective work in ethnology, quite indispensable in itself, very abundant and very detailed comparative material is needed, which can be obtained, in a satisfactory manner, only by a systematic and coordinative research on a broad international basis. Beyond this, a systematic body of work with an adequate organization is also required. But without this humble preparatory work, requiring much time, patience, energy, and also certain material resources, speculation cannot be replaced by scientific proof, and no final synthesis can be made satisfactorily.

Ploughing implements are a most promising subject for such investigations. A large literature has already dealt with them for a long time. Nevertheless, we are still rather far from possessing sufficient, systematically collected, and reliable material, as indicated in the present short survey. However, they really constitute a rare bridge into the past. But "now that bridge is being rapidly destroyed" (Forde, 1953, p. 18), and it is to be feared that it may be, perhaps, completely destroyed before we can make the proper use of it.

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Notes

1. This word denotes a symmetrical ploughing implement, without an unilateral mould board.
2. Constructive formula *Aαa3*. For the symbols denoting single constructive elements of ploughing implements, and for constructive formulas denoting different technical types of the construction of their frames, see Bratanić, 1953, Fig. 1, and Bratanić, 1955, Fig. 2.
3. Constructive formulas *Aa1*, *Bb1*, *Ca1*, etc.
4. Constructive formula *Aa3*. It is perhaps worth mentioning that this special type of ard, in its normal form, shows a rather similar central position in Sweden, with corresponding marginal types on both sides, like its southern analogue in the Balkan Peninsula.
5. The high sheath (1), sometimes with a handle at it; the position of the ploughman (3), his grasping the implement with both hands; the shape of the beam (4), and the horizontal position of the yoke-pin (5); the high and beveled, or obliquely rising, sole (6); the special shape of the stilt (7).
6. From which the specific Polish names of these handles are derived.

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SLASH-AND-BURN AGRICULTURE: A CLOSER LOOK AT ITS IMPLICATIONS FOR SETTLEMENT PATTERNS

Robert L. Carneiro

Primitive peoples in forested areas throughout the world practice a system of agriculture which is known variously as slash-and-burn, milpa, and shifting cultivation. Its general features are as follows. Early in the dry season an area of forested land selected as a garden site is cut, and the trees and undergrowth left on the ground to dry. A few months later the dried vegetation is burned. At the beginning of the rainy season the crops are planted. The wood ashes that remain on the ground restore some minerals to the soil, but otherwise no fertilizer is used.

The same plot is replanted until a decrease in the fertility of the soil, or, especially, the invasion of weeds and grass, makes it uneconomical to do so any longer. At this point it is abandoned, and a new area of forest is cut down, burned, and planted as before. An abandoned plot becomes so overrun with weeds and grass that even if its fertility soon recovers, its recultivation by such simple means as digging sticks or hoes is made almost impossible. To be able to recultivate a once-abandoned plot a milpa farmer must generally wait until a new cover of forest has grown up and shaded out the smaller vegetation. This usually takes many years.

Since under this system of agriculture land is exhausted at a faster rate than it recovers, the area of arable land held in reserve for future cultivation must be several times larger than that currently planted. Therefore, only a fraction of the habitat can be exploited agriculturally at one time.

In a number of societies practicing milpa agriculture it has been observed that villages sometimes have to be moved because the nearby arable land is exhausted. The occasional relocation of the village because of soil depletion has been taken by many writers to be a necessary consequence of slash-and-burn cultivation—something inherent in the system itself. To give but one example, V. Gordon Childe[1] has written:

> Under . . . [slash-and-burn] cultivation any plot will become exhausted after one or two croppings. The simplest reaction is to start again on a fresh plot. The repetition of this process soon uses up all the land conveniently accessible from a single settlement. Thereupon, the whole settlement is transferred to a new location and the cycle repeated here (p. 198).

Although many students are quite ready to assume that the depletion of the land inevitably brings about the relocation of villages, no one, to my knowledge, has ever attempted to demonstrate this in a rigorous way. A proposition with such important implications certainly bears testing in the light of the data at hand. This is what I propose to do here.

My interest in this problem was aroused during field work carried out among the Kuikuru Indians of the Upper Xingú region of central Brazil. I discovered that the Kuikuru, who subsist very largely by the slash-and-burn cultivation of manioc, have maintained their village in the same locale for the last ninety years. It is true that during that time they have had four different village sites, but all of them have been within a few hundred yards of each other. Furthermore, what thrice has prompted the Kuikuru to move their village was not the depletion of the soil at all, but rather supernatural reasons of one sort or another.

The Waurá, a neighboring tribe with a mode of subsistence like that of the Kuikuru, also have shown the same pattern of settlement. Writing about them Pedro de Lima[2] has said:

> According to information that we obtained, the Waurá have lived in the same place for many years, having had a number of village sites, all near the present one. In the course of time [probably 100 years at least] they have built no less than 10 villages, each new one being 100 or 200 meters distant from the previous one. These moves are motivated by superstitious beliefs. (p. 5; my translation.)

Thus, to all intents and purposes, the Kuikuru and the Waurá, shifting cultivators *par excellence*, have nevertheless been able to remain sedentary. We see, therefore, that slash-and-burn agriculture *can* be compatible with permanent settlements.

This conclusion in no way denies the kernel of truth contained in the commonly accepted theory about the implications of slash-and-burn farming. Under certain circumstances shifting cultivation may indeed bring about periodic relocations of the village. What is needed is some technique for evaluating the various factors involved in order to determine how, by their interplay, they either permit or prevent fixity of settlement. This problem is not only soluble, but lends itself to precise, even mathematical formulation as do very few others in ethnology.

The significant variables are six in number, and all of them are capable of being quantified. These six variables, with appropriate symbols for each, are the following:

A	the area of cultivated land (in acres) required to provide the average individual with the amount of food that he ordinarily derives from cultivated plants per year.
P	the population of the community.
Y	the number of years that a plot of land continues to produce before it has to be abandoned.
R	the number of years an abandoned plot must lie fallow before it can be recultivated.
T	the total area of arable land (in acres) that is within practicable walking distance of the village.
L	the length of time (in years) that a village can remain in a single location in so far as the requirements of agriculture are concerned.

Using these variables it is possible to construct several formulas[3] each of which enables us to solve for a particular unknown when the values of the other variables are known. Thus, formula (1): $P = \dfrac{\dfrac{T}{(R+Y)} \times Y}{A}$, will tell us how large a population can be supported permanently in one locale given certain values for the terms on the right-hand side of the equation.

If, on the other hand, we wish to determine the smallest area of cultivable land that will support a village of a given size in the same locale indefinitely, we make use of a different formula, (2): $T = \frac{P \times A}{Y} \times (R+Y)$.

And if we wish to know how long a community can remain in the same place before soil depletion forces it to move (if it ever does), we use formula (3): $L = \frac{T}{(P \times A)/Y}$, where L is less than $(R+Y)$. If L turns out to be equal to or greater than $(R+Y)$, then, for reasons to be made clear in a moment, the locale in question can be occupied indefinitely.

Having exhibited some of the formulas that can be derived from the variables, I would like to try to prove mathematically what we already know historically: namely, that the practice of slash-and-burn cultivation by the Kuikuru has not made it necessary for them to change the location of their village. In order to do this we must be able to substitute actual numbers for the symbols. The values assigned to the variables were determined during the course of field work, and later refined with the help of aerial photographs. They are as follows:

A (the acreage of manioc required to support the average person for one year) $= .7$

P (the population of the community) $= 145$

Y (the number of years a manioc field produces before being abandoned) $= 3$

R (the number of years the plot must lie fallow before it can be recultivated) $= 25$

T (the acres of cultivable land lying within practicable walking distance of the village) $= 13{,}350$

L (the length of time in years that the village may remain in the same locale), is what we are solving for in this problem.

The appropriate equation to use is (3):

$$L = \frac{T}{(P \times A)/Y}, \quad \text{if } L < (R+Y). \quad (\text{If } L \geqslant (R+Y), L\ \infty).$$

Substituting numbers for the symbols we have:

$$L = \frac{13{,}350}{(145 \times .7)/3}; \quad (R+Y) = 28.$$

Solving the equation we get $L = 395$, and since L is greater than $(R+Y)$, L is infinite. Let me try to clarify the meaning of this answer. The value 395 for L represents the number of years it would take the Kuikuru to plant and exhaust successively all of the arable land conveniently available to them. Since this period is much longer than the 25 years it takes for a plot to be exhausted and recover, it is clear that at the end of the 395 years the Kuikuru could simply go back to the first plots and start all over.

Thus we have succeeded in demonstrating mathematically that under a system of shifting cultivation the Kuikuru have been able to remain permanently settled.

The fact that L turned out to be so much larger than $(R+Y)$ indicates that the Kuikuru are sedentary by a wide margin. In order to find out just how ample this margin is, let us calculate the size of the smallest area which, under prevailing conditions, would permit a village of 145 persons to stay indefinitely in the same locale. The appropriate formula in this case is (2): $T = \frac{P \times A}{Y} \times (R+Y)$. When we substitute the actual figures we have $T = \frac{145 \times .7}{3} \times (25+3)$. Solving the equation through we get an answer of 947.25 acres. This means that by utilizing only 950 acres, or about 7% of the arable land within an accessible radius, the Kuikuru could still remain completely sedentary.

Another question of interest is how large a village population, subsisting under the same conditions, could be permanently supported in the habitat of the Kuikuru. To arrive at an answer we use formula (1): $P = \frac{\frac{T}{(R+Y)} \times Y}{A}$, which, once substitutions are made, gives us $P = \frac{\frac{13{,}350}{(25+3)} \times 3}{.7}$. This yields as an answer, $P = 2{,}041$. That is to say, with slash-and-burn agriculture as the only limiting factor, a village of some 2,000 persons could live on a permanent basis where the Kuikuru do now.

The various formulas used here are of course perfectly general. They can be applied to any group practicing shifting cultivation, provided there are figures to insert in place of the symbols.

We have seen that the Kuikuru and the Waurá at least, do not have to relocate their villages periodically because of soil exhaustion. Now the major objective of this paper is to determine whether or not the same is true for the *average* community practicing shifting cultivation. In trying to answer this question I will use the Tropical Forest of South America as a proving ground, not only because I am most familiar with it, but also because it is a large and, I think, typical area of slash-and-burn farming.

The procedure will be first, to ascertain what the average horticultural conditions are for the Tropical Forest; second, to compute by means of one of the formulas how large a sedentary village could be supported under such conditions; and third, to match the figure thus obtained against an independent estimate of average village size for the Tropical Forest. The data that are needed to determine average horticultural conditions for this area are difficult to find in sufficient completeness or detail. For this reason the safest course is to take the figures we have for the Kuikuru, assume that they represent optimal conditions, and scale them down in order to arrive at "average" conditions. Since the figures for the Kuikuru appear to be much nearer average than optimal, the "average" Tropical Forest conditions we obtain by scaling them down will be a *low* average. This gives us the assurance that we are not presenting an overly favorable picture of horticultural conditions in the area.

I will begin by assuming that while the Kuikuru are willing to go 4 or 5 miles to till a field of manioc, the average Amazonian cultivators find it impracticable to walk farther than 3 miles. Taking 3 miles as a radius, I will assume further that of the area of the circle generated by swinging this radius a full 360°, only $\frac{1}{3}$ is suitable for cultivation. This is an area of 5,971 acres, compared to the 13,350 acres of the Kuikuru.

Next, whereas a field of manioc yields for about 3 years among the Kuikuru, I will assume that the average Tropical Forest Indians abandon a plot after 2.5 years. Furthermore, instead of allowing 25 years as the period of necessary fallow for abandoned plots, I will arbitrarily raise that figure to 30 years. Lastly, I shall assume that 1 acre (instead of .7 acre) is needed to grow the amount of manioc required per person per year. This is indeed a high figure; de Fauterau[4] has estimated that among the Indians of French Guiana, for example, the area needed for this is only .2 acre.

We now have the following numerical values:

$$T = 5{,}971$$
$$R = 30$$
$$Y = 2.5$$
$$A = 1$$

Substituting these numbers into formula (1) we have $P = \dfrac{\dfrac{5{,}971}{(30+2.5)} \times 2.5}{1}$, which yields as an answer $P = 459$. Thus, under distinctly low average conditions of agricultural subsistence, it would still be possible in a Tropical Forest environment for an Indian village of nearly 500 people to remain completely sedentary. (One can readily appreciate that if the figures used as average values had been nearer the true average, P would have come out substantially higher than 500.)

The next facet of the problem is to ascertain the average size of villages in the Tropical Forest to see whether it is larger or smaller than 459. This can be determined with reasonable accuracy by referring to a map compiled by Julian Steward[5] for Volume 5 of the *Handbook of South American Indians.* This map shows the distribution of community size for all of native South America broken down into the following class intervals: 1–50, 51–150, 151–500, 500–3,000, and 3,000–plus. It is quite evident from the map that the most typical community size in the Tropical Forest—typical in the sense of covering the largest portion of this culture area—falls into the class interval 51–150. Thus average village size in the Tropical Forest is well below the size that average horticultural conditions would permit.

On the basis of these findings I would venture the following suggestion: If the ethnographic or archeological record reveals periodic relocations of villages of 500 persons or less, causes *other than soil depletion* should be assumed to have been responsible unless there is clear and conclusive evidence to the contrary. What these other causes are constitutes an interesting problem in itself, but one which falls outside the scope of this paper.

Let me say again that under certain conditions—which formula (1) would reveal to us—soil exhaustion may indeed force a society to move its village. However, I am led to conclude that for primitive peoples in general permanence of settlement is certainly compatible with slash-and-burn agriculture.

The conclusions which I have reached in this paper are by no means novel. They were understood and expressed in the earliest systematic treatment of slash-and-burn agriculture that I know of, O. F. Cook's "Milpa Agriculture, A Primitive Tropical System."[6] Writing in 1919 Cook said:

> Milpa agriculture is a permanent system if the intervals between successive clearings of the same land are very long and the forest has time to restore the

> land to its original condition. [In this case] a few people can live indefinitely in the same region. . . . (p. 323.)

What I have done is simply to isolate the relevant factors, reduce them to variables which can be quantified, and arrange these into mathematical formulas. These formulas provide us with a means of answering several questions of interest. They also serve to emphasize that local conditions pertinent to shifting cultivation must be known in detail before statements concerning the permissible size or duration of a village can be made with finality.

New York, New York.

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3. The formulas presented in this paper are modifications of those contained in the paper as originally read. For pointing out the slight inaccuracies in the original formulas and for suggesting how they might be revised I am indebted to Dr. Albert C. Spaulding.

4. Eric de Fauterau, *Etudes d'Ecologie, Humaine dans l'Aire Amazonienne.* Fontenay-LeComte. Vendée, 1952, p. 3.

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NORTHWEST COAST — NORTHEAST ASIATIC SIMILARITIES: A NEW HYPOTHESIS

Chester S. Chard

One of the major ethnographic phenomena demanding historical explanation has always been the admittedly very striking similarities between the so-called "Palaeo-Asiatics" of northeastern Siberia—by which I mean here the Chukchi, Koryak and Kamchadal—and the Indians of our Northwest Coast. In the early days, this similarity was believed to extend to language and physical type as well as to culture, and people tended to think in terms of the identity of these two groups—not just their similarity. Even Boas suggested that the "Palaeo-Asiatics" were a backwash of people from the New World who had drifted back across into Siberia. As late as 1928, Jochelson labelled them "Americanoids." The general view was that the northeastern Siberians and the Northwest Coast Indians represented a single population that had been split apart in recent times by a wedge of alien Eskimo entering at Bering Strait. This in particular was Boas' conclusion as a result of the work of the famous Jesup Expedition around the turn of the century. Boas believed at that time in a central Canadian origin for the Eskimo, and considered them to be late comers to Alaska and Bering Strait.

This view prevailed down to the early 1930's, until the excavations by Collins and others on St. Lawrence Island upset the picture by demonstrating conclusively that the Eskimo were no newcomers, but had been well established around the Strait for at least 2000 years. Thus, the existence of any former continuum of culture between Asia and America necessarily was pushed back to a date too remote to account for such close modern similarities. In one important respect, however, the old view remained unchanged: the Eskimo at Bering Strait were still seen as a barrier between the Old and New Worlds.

So an alternative link had to be found through which more recent contacts could have been maintained, and the choice quite logically fell on the Aleutian Islands. From the mid-thirties onward, we find evidence being advanced in support of this new answer to the great problem of the North Pacific.

In recent years I have given some attention to this question: ethnographically, in connection with an analysis of the culture of the Kamchadal (Chard, MS); archaeologically, as part of a continuing study of northeast Asiatic prehistory. My conclusions have been that some minor and very recent cultural diffusion may be admitted via the Aleutian chain, corresponding conceptually to De Laguna's circum-Pacific culture drift, but far more limited: such items as sunshade hats, lamps with holes for a stick stand, the sadiron lamp, or the use of red-tinted seal hairs for clothing decoration; but that this cannot possibly explain the major, deeper similarities with which we are concerned here.

The evidence that has been advanced in support of the Aleutians as a primary route of cultural transmission is almost entirely distributional in nature: the existence of traits shared by northeast Siberia and northwest America, and also occurring in the Aleutians (thus strongly suggesting their diffusion via the islands); or so-called "split distributions" where the trait is absent in the Aleutians (and also at Bering Strait), suggesting the possibility of transmission via the islands. The direction of diffusion in any case is presumed to be indicated by the relative extent of distribution of the given element in the Old and New Worlds. Some support for the Aleutian route is also provided by the well-known seamanship and nautical exploits of the Aleut; nor should we overlook the equal, if less known, abilities of the Kuriles on the Asiatic side.

Only a very summary critique of this evidence is possible in so brief a paper. In general, the traits adduced are scattered items, not a coherent complex. In many cases, the major argument for diffusion of a given trait via the Aleutian route is its apparent absence in the Bering Strait area, either ethnographically or archaeologically. It seems to be generally assumed that the sequence of archaeological cultures here, as presently known, faithfully mirrors the cultural activities of this area for the past 2000 years, and that no trait could have entered or left the New World during this time without leaving some trace of its passage in the archaeological record. This is a debatable assumption. And in the case of ethnographic evidence, the absence of a trait in historic Bering Strait Eskimo culture seems hardly conclusive in view of the impact of Thule influences on that area.

As examples of the evidence for the Aleutian theory, I will cite four items.

The smoke-hole entrance to subterranean houses, while quite widespread in parts of western America, was believed to be restricted in Asia to the Koryak and Kamchadal. This distribution, plus its presence in the Aleutians, seemed to indicate fairly recent diffusion from America to Asia via the islands. However, I have found evidence that such a mode of entrance was once much more general in eastern Asia, and at quite an early date—indicating that this trait is too old and widespread to have been the subject of any recent transfer across the Aleutians. Furthermore, the similarity of the Aleut and Kamchadal houses has been somewhat exaggerated, conveying an impression of closer relationship than the facts would justify.

Heizer (1943) has made quite a convincing case for the transfer of aconite poison to the New World in fairly recent times as an integral part of a lance whaling complex. I would tend to accept this particular complex, at the same time questioning whether the mere use of aconite may not be an older element in the North Pacific than has been realized. Such use is not nearly as restricted as Heizer indicated. There is now evidence that aconite poison was known to the Chukchi and Asiatic Eskimo. Among the Kamchadal it was certainly not confined to a whaling complex, since they did not hunt whales; and it was used for all purposes by the Ainu and the Kuriles. There are also references to the use of aconite poison on arrows by the Aleut and Koniag, suggesting that even here it was not solely restricted to a secret whaling cult. The Thompson Indians also used a plant closely related to aconite for arrow poison; and three other Plateau groups employed vegetable poisons of possibly similar type.

The practice of mummifying the dead has been suggested as another element which diffused via the Aleutians. But Laughlin and Marsh now state (1951, p. 82), on the basis of their recent work, that among the Aleut this practice could not have preceded the Russian advent by very long, and never spread as far as the western Aleutians.

Most famous of the split distributions—elements not actually occurring in the Aleutians but inferred to have passed through them owing to their alleged absence at Bering Strait—is the cycle of Raven tales. Jochelson's study of Koryak folklore (1908, pp. 358–362) demonstrated that 84% of the motifs in Koryak tales are shared with the Northwest Coast Indians, and only 20% with the Old World. He found a 24% correlation with Eskimo, but this apparently was Central or Eastern Eskimo. He concluded that there was virtual identity between the Indian folklore and that of the Koryak and Kamchadal; and that this could only be explained by a close relationship in fairly recent times—interrupted, of course, by the Eskimo wedge at Bering Strait.

It should be pointed out, however, that Jochelson deliberately excluded Alaskan Eskimo folklore from his study, on the assumption that since these Eskimo were recent intruders, they did not figure in the level at which he was operating, and would only confuse the issue if included. Now, a recent study of the Raven tales by Chowning (Master's Thesis, Univ. of Penna.) has revealed that the Alaskan Eskimo tales form a perfect link between those of the Northwest Coast and Athabaskans on the one hand, and the Siberians on the other. So the "split distribution" turns out to be not split at all, but continuous via Bering Strait, and a major prop of the Aleutian theory dissolves.

I wonder if we have not allowed the ghost of the "Eskimo wedge" to dominate a lot of our thinking for too long a time. We have been assuming that culture traits could not pass from Asia to America through this culturally "hostile" intermediary. But may not the spectacular local efflorescence at Bering Strait have tended to obscure an originally basic kinship between the cultures on either side? I suggest that the barrier to diffusion here may be largely imaginary; that there was, on the contrary, a continuum of culture around the rim of the North Pacific—more marked in earlier periods—of which the Eskimo of Alaska formed an integral part. If there was thus no real blockade at Bering Strait, what need is there to concoct a less plausible Aleutian link to explain the major resemblances on both sides?

Let us see what further evidence there may be for such a former continuum.

My analysis of Kamchadal culture brought out its close ties with the immediate hinterland, as would be expected from the linguistic picture. But it also revealed a very considerable body of culture shared with the Eskimo. Most of this is common to Chukchi and Koryak as well, but I was surprised to find a number of very distinctive Eskimo traits that seem absent in one or both of these intervening groups—e.g., the very elaborate system of tabus, and specifically the prohibition on cooking land and sea animals in the same pot; expiation of tabu violations by confession; divination by lifting a part of the body. This of course raised the question of a former closer contact between the Kamchadal and the Eskimo, of such nature as might account for these parallels in intellectual culture. It suggested the possibility that an Eskimo population might once have been more widely spread along the western shores of Bering Sea—not the elaborate forms of Eskimo culture which flourished under the peculiar conditions at Bering Strait, but a more basic, generalized variety.

As you go northward from Kamchatka, the Eskimo element in the Siberian cultures increases sharply. And this phenomenon has been recognized by other students as well. We find Jochelson admitting that direct intercourse must have formerly existed between the Koryak and the Eskimo to account for the cultural situation; that too much is involved to be explained merely by diffusion via the Chukchi (1908, p. 359). Collins has also commented on the cultural resemblances of the Maritime Koryak and Chukchi to the Eskimo, and has expressed the opinion that these seem too fundamental and deep-seated to have been acquired through recent contacts (1940, p. 541). Birket-Smith sees Eskimo influence visible along the Asiatic coast, "gradually decreasing . . . from the Chukchi in the north to the Gilyak in the south" (1951, p. 150).

This picture of a cultural continuum shared by the Palaeo-Asiatics and Bering Sea Eskimos has linguistic and racial analogues as well.

No one any longer believes that the Palaeo-Asiatic languages have anything in common with those of the Northwest Coast. But there is evidence of at least long association between Palaeo-Asiatics and Eskimo. Thalbitzer, for instance, found indications of a former fairly intimate contact between Koryak and Eskimo, now separated (1941, p. 576). Boas (1933, p. 369) saw a common psychological structure shared by the Eskimoan and Palaeo-Asiatic language groups—a similarity in the mode of analyzing experience not found elsewhere, again suggesting prior association of all three Siberian languages with an Eskimo-speaking population.

Similarly, the former belief in the "Americanoid" physical type of the Palaeo-Asiatics has been shattered by recent Soviet field work in northeastern Siberia (summarized in Chard, 1951, 1954). The resultant studies indicate that the closest racial affinities of the modern Palaeo-Asiatics are with the Bering Sea Eskimo and Aleut rather than with Indians or other Siberians. Eskimo features, furthermore, are most marked among the coastal populations—the Maritime Koryak and Chukchi.

I suggest that we can best explain this entire picture—cultural, linguistic, and racial—by postulating the former existence of a coastal population of Eskimo all the way from Kamchatka to Bering Strait, which was later submerged by the inland Palaeo-Asiatic tribes.

Drucker (1955) has independently arrived at a similar concept of an Eskimo substratum for Northwest Coast culture; and I now propose that we visualize an ancient arc of related culture and population around the entire rim of the North Pacific from Kamchatka to Puget Sound, along which individual traits may have readily diffused in either direction. I would conceive of this arc as a belt of sedentary maritime peoples sharing a common cultural tradition, a rather comparable environment, and leading a roughly similar type of life based on fishing and varying degrees of sea-mammal hunting—perhaps something on the order of De Laguna's earliest stage at Cook Inlet. The local manifestations of culture within this arc would naturally be strongly colored by the particular ecology: e.g., the abundant cyclical fish resources and richer flora in the southern sectors, or the walrus and arctic conditions at the center on Bering Sea. The Old World half of this arc would have linked up with the ancient northeast Asiatic maritime world, from which may have come, about 2000 years ago, the stimuli that gave birth to the brilliant cultural efflorescence at Bering Strait—in combination with the rich natural wealth of that area. From such a cultural hearth at the Strait, influences doubtless subsequently spread out in both directions down the North Pacific rim, but so specialized was this Bering Sea culture in relation to its peculiar ecological base that these influences seem to have had little strength beyond the limits of that restricted region. Hence we see no trace of the effects of these post-A.D. Bering Sea cultures at either end of the arc—in Kamchatka or the Northwest Coast—whereas older and more generalized traits seem to have diffused readily.

Drucker would see the Asiatic parallels on the Northwest Coast as later transferals via Eskimo or Aleut—not as referable to his early Eskimoid stage of Northwest Coast culture. I differ in suggesting that they were part of the common heritage of this early North Pacific continuum which emerges from our several hypotheses. The fact that the historic Northwest Coast Indians and Palaeo-Asiatics share certain elements is explained, I believe, by their respective invasion of the southern ends of this continuum and their adoption of these elements from this common source. There was no direct contact or diffusion between the Indians and the Siberians—only participation in the common tradition of their Eskimoid predecessors on the shores of the North Pacific. Even parallels between Aleut and Kamchadal can largely be explained by this same common substratum without the necessity of extensive direct contacts.

I believe that this hypothesis has the advantage of accounting for the situation on both sides of the Pacific—which any satisfactory explanation must be prepared to do. The day is past when New World culture history can be viewed in a vacuum.

At any rate, I offer this proposed solution to the problems raised by the Jesup Expedition as a working hypothesis. If substantiated to any extent, it would, I believe, strengthen Drucker's theory of Northwest Coast culture history by furnishing a complementary situation; and it would also lend more substance to Okladnikov's view (see Chard, 1955) of Bering Sea Eskimo culture as the terminal link in a chain of east Asiatic maritime cultures—rather than a transplant from interior Siberia or central Canada.

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TECHNIQUES OF PREPARING MANIOC FLOUR AS A KEY TO CULTURE HISTORY IN TROPICAL AMERICA

Gertrude E. Dole

Manioc has been used as a staple food among the aborigines in approximately four-fifths of the area of South America, as well as in the Antilles and parts of Middle America. It is in fact the basis of much of the Tropical Forest type of culture. Tubers of the manioc plant may be prepared for use in several ways. They are most frequently used in the form of flour, the preparation of which requires that juice be removed from the grated pulp. A complex device was developed in aboriginal America to perform this function. This is the *tipiti*, or sleeve press, which is used in much of the area in which manioc is found. The invention of the *tipiti* has been regarded as somewhat of a mystery. Because of the complexity and wide distribution of the *tipiti*, a study of its development and distribution may throw light on the place of origin and spread of manioc cultivation.

It has been assumed by some ethnologists that the *tipiti* was the only native device for preparing manioc flour. Actually several other devices were used aboriginally in about half the area of distribution of cultivated manioc. I shall trace the evolution of the *tipiti* in reverse, in order to point out the antecedent form for each step. I have not attempted here to make an exhaustive survey of South American cultures but only to establish provisionally the distribution of various techniques for preparing manioc flour and the linguistic stocks and culture types with which they are associated.

The manioc sleeve press is one of the most ingenious devices in the material culture of the tropical forest. It consists of a long cylindrical container which is open at one end. The container is constructed by diagonal weaving in such a way as to permit it to be stretched. By stretching the cylinder lengthwise its diameter is reduced, thus exerting pressure on the contents. When the press is filled with wet manioc pulp and stretched, the juice is forced out, leaving the pulp in the form of a coarse flour. To facilitate this process the *tipiti* is equipped with a loop at either end. The loop at the open end may be attached to a house beam and the lower loop weighted with a stone or slipped over another horizontal pole. The lower pole is then used as a lever, giving added mechanical advantage to the pressure exerted on it.

Accounting for the origin and development of the *tipiti* will be simplified if we recognize at the outset that this is only a device for dehydrating the pulp and not a mechanism for eliminating the poisonous hydrocyanic or prussic acid which forms in bitter manioc. That its purpose is not the removal of poison is indicated by the fact that identical methods are used for treating both sweet and bitter manioc. Furthermore, leaching the pulp is neither necessary nor adequate for removing the poison. Prussic acid is very volatile and may be

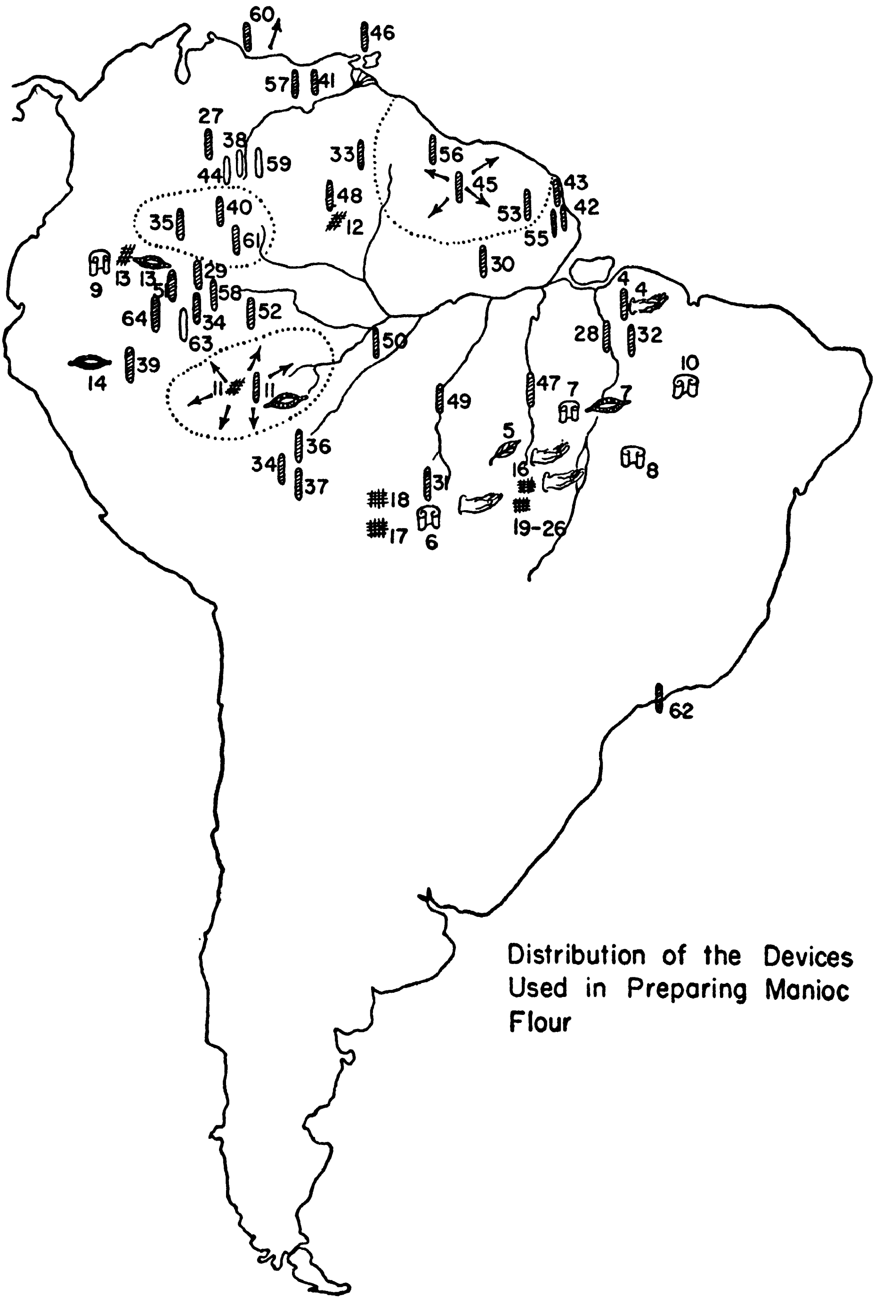
Distribution of the Devices Used in Preparing Manioc Flour
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Fig. 1. Map of the Distribution of Devices for Preparing Manioc Flour

KEY TO MAP OF THE DISTRIBUTION OF DEVICES USED IN PREPARING MANIOC FLOUR

Tribes	*Linguistic stocks*
Hands only: ☞	
1. Cajabí	Tupí
2. Carajá	Carajá
3. Tapirapé	Tupí
4. Tenetehara (with *tipití*)	Tupí
Leaves: /	
5. Chukahamay	Gé
Bark: ●	
6. Nambicuara	Nambicuara
7. Northern Cayapó	Gé
8. Sherente	Gé
9. Sioní (wild manioc)	Tucano
10. Timbira	Gé
Woven mat: ▲	
11. Juruá-Purus tribes	Arawak, Catukina, Pano
12. Shirianá	Shirianá
13. Witoto	Witoto
Open sack: ◇	
14. Cahuapanan tribes	Cahuapana
7. Cayapó	Gé
13. Witoto	Witoto
15. Yamamadí	Arauá
Twined mat: ●	
16. Auetí	Tupí
17. Bauré (fixed in frame)	Arawak
18. Huanyam	Chapacura
19. Kalapalu	Carib
20. Kamayurá	Tupí
21. Kuikuru	Carib
22. Matipú	Carib
23. Mehinaku	Arawak
24. Trumaí	Trumaí
25. Waurá	Arawak
26. Yawalapití	Arawak

Tribes	*Linguistic stocks*
Tipití: |	
27. Achagua	Arawak
28. Amanayé	Tupí
29. Andoke	Andoke
30. Apalai	Carib
31. Apiaká	Tupí
32. Ararendeurara	Tupí
33. Arekuna	Carib
34. Bora	Bora
35. Carijona	Carib
36. Caipuná	Pano
37. Chacobo (sweet manioc only)	Pano
38. Chiricoa (no manioc)	Guahibo
39. Cocamilla	Tupí
40. Cubeo	Tucano
41. Cumanangoto	Carib
42. Emerillon	Tupí
43. Garipon	Garipon
44. Guahibo (no manioc)	Guahibo
45. Guiana area	Arawak, Carib, Tupí
46. Island Carib	Carib
11. Juruá-Purus tribes	Arawak, Catukina, Pano
47. Juruna	Juruna
48. Makú	Makú
49. Mundurucú	Tupí
50. Mura	Mura
51. Okaina	Witoto
52. Omagua	Tupí
53. Oyampi	Tupí
54. Pacaquará (sweet manioc only)	Pano
55. Palikur	Arawak
56. Patamona	Carib
57. Piritú	Carib
58. Resigero	Resigero
59. Saliva	Saliva
60. Taino (also cotton sack)	Arawak
4. Tenetehara	Tupí
61. Tucano	Tucano
62. Tupinamba (some tribes)	Tupí
63. Yagua (sweet manioc only)	Yagua
64. Yameo (sweet manioc only)	Yameo

removed in any one of several simple ways. Some tribes drive it off by heat, others by exposure to the sun or fermentation. One or more of these methods is practiced by all peoples who use bitter manioc for food.

In its most evolved form the *tipiti* occurs as a woven cotton sack among the Arawak-speaking Taino of the Greater Antilles (Rouse, 1948, p. 523). The precolumbian Taino had of course attained a higher culture level than any of the tribes of the tropical forest. It is known, however, that these people reached the Antilles from the South American mainland, and it seems probable that they took with them a knowledge of the manioc sleeve.

The *tipiti* is most frequently found as a basketry cylinder. This form is used exclusively in the Guianas and on the lower Orinoco, where speakers of Arawak and Carib languages predominate. It is found also among Arawaks and Caribs in the Antilles. Tupian peoples both north and south of Marajó island have the basketry press, but according to Métraux the *tipiti* was not widespread among the Tupinamba of the eastern coast of Brazil when they were first visited by French explorers (Métraux, 1928, p. 104). It is found among many peoples of the Arawak, Carib, Tupian, and several smaller language families on the middle and upper Amazon and Orinoco rivers. Kirchhoff notes with reference to the food-gathering Guahibo and neighboring Chiricoa that "the older sources mention tubes 6 feet (2 m.) long made of flexible cane strips. The latter seem to have been similar to the *tipiti* of horticultural tribes, but the Guahibo and Chiricoa used them to extract oil from the cunama palm fruits . . ." (Kirchhoff, pp. 451–452). Both the Guahibo and Chiricoa inhabit a lowland region on the middle Orinoco, where they are surrounded by Arawak peoples who have the manioc and *tipiti* complex. That the highly specialized and efficient manioc sleeve should occur without the cultivation of manioc in the midst of a primary area of manioc cultivation is anomalous. I shall return to this point later.

The immediate antecedent of the manioc sleeve seems to have been a long flexible sack-like container which is open the entire length of one side. Métraux describes such a sack which is used by the Witoto, Yamadidi [Yamamadí?], and Cayapó to remove the juice from manioc pulp by twisting or wringing it. As Métraux points out, the sack "requires a certain amount of muscular force, indicating that this . . . type of apparatus is much inferior to the . . . [*tipiti*]" (Métraux, 1928, pp. 104, 114–115). A sack which was "suspended and stretched like the tipití" is reported also for the Cahuapanan tribes at the headwaters of the Amazon in Peru (Steward and Métraux, 1948b, p. 609). It should be noted in passing that the tribes in which the sack is wrung by hand inhabit the upper reaches of tributaries of the Amazon.

Carrying baskets which are loosely woven of diagonal elements are used widely in the tropical forest. These baskets are long and sometimes cylindrical, as among the Guahibo on the Orinoco river and the Nambicuara of the upper Juruena affluent of the Tapajós river in Mato Grosso. Other cylindrical carrying baskets have an open side, the cargo being bound into the basket when it is in use. The latter are often made of pinnate palm fronds, which the Indians weave diagonally for many purposes. Such baskets may have been prototypes for the open sack.

Additional information about an intermediate form of manioc press among the Witoto comes from Whiffen. Whereas the Bora and some of the other neighboring tribes used the *tipiti*, Whiffen writes ". . . the Witoto use a long web, a rectangular strip about ten inches wide of plaited bark-fibre . . . This they wind around the grated manioc after the manner that puttees are adjusted on the leg. The tighter they twist the pliable web the greater the pressure upon the crushed roots, and the juice is thus wrung out of them" (Whiffen, pp. 98–99). Among the Shirianá of the upper Negro river, juice is pressed from manioc by twisting it in a mat (Métraux, 1948a, p. 863). This is the procedure used among some of the tribes on the Juruá and Purus rivers also (Métraux, 1948b, p. 666). Among the latter the straining mat is oval instead of rectangular.

An entirely different kind of mat is used in two isolated areas among the Trumaí and tribes speaking Arawak, Carib, and Tupian languages in the upper Xingú basin of Mato Grosso,[1] and among the Chapacuran-speaking Huanyam Indians on the Guaporé river (Nordenskiöld, p. 129). This mat is flexible in only one direction, being made by twining together stiff elements such as the midribs of palm leaves. Manioc pulp may be squeezed in the mat by wringing it or by pressing the pulp against the mat as it lies flat on wooden slats over a container. A variation of this type of mat occurs among the Arawak Bauré near the Guaporé (Nordenskiöld, p. 129).

The completely flexible mat of the western Amazon appears to have evolved, not from the partly rigid twined mat of the southern tributaries, but from an extremely primitive device which is used among various peoples on the peripheries of the tropical forest whose subsistence is based on both collecting and horticulture. These peoples are the Gé-speaking Timbira, Sherente, and Northern Cayapó of the savannas in eastern Brazil, the linguistically isolated Nambicuara at the headwaters of the Tapajós river, and the Tucanoan Sioní at the headwaters of the Putumayo river on the Ecuador-Peruvian border. They merely wrap a strip of bast around the manioc pulp to secure it in place while it is squeezed or wrung by hand (Lowie, 1946, p. 481; Lévi-Strauss, p. 363; Steward, 1948, p. 741). Only slightly more primitive than the use of unwoven bast is the use of large leaves as a sheath for the wet pulp. This occurs among one Northern Cayapó tribe, the Chukahamay on the middle Xingú river.[2]

The only difference between wringing the pulp in strips of bast and using a "web" or mat is that the latter are woven of the material used in the simpler technique. Now, throughout the entire area in which manioc is used, there are round or rectangular trays or sieves which are loosely woven of splints or palm fronds. These are used by primitive peoples of eastern Brazil to sift pulverized seeds of wild plants (Nimuendajú, p. 73). Hence this type of utensil seems to have appeared very early in the history of Tropical Forest culture. Among some of the fully horticultural peoples, the same type of utensil is used both to sift flour and to strain wet manioc pulp. It is a short step to apply the weaving technique to the strips of bast and produce a flexible webbing in which the pulp may be wrung. The oval mat used in this way among the tribes of the Juruá and Purus rivers appears to have resulted from the adaptation of the flexible elements to the pattern of the round sieve-trays. The long rectangular mats used elsewhere were adaptations of the rectangular sieve-trays.

One more method completes the sequence of steps in the development of the *tipiti*. This is the squeezing of pulp by hand without any sheath, a method which is used by the Carajá of the Araguaya river (Lipkind, p. 182) and the Tupian Cajabí,[3] Tapirapé (Wagley and Galrão, 1948, p. 169), and Tenetehara (Wagley and Galrão, 1949, p. 40), the last of whom also use the *tipiti*.

The various devices for preparing manioc are not clearly correlated with particular language families. It seems to me to be significant, however, that most of the Arawak and Carib tribes have the *tipiti*, and that no member of these two language families uses the most primitive devices. Very primitive devices are found, on the other hand, among some Tupians and peoples of the Gé, Tucanoan, and isolated linguistic stocks.

Representatives of the various language families are themselves widely scattered in the tropical forest. Let us look at the correlation of methods of manioc preparation with geographical regions and culture types. The most evolved device, the cotton sleeve, was found only among the Arawak Taino, who also had the most advanced culture of all the peoples mentioned here. The usual type of *tipiti* is widely distributed except in the southern and eastern part of the area of Tropical Forest culture. It is almost entirely restricted, however, to lowland areas of true rain forest and to tribes which have well developed horticulture.

The distribution of the *tipiti* is roughly complementary to that of more primitive devices which require the use of hands in wringing the pulp. These latter devices are notably absent from the entire Orinoco basin, but are found at the headwaters of the Amazon and for the most part in the savannas rather than in deeply forested areas. Furthermore they occur principally among peoples of the Gé and isolated linguistic stocks, whose horticulture is rudimentary and among whom hunting forms a significant part of the subsistence base. These incipient horticulturalists have been too hastily bracketed with the Marginal hunters. It would be more consistent with the traditional use of the term "marginal" to regard these peoples as representing a primitive stage of the Tropical Forest culture type.

Some fully horticultural peoples who lack the *tipiti* inhabit peripheral regions where they are surrounded by peoples of incipient horticulture. Such are the Arawak, Carib, and Tupian peoples of the upper Xingú region. Theirs may be regarded as an intermediate form of the Tropical Forest culture.

From this survey of the techniques of preparing manioc flour, the following inferences may be drawn: (1) The Manioc and *tipiti* complex appears to have developed in the Orinoco basin and the lowland region of the middle and lower Amazon. (2) It was taken from there into the Antilles by the Arawaks and Caribs, and down the eastern coast of Brazil by speakers of Tupí languages. (3) It has been spread toward the headwaters of the Amazon by speakers of Arawak, Carib, Tupian, Tucanoan, and Panoan, as well as a few less widely represented language stocks. Here it is still in the process of displacing intermediate and primitive subtypes of the Tropical Forest culture. (4) The absence of the *tipiti* in these outlying areas among peoples who raise manioc suggests that the development of manioc cultivation was accompanied by a movement of peoples upstream, and that many of them reached the headwaters of the Amazon before having an opportunity to participate in the full-fledged Tropical Forest culture. (5) The *tipiti* is thus seen as a relatively recent invention. (Cf. Métraux, 1928, p. 104.)

The distribution of the manioc sleeve raises a question for further investigation. As I have pointed out, there is a close positive correlation of manioc horticulture with the use of the *tipiti* in lowland South America. In view of this correlation it does not seem unlikely that the rare occurrences of the use of the *tipiti* without manioc may represent a loss of the custom of cultivating manioc. Such instances are the use of the *tipiti* for processing wild food products among the Guahibo and Chiricoa of the Orinoco valley. This is an area in which distributional evidence indicates that the cultivation of manioc reached a high development. We know that the Shocleng of southern Brazil abandoned horticulture when they were driven from their precolumbian homeland (Henry, p. 3). The Mura of the middle Amazon and the Guayaquí of Paraguay ceased to raise crops and Guajajara horticulture diminished after the Conquest (Lowie, 1948, p. 5; Nimuendajú, p. 61). These instances of the loss of a basic culture trait suggest that a review of the cultures of contemporary food-gathering peoples such as the Yaruro, the Guahibo, and Chiricoa along the Orinoco might reveal evidence of deculturation. It may be that these "Marginal" cultures are not fundamentally primitive but have undergone secondary simplification as a result of oppression by advanced horticulturalists or because of depletion of soil fertility or both.

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Notes

1. Field notes. Grateful acknowledgement is made to the Henry L. and Grace Doherty Charitable Foundation, Inc., for support of field work among the upper Xingú tribes in 1953–1954.
2. Field notes.
3. Field notes.

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CULTURE STABILITY AND CHANGE AMONG THE SEMINOLES OF FLORIDA

Ethel Cutler Freeman

The factors and processes that make for stability or change within cultures are of increasing concern to nations as well as to anthropologists and sociologists as our Western culture encroaches ever more heavily upon the domain of the primitive peoples of the world. It is to the advantage of any nation to divine the ways by which minorities adjust their customs, beliefs and ideologies to accommodate to the dominant civilization. Lessons learned in the study of one group may be applied to minimize tensions and conflicts among other groups.

The Seminole Indians of Florida qualify admirably as subjects for such a study. This isolated and encysted society has been forced suddenly into economic and political contact with an abhorred dominant civilization.

I have known these Indians since 1928, which was the beginning of their first continuous contact with the white man. I have lived among them in the heart of the Everglades for part of every winter for the last eighteen years. My personal observations of Seminole society in flux may shed light on the problems of these and other primitive people.

This paper will sketch the major mechanisms by which our impinging white civilization has contributed to disequilibrium of Seminole culture by interfering with the functioning of that minority's institutions. Special emphasis will be placed upon the role of dynamic individuals as influences on both cultural stability and change. It is the thesis of this paper that prerequisites of culture change lie within the culture itself and that when the culture is out of equilibrium a powerful personality, usually frustrated, acts as a direct stimulus. By that I do not mean that there are not other stimuli as well.

A longer paper, with detailed ethnographical data, will be available to interested persons.

Today 918 Seminoles are the only Indians in Florida. They belong to the Southeast Woodland culture group, with customs and social organization resembling that of the Creeks, Choctaws, Cherokees and Yuchis. Social structure follows the Crow pattern with functioning clans.

Originally the Seminoles belonged to the Mickasuki and Muscogee tribes of the Creek Confederacy of Alabama and Georgia. Hard-pressed by European colonizers in the later 17th and early 18th centuries, they migrated into Florida and were joined by remnants of other tribes.

During the Seminole Wars of the last century the United States Army killed, or captured and sent west, most of the estimated 11,000 Seminoles that had roamed over Florida. The Seminoles of today are the descendants of about 200 who escaped and fled into the Everglades.

In 1775 these two tribes were recognized as one people, "The Seminoles of Florida," and the United States has dealt with them as a political unit ever since in spite of their protests. The Muscogees, now called Cow-Creeks, and the Mickasukis, known today as the Big Cypress Seminoles, are traditionally antagonistic bands. Their basic culture is similar but their languages are not mutually understandable. Their ideologies have been antipathetic and trouble has arisen on face-to-face contact throughout the years. The first portent of possible future unity occurred three years ago when they joined political forces to sue the Federal Government.

The Seminoles live on three Federal Reservations in south Florida and also as squatters on white-owned land.

The Big Cypress account for two-thirds of the Seminoles, the Cow-Creeks for one-third. As the pressures from the dominant society increased, the two historically separate bands began to divide and realign themselves.

About 1937, the Big Cypress split into two groups. One group lives on the isolated Big Cypress Reservation in the Everglades. Another group camped along the Tamiami Trail, that highway built across the Everglades in 1928. Surprisingly, the isolated Big Cypress have become the most progressive Seminoles. The "Trail Big Cypress," who live close to Miami and civilization, have repudiated their "Reservation" brethren and formed a nativistic band.

Recently three schisms have developed among these "Trail Big Cypress." A pluralistic faction has broken away from the ultra-conservatives, while a small band of assimilationists has been ejected from the nativistic band.

Cow Creeks, about 200 strong, live on and around the Brighton Reservation northwest of Lake Okeechobee near white towns. Other Seminoles of both bands live on the small Dania Reservation on the east coast near Miami.

Untrammeled independence and the wish to be a unique people apart is the key-note of Big Cypress ideology. The Cow-Creeks have no such definite goals. They do not reject the outsider. Some of the women have children by white or Negro fathers. In the War of 1812 they joined the Creeks and Americans against the other Seminoles.

This variation in ethos explains to a great extent the friction that has existed between the two bands. Their stereotypes of one another reflect their differing ideologies. The Cow-Creeks think of the Big Cypress as "disdainful egoists," while the Big Cypress consider the Cow-Creeks to be "lazy appeasers."

An analysis of Seminole history falls into four periods: a long period of comparative stability melted into one of subtle economic changes. This was followed by a hectic decade in which dynamic individuals were the direct stimulus for profound changes that culminated, during the last period, in a reorganization of Seminole society to meet today's challenges.

PERIOD 1—FROM 1700 TO 1928

In spite of migrations and wars Seminole institutions continued to function, so this era was one of comparative stability for both bands. Isolation and an enduring subsistence pattern were the principal stabilizing factors. Among the Big Cypress, non-conformists and dynamic personalities advocating change were killed, and laws against miscegenation were strictly enforced. The Cow-Creeks were more lenient, and an admixture of Negro and white is evident today.

PERIOD 2—FROM 1928 TO 1940

Slow economic change in a restricted area developed cultural voids and created unrest. The Seminoles could no longer live off the country and were forced into a money economy. They developed a handicrafts industry and marketed their produce to tourists on the Tamiami Trail. A growing number of "Trail Big Cypress" found they could preserve their independence with income derived from this source.

The rapid development of south Florida by the white man gave the Big Cypress Seminoles a great fear of the encroaching "foreigners." They refused white education offered by the government and resented its acceptance by the Cow-Creeks. Soon after, however, they gained assurance through the introduction by the government of a successful cattle program which began to educate the Seminoles in the ways of the white business world.

The moral disintegration of Tiger Tigertail, the most powerful Big Cypress medicine man, was a disturbing factor. As leader of the Big Cypress Green Corn Dance, their major social control, and as keeper of the Sacred Medicine Bundle, Tiger's character was of utmost importance. He was often drunk and violent and committed crimes. Tiger finally was ostracized and threatened with death by the Big Cypress Trail Seminoles and went to live at the Big Cypress Reservation. All the Indians believed that Tiger had originally influenced the supernatural to protect them. Now that he was evil they suffered from a terrifying sense of insecurity. Eventually the Big Cypress realized that their mounting feelings of frustration were due to the undermining of their beliefs and way of life.

Medicine, magic and religion did not play as important a part in Cow-Creek life. Their medicine-man practiced as usual, but they made no strong attempts to preserve their way of life. Intermarriage with diversified outgroups may have been a factor that impeded a unified objective.

PERIOD 3—FROM 1940 TO 1950

Voids in Seminole culture set the stage for change in the foregoing period. Changing economy and ecology in which dynamic individuals acted as direct stimuli to both stability and change mark this decade.

Tiger Tigertail led the Reservation Big Cypress to accept Christianity. His younger brother, John, started a nativistic movement among the Trail Big Cypress in an attempt to preserve Seminole culture.

White civilization directly affected Seminole life. The Indians' fear of deportation became acute when they realized that their reservation lands were coveted for crops and mineral rights.

World War II created new economic conditions. Gas rationing cut off the tourist trade. But farmers welcomed Seminoles when white laborers went to war. The Indians dictated the conditions under which they would work. Kinfolk groups picked and planted crops for white ranchers. Isolated from foreign contact and working under their Head-men, they followed their clan system of food gathering of the early days. The Big Cypress Trail Seminoles developed a wholesale frogging industry patterned on the same cultural tradition.

Christianity was introduced on the Big Cypress and Cow-Creek Reservations but the Trail Big Cypress headed by their Council and Medicine-men would have none of it. In 1937 many of the Trail Big Cypress had gone to the Big Cypress Reservation because they did not belong to the clans which could inherit official positions or status. Status could no longer be gained by war and hunting prowess. The acceptance of Christianity gave these Seminole prestige. Church positions became goals for the ambitious.

One Sunday when the heads of the Oklahoma Southern Baptist Church were at the Big Cypress Reservation Tiger Tigertail suddenly asked to be baptized. He directly stimulated the acceptance of Christianity for 22 of his people and their families followed his lead. Thus Tiger again had spiritual and political authority. Tiger was a reformed man and doubtless unconscious of any ulterior motive in his conversion.

Christianity as interpreted by the Southern Baptists caused a speedy change in social and political organization. The Green Corn Dance, the legal, social and religious ceremony that ushers in both the symbolic and calendar New Year, was made taboo to converts. Church members were told to ignore the Seminole Council and to accept Church dictates instead.

This led to a definite split between the Reservation Big Cypress controlled by the Church and the Trail Big Cypress ruled by their old government.

When Tiger Tigertail was ostracized from the Trail group the sacred Medicine Bundle and leadership of the Green Corn Dance was given to John Tigertail. John also was a purposeful and strong character. He determined to preserve the old religion and customs and so precipitated an active nativistic movement.

The Cow-Creeks took no part in the altercation among the Big Cypress. It was not until the next period that some of the Cow-Creeks casually decided to accept Christianity. Others held to their old beliefs, but this difference in religion did not creat a ripple of disturbance among them.

PERIOD 4—FROM 1950 TO THE PRESENT

While the third period was essentially one of economic, social and religious conflict sparked by two powerful personalities, the fourth period is one of political change and reorganization.

A large monetary settlement made to the Ute Indians for lands taken from them by the United States Government inspired the Reservation Big Cypress and Cow-Creeks to join forces at last, as the "Reservation Seminoles," and claim $50 million for their lost land. In contrast, the Trail Big Cypress announced they would accept no money if it were awarded them. They felt that an award from the Federal Government would buy them, and they would no longer be a "sovereign nation." The claim has not yet been settled.

A bill popularly known as "The Termination Bill" was responsible for a further development in Seminole political organization. This bill included the illiterate, non-English-speaking Seminoles of Florida among the eleven tribes that were said to be so acculturated that their reservations were to be taxed and all government health, welfare, and educational services ended. The Seminole Bill was opposed so strongly by Indian welfare organizations that it never came to a vote. However, the realization that they could lose their reservations through lack of income to pay taxes frightened the Seminoles into more effective unity. They organized a twelve-member committee from the three reservations to represent them in dealings with the United States Goverment. The Reservation Seminoles and the Trail Big Cypress became even more estranged when the latter refused membership on the committee or any dealings with "Washington."

In accordance with the United States Government's recent policy of Indian assimilation and the eventual abolishment of the Indian Office, the enforcement of law and order on reservations has been transferred from Federal to State jurisdiction. Under Federal control Seminole law prevailed, even in criminal cases between Indians. The State has interfered with the functioning of Seminole social and clan organization. Recently Florida authorities took from her kin group, to whom Seminole custom gives them, the children of a woman who died, and awarded them to the father as their legal guardian. This may have far-reaching consequences. Now that children are economic liabilities instead of assets, this break in tribal law may bring about a situation where neither clan group feels responsible.

Education has been partially transferred from Federal to State agencies. The Indian school on the Brighton Reservation is closed. The children are taken from their homes early in the morning, transported fifty miles to a public school and returned at bed-time. The Seminole woman's responsibility for the continuity of the culture cycle and the molding of Seminole personality is being taken from her.

Many old customs still carry on in all groups. Most Seminoles still live in open-sided huts much as their ancestors did. The amassing of material goods by an individual is still frowned upon. Ideally, no man or clan has more prestige than another. Land is communally owned.

Patterned story-telling has perpetuated an active fear of the white man. This mechanism tends to maintain old customs, laws, religion, and an isolationist point of view. Day labor and the dispersal of people over a larger area has contributed to the disappearance of patterned story-telling among the Cow-Creek and Big Cypress Reservation Indians. But the old traditions are kept alive in this way among the Trail Big Cypress.

We have seen how the conciliatory Cow-Creeks and a portion of the Big Cypress have united under the pressures of an impinging society as progressive "Reservation Seminoles."

It is illuminating that the three-way split that took place among the Trail Big Cypress three years ago now portends a greater unity. The "Big Cypress Moderates," who could not go along with the fanatical nativistic program of the faction still known as the "Trail Big Cypress Seminoles," show the greatest promise of realigning themselves with their reservation kinfolk. Both have accepted the concept of compromise as a means of keeping institutions functioning.

The "Reservation Seminoles," the "Trail Moderates," and the nativistic "Trail Big Cypress," all are trying to preserve their culture. This common goal, however varied in degree, may serve to unify them. Only the few "Big Cypress Assimilationists," led by a frustrated young Seminole, want a fundamental cultural change. The passing of John and Tiger Tigertail would make reconciliation easier between the Big Cypress groups.

These four periods of Seminole history demonstrate some basic facts. As in many peripheral societies, isolation is one of the reasons for cultural stability. An impinging civilization creates voids and needs within a minority. Adaptations and compromises may be more effective in preserving a culture than unyielding resistance. Also, when the time is ripe, dynamic individuals are the direct stimuli for change.

A continued study of culture stability and change among the Seminoles will give further clues to the laws of institutional and ideological change.

Note: Since all cultures are in a state of change, rapid or slow, a stable culture here refers to one in equilibrium, one that exhibits over a period of time and throughout the culture area a continuity of pattern which shows little deviation from the earlier pattern in content or tempo. In other words, a terse definition of a stable culture would be one that changes slowly in time, space, extent, and speed.

Also the term "dynamic individual" is used to express an effective person, one who produces results, a purposeful leader.

Names are typically Seminole but do not refer to any actual or living persons.

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THE EVOLUTION OF STATUS SYSTEMS IN POLYNESIA

Irving Goldman

In a previous paper[1] I presented the theory that rivalry for social status had given impetus to and had set the course for the main lines of cultural evolution in Polynesia. The thesis, briefly, was as follows: All Polynesian societies can be grouped into one of three systems regulating social status. I labelled these "Traditional," "Open," and "Stratified." The *traditional* (Ontong Java, Tokelau, Tikopia, Pukapuka, Manihiki-Rakahanga, Maori, Tongareva, Futuna, Uvea, Manua) stressed seniority of descent as the primary criterion of status—a few were moving from age to descent seniority. Where age was the criterion, social distinctions tended to be minor; and where seniority ruled, status distinctions were both finely graded and more important. In either case, the rewards of high status were mainly prestige and privilege but very little power. Status mobility was low. The *open* (Niue, Easter, Mangaia, Marquesas) stressed military and political prowess. Here, status differences were more sharply defined, and the rewards of status included power along with prestige and privilege. Status loomed larger on the social scene and status mobility was very high. The *stratified* (Tonga, Mangareva, Tahiti, Hawaii) emphasized clear-cut distinctions between landed and landless; social status was of major importance and its rewards, as in the *open*, were power together with privilege and prestige. Status mobility was high among the landed and low among the landless. All three status systems shared a common respect for aristocratic lineages. The aristocracy, primary in the *traditional*, was pushed into the background in the *open* to reappear with new powers and privileges in the *stratified*.

Comparative analysis of the three status systems led to the conclusion that they were variants of a common type, represented by the *traditional* societies—that is, the *open* had developed out of tendencies inherent in the *traditional*, and the *stratified* had developed out of tendencies in the *open*. In short, an evolutionary sequence, *traditional* to *open* to *stratified*, was indicated. Evidence supporting this conclusion was:

1. The overlapping or intergrading of the three status systems.
2. The approximate overlapping and intergrading of each of eighteen main Polynesian societies along a scale that may be called "politicalization of status," indicating that the main line of status evolution led from rank and prestige to class and power.
3. The presence of all three status types in each Polynesian subarea, indicating a developmental tendency independent of locally diffused culture traits.
4. The testimony of Polynesian genealogical traditions. By and large, these attested to an "open" phase in the *stratified* societies and to a "traditional" phase in the *open* societies. On chronology, the traditions point generally to the fourteenth and fifteenth centuries as the eras of major social changes.

Most important was the finding that the three status systems correlated surprisingly well with significant differences in virtually every other realm of culture. This finding gave confidence to the conclusion that it was changes in status systems that had brought in their wake cultural transformations in kinship, economy, social organization, government, religion, warfare, morality, and ethos. These changes were adaptations to new criteria for status. Status rivalry then, inherent in Polynesian definitions of status, was virtually a self-propelling system, but not, needless to say, an exclusive determinant of change.

In the present paper, I hope to show some of the specific ways in which status rivalry interacted with other determinants of cultural change, and to call attention at the same time to some general implications for evolutionary theory of the Polynesian data.

The first general implication is for comparative method, an integral aspect of evolutionary theory. Here, the suggestion is that it is in culture areas rather than among selected individual societies that we find the most suitable units for comparison. In the culture area we deal directly and concretely with variations among historically-linked lines. Such variations leading to the appearance of new forms are the central concern of evolutionary theory. As the Polynesian study has shown, variations can be classified into types, the types can be correlated with other cultural features, and comparative analysis, supported by archeology and by traditions where available, can reveal direction of change. Moreover, the culture area safeguards the vital principle of historical relativism in cultural comparisons.

The use of the culture area as the unit of comparison will require new comparative methods in order to give more general scope to otherwise limited historical theories. With respect to Polynesia, two kinds of extended comparative study suggest themselves. The first and most obvious is to relate the Polynesian findings to the Malayo-Polynesian area as a whole. The linking of Polynesia with Micronesia, Melanesia, and Malaysia would span wide cultural variations among horticultural peoples. The second, going outside the area, would compare processes with those in other culture areas sharing similar institutions. Along these lines, a promising approach is to compare Polynesia with early urban societies in the Middle East, the Mediterranean, and the New World, as well as with primitive states in Africa. In these areas, as in Polynesia, we observe the presence of hereditary aristocracies, the emerging power of warrior chiefs, intense status rivalry, economic stratification, administrative bureaucracies, forms of private property, feudatory relations, loosening of kinship bonds, organized priesthoods, territorial conquest, and so on. Such comparisons would constitute further testing of the status-rivalry hypothesis.

The hypothesis that status rivalry plays a primary role in cultural evolution is the most pointed implication of the Polynesian study. In anthropology we have yet to explore fully the cultural significance of status rivalry or of different status systems. We have yet in fact to establish a general classification of status systems. On this score, too, the Polynesian material is suggestive because it includes the primary elements for a general classification of status systems. The primary elements consist of four main criteria of status: age, prowess, descent, and wealth, and three main prerogatives of status—prestige, privilege, and power. Various combinations of these define a basic status system. On the strength of the Polynesian data we might expect the combination of wealth with power to be the most potent force for cultural change, whereas the combination of age and prestige would be a feeble one.

By bringing forward status rivalry as a major force in cultural evolution, I wish to suggest new lines of research rather than to introduce another monistic determinant of cultural change. The point that cultural causality is multiple and complex need hardly be labored. In the following examples, drawn from the Polynesian data, the interaction is brought out between status rivalry and such other major determinants of cultural change as physical environment, demography, economy, warfare, migrations and diffusion.

PHYSICAL ENVIRONMENT

The most significant feature, in this respect, is the fact that Polynesia is an island area. Much can be said about distinctions between island areas and continuous land masses as they affect culture change. For present purposes, it is sufficient to mention two: the inelasticity of island land boundaries that must inevitably intensify the pressure of population upon resources, and the relative isolation of islands. On the first point, we can say that land pressure and status rivalry intensify each other. Relative isolation means that diffusion acts slowly and is unlikely, therefore, to interfere markedly with a pattern of development. For these reasons, island habitats are unusually valuable subjects for evolutionary study.

Ecologists distinguish between low coral atolls, relatively unpromising for horticulture, and the larger and structurally more diversified high islands, with their greater suitability for root horticulture. As is well known, the atolls supported only the simpler island cultures, and the high islands the more complex. In terms of the present study, all the atolls were *traditional* societies, while the high islands, as we would expect from the familiar dictum that environment limits but does not create, supported all three status systems. Sir Peter Buck's observation that the poverty of the atolls limited the growth of ceremonialism is amply demonstrated by the data. However—and this point needs to be strongly emphasized—neither smallness of size nor relative poverty of resources alone impeded the evolution of basic structural features of economy, social organization and government.

In this connection, the example of Mangareva is most instructive. A small and often famine-ridden almost-atoll, Mangareva had developed a stratified society comparable in its main lines to that of Tahiti. Mangarevan ceremonialism was stunted, but from an evolutionary point of view, ceremonialism may be merely a side issue.

The limitations the atolls imposed upon cultural development may have been due rather to two other conditions, their atypical features that attracted mainly the humbler colonists, and their relatively undiversified terrain. On the high islands, by contrast, the scattered distribution of fertile soils gave added advantage to groups fortunate enough to possess them, and paved the way, where the incentive was present, for political domination of weaker groups.

Habitat as a variable is most significant when it differentiates between societies of the same type, that share basic motivations. In Polynesia, this is illustrated by the different ways in which each of the three main status systems dealt with common problems of scarcity and of relative abundance. The *traditional* societies on the atolls met scarcity by establishing community reserves of taro beds or coconut groves. The *open* societies emphasized the facts of economic inequality. There, the well-to-do and the strong looked after the needy who were willing to accept subordinate and servile status. In Mangareva, the only example of a *stratified* society with scarcity, the strong pushed the weak off the land altogether; the lower class became fishermen. Comparing Mangareva with the other *stratified* societies, we can see how its distinctive characteristics such as barren ceremonialism, more violent internecine strife, greater political instability, and the inability to provide place on the land for the commoners were all reflections of its relative poverty.

While scarcity can be defined biologically, terms such as surplus or abundance have cultural meanings only. Thus, in Manua, a *traditional* society, the ready availability of virgin land muted economic competition and fostered relatively high status mobility. Quite another example is provided by the Hawaiian Islands (*stratified*). There, rival chiefs and tribes fought to annex new territories even though aboriginal population density—if the figures are reasonably correct—did not exceed 47 per square mile. In non-warlike Manua it was 117 and in eminently peaceable Tikopia 400. With land and resources as symbols of status and as sources of power, the *stratified* societies and the *open* societies felt land hunger keenly, regardless of the so-called objective situation.

POPULATION

All three status systems were represented among islands with varying population size. Population size, therefore, was not decisive in shaping basic structural features. Population correlated, however, with ceremonialism and with growth of craft specialization. Average population densities did not differ much between *traditional* and *stratified* societies, but were lower—if the figures are correct—among the *open* societies. Lower population density may have been due to the poorer resources of Niue, Easter, Mangaia and the Marquesas, as well as to the related handicaps of political instability and chronic warfare.

It was with local distributions of population that considerations of social status were actively involved. Fundamentally, population followed the distribution of natural resources, being most dense in fertile areas and least dense in unpromising areas; but this clear ecological relationship must be considered along with examples such as these: In land-abundant New Zealand, junior lines escaped commoner status by starting a new community. Thus, where there was no economic compulsion to move, status supplied the motive. In the land-abundant Hawaiian Islands, on the other hand, high chiefs regulated land occupancy according to their own political interests. This would account for the fact that some valleys were intensively cultivated with terracing and irrigation virtually to the mountain peaks, while in others only the fertile zones were developed.

Even more striking was the relationship between status systems and the social distribution of population whether by villages or by dispersed settlements. Except for Tongareva, the *traditional* societies were organized in villages; the *stratified* societies in dispersed communities; and the *open* societies were divided, with villages in Niue, and perhaps in Easter, and none in Mangaia or the Marquesas. Villages reflected the more democratic and less power-centered political systems of the *traditional* societies, whereas the dispersed settlements corresponded to the grouping of peoples around the estates of leading chiefs. The Tongarevan exception seems due, in fact, to the decline of traditional authority and the necessity, therefore, to guard coconut groves against pillage and theft.

ECONOMY

The system of land ownership was the economic factor most intimately and decisively involved in Polynesian cultural evolution. Thus, the transformation of kinship-based land systems into feudatory and quasi-private property relations became a governing feature of the *stratified* societies. By contrast, technology, modes of subsistence, specialization of labor and trade were of minor significance. Regardless of social system, Polynesians shared common technical skills, providing that they possessed the necessary materials such as wood and stone. The most elaborate irrigation systems, to be sure, were in the two most developed political systems, Tahiti and Hawaii. On those islands, the evidence suggests, political exigencies provided both motive and organization for large scale public works needed for intensive cultivation. As for subsistence modes, reliance upon the coconut seems to have been a meager basis for cultural development compared with root horticulture or the cultivation of breadfruit. The coconut was the principal cultivation on the atolls.

The view of craft specialization as a by-product of economic "surplus" needs to be reconsidered in the light of the Polynesian data. Among many conditions stimulating craft specialization, the most important were considerations of social status. Specialization gave high social standing in all Polynesian societies, and so we find the *stratified* societies multiplying specialists, i.e., honorific titles, beyond administrative and economic requirements. Hawaii was a prime example. Socially fostered scarcity was another condition that stimulated craft specialization. In Mangaia, for example, the vanquished and dispossessed had to rely on craft skills for sustenance. In both the *open* and the *stratified* societies craft skills often were either avenues to status or an economic refuge for the landless.

WARFARE

Warfare had multiple causes but tended to come strongly under the influence of status rivalry. When it did, it became a principal instrument of culture change. Wars toppled the traditional lines of authority, gave power to new social elements, ruptured kinship ties, rearranged boundaries, sharpened social cleavages by hardening the line between weak and strong and by placing a premium upon violence. Social stratification and statelike political systems were installed through this complex process rather than by simple conquest of one ethnic group by another.

MIGRATIONS AND DIFFUSION

The thesis of this study that all Polynesian societies were basically variants evolved from a common underlying Polynesian culture does not necessarily contravene the carefully established facts of diffusion and migration. Migrations carried cultural developments forward to other islands and diffusion transported traits from all parts of the Pacific basin. But such movements affected only cultural details and not the main lines of evolution. This can be deduced from the observation that almost every principal structural feature of the *stratified* societies had its forebears among the *traditional* societies. In many cases the transformation can be traced step by step. Two transforming processes were at work, intensification and readaptation. Intensification means simply that traits weakly developed in the *traditional* societies became more emphatic in the *stratified*. Examples include: caste, tabus on women, violence in mourning, prevalence of warfare, cruelty in combat, the use of force, sorcery, and of course inequalities in prestige, privilege, and power. Readaptation was the more complex process by which traditional practices and forms were turned to new uses as the total cultural pattern changed and established new contexts. These are examples of readaptation: formal tributary payments to chiefs were readaptations of first fruit offerings; feudatory relationships were a readaptation of the traditional paternalism of Polynesian lineage heads to new social and economic conditions; stratification was a readaptation of graded rank to class after the status emphasis had shifted from control over products to control over basic resources; and in centralized political authority over a territorial unit we see the readaptation of traditional political and economic powers of tribal chiefs to new conditions resulting from both military conquest and the progressive weakening of kinship ties. Finally, the emergence of private property is best illustrated in the Marquesas where chiefs who had been stewards over tribal lands asserted themselves as landlords. The prevailing conditions of an *open* society, in effect a transitional society, made such an assertion both possible and real.

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Notes

1. Goldman, I., "Status Rivalry and Cultural Evolution in Polynesia," *American Anthropologist*, 57 : 680–697, August, 1955. Consult for bibliography.

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LES TENDANCES MODERNES DE L'EVOLUTION DES SOCIÉTÉS MÉLANÉSIENNES (NOUVELLES-HÉBRIDES ET NOUVELLE-CALÉDONIE)

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Dans les limites trop brèves du temps imparti, je voudrais esquisser les grandes lignes de l'évolution de deux sociétés mélanésiennes que je connais particulièrement bien, celle de Nouvelle-Calédonie et celle des Nouvelles-Hébrides. Je procéderai d'abord, pour l'une et l'autre, par un rappel des principaux éléments de la structure politique traditionnelle.

Aux Nouvelles-Hébrides, constituée à l'image de l'habitat, la Société est caractérisée par une extrême dispersion: petits groupements d'agriculteurs rassemblés dans des villages qui ont peine à dépasser cinquante habitants. Sur le terrain le groupe humain se définit par le barrière extérieure qui isole le village des cochons domestiques laissés en liberté, par une place de danse ombragée de banyans, et souvent par des grands tambours de bois creux, sculptés d'un visage humain à leur sommet, tambours plantés en terre par groupe ou posés à même le sol. Un peu sur le côté et parfois même tout à fait en retrait, protégé par des barrières qui l'isolent du contact avec le monde impur des femmes, une case de belles dimensions est réservée aux hommes, célibataires ou mariés. C'est là qu'ils viennent fumer, manger et causer entre eux, dormir, même s'ils sont mariés, boire le kava et dans certains districts participer à des liaisons amoureuses homo-sexuelles.

L'élevage du cochon pourrait presque permettre de définir une civilisation néo-hébridaise. Les canines supérieures des bêtes leur sont enlevées au bout de quelques mois, afin de permettre aux canines inférieures de croître sans s'user sur les dents du dessus; elles poussent alors en spirale plus ou moins ouverte sur l'extérieur. Ce sont ces défenses, dont la course peut arriver à effectuer bien plus d'un tour complet, qui définissent la valeur de la bête; chaque niveau de la courbe est affecté d'un terme descriptif permettant un concept de la hiérarchie des valeurs. Ces cochons sont alors utilisés comme une véritable monnaie, presque au sens occidental de ce terme. En effet, non seulement ils servent à des usages cérémoniels, mais ils permettent de payer des travailleurs; ils sont échangés contre des objets de valeur (nattes, colorants, éléments de vêtement ou de la parure), des femmes, sinon même parfois du terrain. Ils constituent éventuellement le prix du sang pour un meurtre, ou la réparation d'une offense. On les utilisera pour rémunérer l'œuvre d'un sculpteur, ou acheter le droit de participer à une cérémonie. Comme la plupart des gens ne disposent pas d'assez de cochons pour ce genre de besoins, ils s'adressent aux gros éleveurs qui leur prêtent les bêtes nécessaires. Ils devront alors dans chaque cas rendre un cochon dont les défenses auront crû de la longueur correspondant au niveau qu'auraient dû atteindre les défenses de la bête empruntée, en plus d'une certaine longueur à débattre au titre d'intérêt. On devine la complexité des contrats oraux auxquels donnent lieu de telles tractations.

C'est la réunion des hommes à l'intérieur de leur grande case, dite *nagamal*, qui constitue le corps social. Le pouvoir de décision y appartient aux plus vieux et parmi ces derniers les gros éleveurs sont ceux dont la parole a le plus de poids. Pourtant, l'originalité hébridaise réside non dans ce rôle prééminent de l'âge et de la richesse, mais dans les modalités de son organisation. Du point de vue formel en effet, l'organisation politique revêt l'apparence d'un système ordonné. Au clivage vertical par la séparation des sexes s'ajoute, dans la société masculine, une organisation par couches horizontales, suivant une hiérarchie bien définie de grades qu'il faut acquérir successivement au cours de la vie: la possession des plus hauts grades procurant prestige et puissance politique.

Les cérémonies de prise de grades constituent la partie publique des rituels particuliers de la société des hommes; ces grades s'acquièrent au cours de cérémonies auxquelles femmes et enfants peuvent assister, les premières évitant de trop s'approcher. Un monument planté, pierre ou statue, à chaque fois différent, en est le symbole, et le paiement en cochons de ce dernier, marque à la fois la fin et le sommet du rituel. Un élément essentiel de la cérémonie est le sacrifice d'une bête de prix, que l'impétrant assomme en proclamant à haute voix le nouveau nom auquel il a droit, son titre précédent ne pouvant plus être prononcé sans injure personnelle entraînant le sacrifice d'une bête pour l'expiation.

Il y a de quatre à douze grades, suivant les districts, sinon même suivant les villages, et l'organisation porte le plus souvent des variantes phonétiques de son nom en bichelamar: *namangi*. La règle principale du système, règle qui prend effet dès le premier grade, est de ne pas manger de nourriture préparée par les femmes: il faut faire sa propre cuisine. Cette clause embarrassante se voit souvent résolue aux grades inférieurs par la vie en commun des gens de même grade. Le nouveau promu peut participer au feu de ses collègues moyennant le don d'un petit cochon ou d'une natte. La chose est d'ailleurs parfois facilitée par le fait que certains grades différents peuvent se regrouper pour participer à un même feu.

Ce principe, absolu sauf lors d'exceptions rituelles bien définies et rares, s'applique à tous les aspects de la vie quotidienne. Ceux qui ne peuvent manger ensemble, ne peuvent ni s'asseoir au même endroit, ni au même niveau à l'intérieur du *nagamal* séparé en compartiments, ni utiliser les mêmes instruments. Excepté les tout premiers, dont les représentants peuvent encore manger ce que fournissent—au lieu de le cuire—les femmes, les grades ou les catégories de grades ont chacun leurs arbres fruitiers taboués, leurs coins de jardin et leur marque qui, gravée sur un arbre, en interdit les fruits à tout autre que le propriétaire.

Il en est de même en ce qui concerne les peintures rituelles ou l'ornementation végétale (feuilles ou fleurs) utilisée en grand pour la parure masculine. Chaque grade disposera d'un motif particulier à dessiner sur son visage avec des pigments végétaux, d'une fleur particulière à se piquer dans les cheveux à un emplacement déterminé, et de feuilles aux couleurs vives à glisser dans la ceinture à l'aplomb du pli fessier.

Un écho de cette hiérarchie se retrouve dans la société des femmes, où les épouses de certains dignitaires ont droit, après paiement, au privilège du port de jupes teintes en rouge par exemple, ou de bracelets de coquillage.

Nous venons de voir que le rôle de l'individu était subordonné à son rang dans le *Nimangi*. Cette organisation semble donner à chacun ses chances d'acquérir, grade après grade, la suprématie politique à l'intérieur du village; cette montée dans la hiérarchie étant fonction de la richesse de l'individu, on pourrait y voir un système ploutocratique se renouvelant par la base, les individus les plus capables arrivant toujours au sommet. Mais il n'est pas niable que l'élevage et le commerce de cochons soient un des principaux moteurs du système. Parvenus aux plus hauts honneurs, les gros éleveurs se font payer les rituels par les plus jeunes, et, grace au prêt à intérêt, augmentent considérablement le volume de leurs affaires. Par le fait même, il leur est facile, non seulement d'accroître leur prestige en payant de nouvelles cérémonies, mais aussi de donner à leurs enfants une sérieuse avance pour l'acquisition de grades bien avant l'âge auquel y parviennent dans chaque cas les gens du commun.

Ainsi, à peine entrevue, la belle ordonnance du système, apparait faussée. Pourtant, si l'avance que permettait la richesse paternelle est un gros atout, elle pourrait se voir compenser par plus d'astuce commerciale. En réalité, facteur déterminant, elle se voit compléter par une acceptation générale de l'inégalité de possibilités qu'elle entraîne. Le faible nombre des titulaires de grades élevés ne correspond pas seulement à la faiblesse de la durée moyenne de la vie, mais aussi au fait que nombreux sont ceux qui se contentent de grades inférieurs. Par contre l'inégalité de fait se voit légèrement corrigée par l'inégalité des taux de paiement pour un grade déterminé; on s'aperçoit que le commun des mortels se voit demander bien moins que les candidats mieux nés: la différence est parfois du simple au double; elle correspond à des nécessités de prestige qui sont l'apanage dans chaque district de quelques grandes familles nécessités d'ostentation, de parade des richesses. L'avarice, défaut généralement condamné, parce qu'existant, leur est permise encore moins qu'à d'autres. Ils ne pourraient s'abstenir d'acquérir leurs grades aux plus hauts prix et de se faire initier au plus grand nombre possible de rituels. En certaines régions, cette différence de condition a permis, comme dans le Nord de Malekula (Big Nambas) l'établissement d'une chefferie plus classique combinant sa fonction héréditaire avec une hiérarchie de grades dont elle s'est arrogée les prérogatives essentielles.

Mais l'organisation politique fondée sur une chefferie héréditaire est trop peu caractéristique des Nouvelles-Hébrides pour que nous ne l'étudions ailleurs que dans son habitat d'élection, la Nouvelle-Calédonie et les îles Loyalty.

Aux îles Loyalty, que nous prendrons comme exemple type, l'offrande annuelle des prémices de la récolte est la clé qui permet de comprendre le mécanisme de cette société encore très vivace. Pour l'autochtone vivant entre la pêche et les travaux de la terre, l'offrande d'une partie de son travail constitue peut-être l'acte social par excellence. Les premières jeunes ignames portées en don par un cadet à son frère aîné déclenchent une mécanique complexe dont le déroulement offre une image presque complète de la structure de la Société Loyaltienne.

La première offrande, celle du cadet à l'aîné, n'est pas mise de côté pour l'alimentation quotidienne; on en garde une partie et le reste, s'ajoutant à l'offrande personnelle, est porté par l'aîné d'un groupe de frères à celui que l'on considère comme la tête de la lignée. La meilleure part des offrandes reçues par ce dernier, augmentée d'une substantielle contribution, est remise ensuite à celui qui détient la position prépondérante au sein du groupe local (sous-clan); le même processus répercute les prémices, toujours plus considérables malgré la dîme prise au passage, jusqu'au chef de clan, puis au chef du village ou d'un groupe de clans autonomes à l'intérieur du village, enfin en dernier lieu à l'homme que l'on désigne ordinairement du nom de grand chef.

Toute chefferie est ainsi en quelque sorte supportée par le groupe des gens qui font l'hommage des prémices. Ce sont ceux qui sont les plus proches du chef, disent les anciens, ils sont comme "le père et la mère du chef".

Aux échelons inférieurs, le rituel du don des ignames n'est guère plus apparent que le transport des tubercules suivi des brèves allocutions réciproques de présentation et de remerciement. Il en est tout autrement au niveau du chef de tribu comme du grand chef; les prémices de chaque clan, constituant une provision importante, doivent être disposés cérémoniellement dans une enceinte particulière située à l'intérieur de la chefferie.

Certains clans ne sont pas tenus à l'offrande rituelle des prémices, mais se contentent à un autre moment de l'envoi d'un présent dit de "bonne volonté". Ce privilège les désigne comme gens d'importance; leur parole jouit d'un poids particulier du fait qu'ordinairement ils représentent les clans des plus anciens possesseurs du sol et ont la prérogative de régler toutes les questions foncières, plan sur lequel le chef ne détient aucune autorité coutumière.

La tradition sociale veut qu'aux îles Loyalty le chef soit entouré d'une véritable cour de dignitaires, dont chacun joue un rôle bien défini en tant que représentant d'un clan particulier, à moins que lui même ou celui qu'il délègue ne vienne s'ajouter au commun des serviteurs. Voici une liste type de ces dignitaires spécialisés:

— le héraut, dit "bouche du chef", qui annonce au peuple les décisions du chef et de son conseil;
— l'"homme de la demeure", serviteur spécial du chef qu'il ne doit pas quitter; il a souvent pour l'assister un officier de bouche chargé de faire, ou plutôt de surveiller la préparation de la nourriture destinée au chef;
— les conseillers du chef, conseillers mais aussi ouvriers en sa demeure, autant l'un que l'autre, d'autant plus que, représentants de leurs clans, le hasard peut faire qu'ils ne soient de loin pas anciens au regard de l'âge;
— le prêtre, devin, sorte de conseiller à un degré supérieur. Il laisse entrevoir l'issue, bonne ou heureuse des entreprises projetées par le chef; sa parole a force de loi, même pour ce dernier; il a le privilège d'énoncer le jugement ou la décision qui clôt automatiquement les discussions du conseil de la chefferie;
— les hommes chargés de l'entretien ou du renouvellement des constructions de la chefferie; il peuvent décider de ces travaux sans en référer au chef.

Un de ces dignitaires, pas toujours le même, a, par suite de circonstances historiques, le redoutable devoir d'infliger une correction manuelle au chef, s'il juge que sa conduite n'est pas telle qu'elle devrait être. A Lifou, certains autres de ces dignitaires, en principe deux par grande chefferie, ont le privilège éventuel de déposer le grand chef et de lui nommer un successeur.

Le chef loyaltien n'est pas l'aîné des fils ou même neveux du chef mort, mais celui d'entre eux, qui a été accepté et intronisé comme tel par les dignitaires de la chefferie; c'est là en fait toute sa légitimité. Par voie de conséquence, le chef n'a de pouvoir absolu sur ses sujets que dans la mesure où il est soutenu par l'opinion générale. On sait qu'un mécanisme social est prévu pour arrêter ses débordements possibles; mais encore faut-il qu'ils atteignent un certain degré. Il existe vis-à-vis du chef tout un comportement d'ordre affectif qui lui permet de sortir des normes; un chef a droit à une conduite qui le distingue du commun des mortels.

Ce schéma général est celui des grandes chefferies; sous une forme plus modeste, mois élaborée, il est aussi celui des chefferies de moindre importance. Il ne suffit pas à rendre compte de tous les cas, et surtout de celui, fort gênant, du point de vue administratif, où des chefferies voisines se recouvrent territorialement par dessus les limites officielles de district. Il y a aussi le cas des chefferies coutièrement autonomes par rapport à la grande chefferie de leur district. Il y a encore le cas, apparemment absurde pour nous des deux chefferies Bahit et Imwene coexistant parallèlement dans le district de Wekiny (Weneki) sur Ouvea, avec les mêmes limites géographiques et parfois les mêmes sujets; la seule différence est que l'une est reconnue par l'administration et l'autre pas; mais toutes deux sont traditionnelles.

En Nouvelle-Calédonie, excepté dans les régions de Canala et l'île des Pins où l'organisation politique est similaire à celle des îles Loyalty, le schéma apparait plus simple dans son principe. La complexité, que nous ne pouvons analyser ici, réside cette fois dans la multiplicité des modalités locales de l'institution, dans la réalisation à chaque fois différente du rôle du chef par rapport aux prémices, de sa plus ou moins grande autonomie, parfois très relative, vis-à-vis des clans maîtres de la terre ou des rituels de fertilité. Nous ne pouvons malheureusement nous étendre ici sur ce point.

Voyons maintenant ce qu'un siècle d'évolution a fait des structures dont nous avons esquissé la description.

Les Nouvelles-Hébrides, qui se rattachent déjà à la Mélanésie classique par l'absence de chefferie héréditaire, ont subi, dans des circonstances de contact culturel analogues, une évolution parallèle. La christianisation a très vite détruit toute l'organisation politique locale, ne laissant subsister de la tradition que les règles matrimoniales. Les groupements restés païens, ceux qui ont résisté à la tentation de rejoindre le christianisme et qui aujourd'hui hésitant à le faire, cherchent une voie qui leur évite de faire ce pas, tout en leur permettant d'adapter leur vie sociale aux conditions modernes. A l'intérieur des groupes christianisés, et tout au moins du principal d'entre eux, celui des presbytériens, une réaction se produit, poussant les intéressés à prendre une autonomie de plus en plus grande vis-à-vis du missionnaire, qui pour eux n'a parfois apparemment plus d'autre intérêt que d'être un pion entre leurs mains habiles à manier des intrigues locales extrêmement complexes, jouant du missionnaire contre l'administration, le colon, ou même contre un de ses collègues moins souple ou moins influençable. Certains ont poussé ce désir d'indépendance jusqu'à déserter la mission et à édifier un néo-paganisme sous l'égide d'un Messie, forme moderne d'un dieu ancien rappelé à une existence nouvelle dans la conscience collective. C'est le cas du mouvement John Frum de Tanna au sud des Nouvelles-Hébrides. Après un demi-siècle de christianisation intensive, la société païenne qui a surgi du bouleversement effectué en 1941 offre une apparence de retour complet au passé, à un passé baigné dans la ferveur intense d'un culte des ancêtres redevenu public, et dont les manifestations ont lieu chaque soir à la cérémonie du *Kava*. La structure politique ancienne ne s'est pas en réalité reconstituée. Tant que persistera l'opposition administrative et missionnaire auz volontés locales d'autonomie, les prophètes et leaders du mouvement, utilisant plus ou moins consciemment des techniques de non coopération, fourniront un encadrement suffisant dans une situation fluente. Ces hommes changent au gré de la répression, des arrestations ou des mises en liberté. La dépopulation ancienne fait que ceux qui prétendent à une dignité traditionnelle représentent en gros deux hommes adultes sur trois, dont aucun ne peut plus prétendre à une prééminence, même sur le plan du village. Il en résulte une sorte de vide institutionnel, aujourd'hui reconnu, et que le Gouvernement Condominial s'emploie à remplir de façon suffisamment démocratique pour que la solution proposée recueille l'adhésion générale. C'est d'ailleurs le seul moyen de permettre aux aspirations profondes des gens de l'île de s'exprimer avec des modalités plus positives qu'un mythe irrationnel spéculant sur des bouleversements cosmiques.

Ce messianisme exhaltant était le fait de chrétiens dont beaucoup étaient lettrés dans la langue vernaculaire, et dont les apôtres furent les plus jeunes "Teachers", formés à la langue anglaise, et qui représentaient l'espoir de la mission.

Curieusement, les populations montagnardes de l'intérieur d'Espiritu Santo, ont subi une évolution qui aboutit aujourd'hui à des modalités beaucoup plus rationnelles. Ce groupement, peu nombreux (environ 2000 individus), est resté païen malgré des tentatives de christianisation aggressives. N'ayant jamais subi de pénétration européenne sur son territoire, pas même administrative, il a néanmoins été en contact régulier avec les européens installés sur la côte, au moins depuis une trentaine d'années.

Les relations sociales se comptabilisent aujourd'hui en schillings d'argent, au lieu de l'être en cochons à défenses recourbées. Les conditions géographiques, l'éloignement de tout centre européen, conférant aux montagnards une autonomie politique quasi absolue, les modalités de l'acculturation ont pu prendre des aspects rappelant celles de l'intérieur de la Nouvelle-Guinée. L'organisation de nouveaux "cultes" à tendances syncrétistes, pas toujours assimilables aux Cargo Cults, et dont l'un chassait l'autre, a provoqué depuis 1928 un bouleversement complet de l'organisation ancienne, qui ne se retrouve plus qu'à l'état de traces. Pendant les six années que dura le mouvement dit du "Naked Cult", l'exogamie du groupe local, sinon de la vallée, se vit remplacée par une endogamie à l'intérieur même du groupe local, faisant fi des interdits matrimoniaux qui avaient régi des générations. Le système social fondé sur la hiérarchie de grades avait été balayé spontanément avec la mise à mort déjà ancienne et générale de tous les cochons du centre de l'île. La coutume de la dot à rebours, si résistante à l'influence missionnaire, s'est vue abandonnée par la majorité des habitants. Si ces mêmes gens boivent encore le kava, c'est d'une manière entièrement sécularisée, les femmes et les enfants buvant aussi sans qu'aucun interdit ne soit plus en vigueur à ce propos.

Les manifestations collectives ou personnelles du culte des ancêtres semblent bien diminuées, en même temps que la croyance à la sorcellerie et à ses méfaits fait l'objet d'une répression menée par le dernier prophète de la région, tenant d'une mystique de la pureté collective excluant de la région, tant les cochons, que les sorciers, teachers et missionnaires presbytériens, mais appelant de ses vœux ce qui serait enseignement non confessionnel et assistance médicale.

Les groupements chrétiens ont eux évolué dans un double sens: recherche d'une autonomie économique vis-à-vis du colon, du commerçant et du coprah maker européen, par la floraison spontanée de coopératives de production et de consommation. Sauf en de rares cas, manquant totalement d'assistance technique, ces coopératives ont fait faillite les unes après les autres, souvent d'ailleurs après avoir été grugées par des conseilleurs européens de mauvaise foi. Mais il est intéressant de noter que leur but avoué était de s'organiser pour avoir leurs écoles, leurs hopitaux, et qu'elles cherchaient à diriger toute la vie des groupements qu'elles influençaient. Là aussi il s'agissait d'une tentative d'autonomie sur le plan local. Certains éléments autochtones commencent à penser en termes vagues à un regroupement politique sur le plan de l'archipel. Des forces confuses s'agitent, cherchant une voie à une volonté d'indépendance chaque année plus précise, mieux affirmée que certains espèrent au moyen d'une éviction soudaine des Européens, mais que le plupart conçoivent dans l'ignorance voulue du cadre institutionnel condominial, qui n'a pas su encore présenter des possibilités d'intégration aux ambitions qui se font jour.

La territoire française de la Nouvelle-Calédonie et des îles Loyalty présente à ce point de vue un tableau entièrement différent. La Nouvelle-Calédonie ayant été choisie par le Gouvernement du Second Empire, puis de la troisième République comme colonie de peuplement, la première période de contact fut marquée par l'adaptation nécessaire de la Société Autochtone à la situation nouvelle. Cette adaptation ne se fit pas sans heurts. D'abord la conquête militaire avec les expéditions dites de pacification dans l'intérieur de l'île, puis deux rébellions organisées et les répressions qui suivirent. Aux pertes dûes à la dépopulation, phénomène général en Océanie, s'ajoutèrent les pertes importantes provoquées par ces évènements. Il en résulte qu'aujourd'hui sur la Grande Terre, la population autochtone se concentre dans les hautes vallées de la côte ouest et sur la côte est de l'île. Ces transformations démographiques s'accompagnèrent d'une aliénation importante de terres au profit de la colonisation européenne et du regroupement des autochtones à l'intérieur de réserves dont les limites variaient au gré de l'administration locale. Cependant entre les deux guerres et après la dernière insurrection de 1917, la situation foncière se stabilisa, mais la population indigène restait soumise au régime dit de l'indigénat: impot de capitation, interdiction de se déplacer sans autorisation administrative, larges réquisitions de main d'œuvre au profit de l'administration ou de la colonisation. En somme il s'agissait d'une situation coloniale classique dans la perspective générale d'un Empire, régi par une métropole occidentale. Le côté positif du tableau était donné par une politique brutale, mais valable, de mise en valeur des réserves. Les paillottes furent brûlées par mesure d'autorité pour être remplacées, par de grandes cases à l'européenne aux murs en torchis; on dut planter une partie de la surface des réserves en cultures perennes et surtout en café, introduisant ainsi l'agriculteur mélanésien à une existence de producteur autonome dans l'économie monétaire. Aux approches de 1940, une politique scolaire commença à doter la population autochtone, des éléments d'une connaissance généralisée du français.

Sur le plan de l'organisation politique traditionnelle, les bouleversements dûs aux modalités de la prise de possession portèrent un coup funeste aux multiples chefferies de toutes grandeurs trouvées en place à la prise de possession. Un arrêté local du 27 octobre 1897 prétendit introduire le concept de grandes et de petites chefferies organisées suivant une hiérarchie donnant droit à des grades à consonnance militaire et à des galons d'or ou d'argent. La politique de cantonnement des autochtones obligea l'administration à faire un choix dans les candidats possibles. Les chefferies administratives qui émergèrent de cette réforme furent considérées comme devant se mettre au service de la colonisation. Le chef, même s'il était encore parfois dans une certaine mesure traditionnel, se transforma en auxiliaire du gendarme, en agent de recrutement pour le travail forcé — il lui était ristourné 5% sur les salaires de ses sujets engagés. En fin de compte, le chef dont l'action était trop souvent opposée aux intérêts des siens, ne faisait plus reposer son autorité que sur la menace de punitions administratives décidées à la diligence du gendarme syndic des affaires indigènes. Quand en 1945, le régime de l'indigénat se vit supprimé, les chefferies perdirent leur dernier appui et ce qui pouvait rester de prestige aux meilleurs se désagrégea au cours des années qui suivirent.

Devant la nécessité, confrontée avec une situation qui pour elle était révolutionnaire, se voyant octroyer d'abord la liberté de résidence puis progressivement des droits politiques aujourd'hui entiers, la société autochtone tendit à se resserer, et à chercher une adaptation à ses nouvelles conditions d'existence. L'ancienne institution du Conseil des Anciens, en pratique officiellement ignorée jusqu'alors, fournit le cadre d'une certaine réorganisation de la structure politique, réorganisation qui s'effectua dans une grande mesure spontanément, mais avec la sympathie tant des missions que des pouvoirs publics.

On pouvait croire que la levée brusque d'un régime d'arbitraire que les conditions de la guerre n'avait en rien adouci, provoquerait une flambée nationaliste qui peu à peu approfondirait une volonté d'autonomie sur le plan politique. Certains indices portaient à le craindre entre 1945 et 1948. Mais d'autres facteurs jouaient. Même au cours des années de guerre, le réseau médical et scolaire s'était développé. La dispersion des colons européens aidant on se trouvait en face d'une génération d'hommes faits, chrétiens dans leur immense majorité et parlant tous le français qui s'imposait de plus en plus comme langue véhiculaire. Certains avaient fait la guerre sur les théâtres extérieurs comme volontaires de la France libre, suivant en cela la tradition des anciens combattants de la guerre de 1914–18. Beaucoup avaient ainsi appris à aimer la France, soit parce qu'ils y avaient été bien reçus, soit avec les Missionnaires qui furent longtemps leur seul appui contre l'arbitraire. Une nouvelle génération se formait, élevée dans des écoles où seul le français était admis et chez qui l'analphabétisme ne serait plus qu'un souvenir.

Ainsi par sa libération juridique inattendue, et effective avant d'être exigée, cette population se retrouvait française au même titre que les Européens et ses revendications ne portèrent-elles d'abord que sur la suppression de toute discrimination raciale. Des mesures législatives prévues pour une élite africaine et appliquées localement sans modifications se traduisirent du jour au lendemain par une grosse masse électorale mélanésienne. La situation économique des agriculteurs autochtones, qui, grace à leur production de café et à leur méthodes culturales bien adaptées, sont pauvres, mais ni misérables, ni sous-alimentés; la modération de leurs leaders politiques, presque tous élevés par les missions; l'attitude psychologique des autochtones, exempte de tout complexe d'infériorité vis-à-vis des européens; l'attitude des européens eux-même dont fort peu insistèrent pour l'application de mesures discriminatoires telles que le double collège électoral; l'émergence, parmi les Européens de personnalités politiques nouvelles, non compromises dans la situation coloniale antérieure et qui surent créer un climat de collaboration politique entre autochtones et européens, tout cela aboutit à une situation presque unique dans l'Union Française et qui n'a peut-être d'autre parallèle que l'harmonie interraciale qui règne dans le territoire de Hawaii.

Il suffira en conclusion de citer les dernières mesures politiques et sociales prises dans ce climat si particulier: suffrage universel sans distinction d'origine raciale; élections au collège unique; extension des prestations familiales à tous les travailleurs et aux mêmes taux, quelque soit leur origine; possibilité aux autochtones d'obtenir des concessions de terres du Domaine de l'Etat dans les mêmes conditions que les européens; suppression progressive en cours de toute discrimination raciale en matière de salaires; organisation d'un système de Sécurité Sociale pour tous les éléments de la population.

Quand on pense aux luttes qui déchirent tant de parties des ex-empires coloniaux, il apparait que l'exemple de la Nouvelle-Calédonie, petit territoire de 65.000 habitants, vaut largement la peine d'une analyse plus approfondie. Et de cette comparaison avec la situation existant dans l'archipel hébridais si proche, que conclure, sinon qu'il n'y a pas de voie unique pour l'évolution politique des territoires non autonomes. Aucune loi rigide n'oblige à passer par le stade du nationalisme. N'y-a-t-il pas là matière à méditation?

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A RE-EVALUATION OF THE CULTURAL POSITION OF THE NOOTKA[1]

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There are three aids to the study of culture history: archaeology, historical documents, and ethnography. These are available in varying proportions for different cultures and often do not present a continuous picture, but with support from the ethnographic sources for the region, a well documented study of a single group can be assembled. This will be done for the Nootka in an effort to show that they have not always been as marginal to the more elaborate cultures of the Northwest Coast as they appear in the period during which the major ethnographies were written. In contrast to the Haida, Tsimshian and Tlingit with elaborate clan systems reflected in the art, and the Kwakiutl, where secret societies and the potlatch system were highly developed, the Nootka at the same period appear poor. They seem on the outer fringe, both geographically and culturally with a simple bilateral family, a less developed art and a ceremonial system which shows relationship to others on the coast, but is comparatively less complex. This comparison is chronologically important, for, since it is known that the Northwest Coast developed relatively late, it may be that the Nootka in the early contact period exhibited the base from which all coast cultures began. The great variety of documentary material for the Nootka shows that without historical depth, a culture can easily be misinterpreted.

The archaeology of this region presents difficult problems in chronology and while it gives some clues as to the remote origins of the cultures, it does not yield much for the immediate pre-contact period. The materials are mostly stone and bone, while the earliest collections gathered by the explorers are utensils and carvings of wood, throwing light on those phases of the culture which are not obtainable through archaeology in this damp climate and wet soil. These objects can also express sanctioned behavior by their very presence in the collections and thus help to bridge the gap which Drucker states lies between the study of historical documents and modern ethnographic reports, the latter dealing with sanctioned patterns of behavior and the former with overt culture traits. This point was also made by John Mills in his ethnohistory of the Nootka (Mills, p. 141). On the other hand, Mary Gormly (personal communication) who is at present working on the Spanish sources for the region, points out that the Spaniards by their longer presence in the area and their living on shore, in contrast to the British explorers who seldom left their ships, were able to observe many cultural activities which were not apparent to others who were not present through an entire annual cycle. A careful study of documentary material supported by these collections and compared with modern ethnographies can help place this culture in its proper perspective.

The historic material points to a number of culture traits which are not found in the modern ethnographies of the Nootka, but are related to traits found among some of the northern tribes. In the Cook collection in the British Museum are several pieces of armor which may be from Nootka. They agree with the descriptions found in the diaries, and the catalogue does list one from Nootka (BM-NWC 73). Ledyard describes the armor of the Nootka as of "moosehide covered externally with slips of wood sewed transversely to the leather" (Ledyard, p. 77). In Drucker's culture element list the use of rod armor is denied except by one Hesquiat informant who described what might be rod armor as of "heavy twigs" (Drucker, 1950, p. 187). It can probably be stated that armor of the type known ethnographically only from the northern tribes was also used by the Nootka at the time of contact. Their early use of muskets may have eliminated this armor from the culture sooner than in the north. By 1788 the Nootka used muskets, but still fought among themselves with spears and clubs. However in 1791, Haswell estimates that Wickananish, the chief of Clayoquot, had 200 guns and some ammunition, and in 1792 all the important people at Nootka had their own muskets (Wike, p. 41–42). There was considerable argument between the early traders whether to give the Indians guns or intoxicating drink, but when competition became keen, anything which produced sea otter skins was regarded as legitimate. Both Edgar (Journal ms.) who was with Cook, and Cook himself mention that when the Nootka go to war they wear large, frightful masks and paint themselves red and black. The use of these masks, however, is not sufficiently authenticated to make a good argument since not one has appeared in a collection. Samwell (Journal ms.), the surgeon with Cook, who was a careful observer, mentions that the elaborate headdresses were worn in war dances which the visitors saw, while they never saw a war party leave ready for action.

The handsomest pieces of stone carving in the Cook and Vancouver collections are called stone fighting weapons and resemble the copper breakers of the Kwakiutl (BM-NWC 93, 94, 95). They are mentioned by the Spaniards as wedges for splitting wood, but this is contradicted by Ingraham (letter, ms.). Of the pieces seen, only one could possibly be used as a wedge because of the carved heads where the striking surface should be. These carvings resemble a face, some more birdlike than others. The mouth is always open, like a fledgling, and the eyes are protruding circles, very reminiscent of the stalk eyes of the Salish Xwexwe mask. This weapon would call for even closer combat than the longer whalebone club. That these weapons are no longer known in Nootkan culture is understandable by recalling the statement that by 1792 practically every important person had a gun.

Another feature which both indicates a former culture trait and a fine control of art style is the "slave killer," which is prominent in the Cook collection. Two pieces are in the British Museum where they are catalogued as "Northwest Coast" (BM-NWC 97, 98) and a similar one is in the Museum at Florence (Giglioli, plate 3, #62). These "slave killers" are short wooden clubs with a human head carved at the upper end. The head is decorated with human hair and has a protruding tongue of stone which is the striking end. Niblack (USNM Report, 1888: plate 28) shows a comparable piece from the Tsimshian with a protruding raven's beak. While the British Museum has not definitely catalogued these pieces as coming from Nootka Sound, they are described as from there by Cook (Vol. 2 : 319) and Rickman (p. 244). Another form of slave killer which the British Museum does catalogue as coming from Nootka is a short wooden club with a blade of stone transversely set into the handle and above it is carved a human face. Pieces similar to this are figured by Niblack (USNM Report 1888: Plate 46) as coming from the Tlingit and were collected by United States Navy men which might place them in the second half of the

19th century. Such a piece is also described for Fort Rupert Kwakiutl by Boas (MemAFLS 28: 60), but no reference can be found to them in the Nootkan ethnographic literature. Slaves were still held in the latter part of the 19th century but probably fear of the law prevented any sacrifice as was formerly done, for unless there was a ceremonial act involved there would not be a special instrument for the purpose. Swadesh mentions in his explanation of the ethnographic texts of Sapir (SW Jour. of Anthro. 4: 83) that slaves were more important as war booty than human heads, for they could be sold for guns. There are indications that some slave trading was carried on by the white fur traders, taking Indian women and children from one group to another. Drucker states that sometimes a slave was assigned to a chief's son as a companion and if the chief's child died, the slave was killed (Drucker, BBAE 144: 272). He also describes a "display killing" directed by a Kwakiutl woman who was married at Chickliset, when a Kwakiutl chief arrived, boasting that he was travelling around Vancouver Island to beat all the chiefs he visited (Drucker, BBAE 144: 384). By the presence of these objects it is clear that formerly the Nootka participated in the ritual killing of slaves which was still a known trait in the northern cultures well into the 19th century. As with the stone fighting weapons, the art style was thoroughly developed and well executed.

Continuing with other subjective features of culture, one finds in the narratives of the early explorers and traders, as well as in the collections they deposited, indications of a richer ceremonial life than is recorded in later ethnographic literature. The description of the elaborate entertainment which Maquinna arranged for Vancouver and Quadra shows every trait which is known in the ceremonial procedures on the Northwest Coast in historic times, except that many of the costumes and much of the regalia consisted of trade goods, which they had recently acquired, already absorbed, and were anxious to display. Masks representing animals were used and dancers came out from behind a screen and performed individually. It is possible that many of the ceremonial practices which have been attributed to contact with the Kwakiutl by recent informants are actually old basic ones which disappeared among the Nootka and came back to them in later years from the Kwakiutl.

Whenever masks were mentioned in these narratives the author seldom neglected to add that they were well carved and not only this evidence, but also the actual pieces confirm the fact that the Nootka of the late 18th century had complete technical control of the skill of wood carving and had established an easily recognizable version of Northwest Coast art style. Samwell, the surgeon on the Cook expedition states: "There is hardly anything in the heavens above, earth below or waters under the earth that they have not an image of. Masks of the human face . . . were well executed." These carvings were of great variety and ranged from small decorations on costumes to great house ornamentation. Boit, speaking of the village of Hopatcisath which he was ordered to destroy by Captain Gray, says: "it had upward of 200 houses and every door was in the resemblance of an human or a beast's head, the passage being through the mouth . . . and there was more carved work . . . some of which was by no means inelegant." (Boit, p. 243.) The use of house posts with the entrance through the mouth is found on the large totem poles of the north in the next century and is never mentioned again in modern Nootka literature. If this statement did not occur several times and by men from different expeditions it would not be so impressive. The profusion of carving is another trait which is not outstanding in later Nootka culture, for house fronts and even masks are more apt to depend on painting than carving for the development of the design.

Another very excellent example of the fine carving of the period is a club (BM-NWC 100), described in the catalogue of the British Museum as a "hand instrument used in war at Nootka." It hardly seems effective as a weapon, and is more likely a dance baton, perhaps used in a warlike dance. The carving of a wolf's head is the best that can definitely be assigned to this early period of exploration. It is characteristically Northwest in style with the ornamentation of human hair and the inset teeth. The motif of an animal's mouth holding a human head or mask is frequent in the art and sometimes is even more realistically carried out by having the compressed body of the person in the jaws of the animal and the head facing outward like this mask. This is also exemplified in a fine Tlingit dance staff in the form of a sandhill crane holding a man in his beak (WSM cat., #1237).

Some of the most striking pieces in the European collections are human heads carved of a block of cedar which resemble such a piece used by the Kwakiutl in the Toxuit ceremony where the dancer's head is supposedly cut off and shown to the audience, the carved head being substituted. There are three of these pieces, two in the British Museum (BM-NWC 57, 58), and one in Florence (Giglioli, Plate III, #37). They are mentioned in King's journal: "The natives would sometimes bring strange carved heads and place them in a conspicuous part of the ship and desire us to let them remain there and for these they would receive no return." (Folio 22.) That an odd piece of this sort might have been traded over from the Kwakiutl is of course possible, but from King's remarks they must have been numerous, so that local origin is a more plausible explanation. They are fine examples of realistic carving and it is a pity that none of the writers of the journals mention them in greater detail. If they were ceremonially used, it was either at a ritual not done for the entertainment of the visitors, or it was part of a practice which they did not care to reveal, so they must, at present, remain either as an example of Nootka work, or trade from the Kwakiutl.

While these culture traits just enumerated do not appear in the later life of the Nootka, there are also a number of pieces in the early collections which show that in some features there has been an extraordinary stability in the culture. This is especially brought out in the rattles which were collected by Cook. They are illustrated in Inverarity (#111). These are in the simple bird shape which is the basic form of the rattle on the entire coast and which is the fundamental pattern of the elaborate and complex rattles of the Tsimshian and Tlingit. In the Cook collection there is a second rattle which shows a step toward this elaboration, also illustrated in Inverarity (#112) with carved instead of painted designs. The wings are carved on the back and a human face is carved on the under side. One striking item of detail also occurs on this rattle, namely a deep triangular cut, quite unnecessary to the execution of the design, on the under side of the bird's throat. This same detail is found on all the complex bird rattles of the later 19th century. The Nootka, in historic times, have been famous for their wolf ritual and the use of the wolf mask. In the old Northwest material in the British Museum, but not definitely attributed to Cook, is a wolf mask which has been often illustrated. Boas used it in his "Secret Societies and Social Organization of the Kwakiutl" (USNM Report for 1895) stating that it shows that no change has taken place in these masks in the last century. Inverarity (#102) uses the same piece, attributing it, like Boas, to the Kwakiutl. Since the Wolf ritual is one of the principal ceremonies of the Nootka and since Cook had no contact with the Kwakiutl, it is possible that this piece is Nootka in origin. Otherwise it would again presume trade from the Kwakiutl which did increase during the early days of the maritime fur trade, but the internal evidence of this being Nootka is very strong.

In the field of weapons, one piece which appears in every Nootka collection supplies also the continuity of this culture, namely the whalebone war club. It is made of material distinctive of the Nootka, for they were without doubt the best whalers on the coast and in addition made the whale hunt one of their great rituals. It is fitting therefore that this characteristic piece should be an index of this culture. This club, about 2 feet long, flat and broadening toward its lower end, and with a profile of a bird's head for the handle is found archaeologically, not only along the coast, but as far away as the Plateau region. It was never copied by others but was a constant article of trade. This club was an effective weapon in close combat. Drucker states that chiefs had names for their war clubs (Drucker, BBAE 144 : 335), indicating a special attitude toward the weapon. In the Cook and Vancouver collections these pieces are well represented and with all the perfection and style found in any piece which can be attributed to the 19th century, showing again that the Nootka at this time had developed both technique and style in their carving.

The notes which have been gathered here to review again the aspects of Nootka culture which the early expeditions described begin in 1778 with the visit of Captain Cook. When his shipmates sold sea otter skins in Canton for $120 apiece they started the maritime fur trade which soon was to change many aspects of Northwest Coast Indian life. Joyce Wike, in her dissertation (Wike, p. 3) stated in her review of this period that the area had the peculiar advantage of receiving European trade goods without the disruptive influences of colonization. This is true and there is much virtue in the circumstance, for it gave the fur trading tribes, especially the Nootka and Haida who had direct contact with the Europeans and opportunity to select from the proferred goods, and time to adapt these to their own culture with no European pattern of use to follow. Many other groups received European goods through trading with Indian neighbors before actual contact with Europeans themselves. The Kwakiutl acquired guns from the Nootka and often traded slaves for them, thus improving the position of the Nootka in trading with other Indians. This went a step further when the Europeans began to acquire Indian trade goods and carry them from one tribe to another until they found someone who would take the material in trade for furs. The superior position of the Nootka and Haida in the early trade was lost when the sea otter was almost depleted and gave way to land fur trade which created new centers on the mainland.

In this early period of sudden and heavy impact on the Nootka which established their success in trading, the principal reaction on their culture seems to be a diversion of interest. Formerly a little sea otter hunting was carried out with other activities. With the demands of the fur trade many other activities gave way to more hunting. The trade goods received for the furs did not replace the neglected activities but created or stressed new ones. The orderly flow of inherited privileges was disrupted through more deaths due to new diseases, more warfare stimulated by competition in fur trading, and the hazards of more sea hunting. The ceremonial cycle could not be carried out with regularity because of other demands on time. These subjective matters are difficult to document, but on a small scale they have been observed on reservations on the Northwest Coast when commercial fishing and cannery work gave Indians money, but deprived them of assembling their food, principally fish in their own way for the winter ahead. The money, either not wisely used, or not sufficient, would not replace that which had been lost.

The Nootka were drastically affected very early and when the maritime fur trade moved away, were left without cultural contacts because the other tribes with whom they formerly traded were then too busy with the new land fur trade. It is difficult without close knowledge of the terrain of the Nootka to realize the extent of their isolation. At the head of Alberni Canal they could meet some Salish. Through the pass in the mountains that leads to the valley of the Nimkish, is their contact with the Kwakiutl. And by sea with good weather they can go to the Queen Charlotte Islands. The Haida of the 19th century however, directed their interest toward Prince of Wales Island and Alaska. The Nimkish were not the most active culturally of the Kwakiutl and the Nanaimo of the Salish were also not culturally stimulating. So the Nootka after reaching along with the other of the Northwest Coast tribes a cultural level exemplified by the pieces in the early collections, suffered a period of cultural regression and stagnation due to their own internal disorganization and the changes brought about by European intrusion among their neighbors at the same time when that stimulus was removed from them.

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Notes

1. The original material for this paper was gathered during sabbatical leave in 1952 from the University of Washington and aided by the Neosho Grant for the Study of Material Culture.

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Museum Collections

Cambridge University Museum
Florence, Museo di Fisica e Storia Naturali
London, British Museum—Ethnographic Collections
Seattle, Washington State Museum, University of Washington
Washington, United States National Museum

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THEORETICAL CONSIDERATIONS CONCERNING THE PROBLEM OF PRE-COLUMBIAN CONTACTS BETWEEN THE OLD WORLD AND THE NEW

Robert Heine-Geldern

In the present paper I shall not attempt to prove the reality of pre-Columbian trans-Pacific contacts. For this I must refer to Gordon Ekholm's and my own published articles. I shall deal exclusively with the theoretical and methodological aspects of the problem.

In the 19th century, in pre-Boas and pre-Graebner days, it was quite legitimate, from the purely logical point of view, to assume that the ancient cultures of America—including the civilizations of Meso-America and the Andes—originated independently without benefit of any Old World influences. This assumption conformed perfectly to the then prevailing ethnological theory in which the innate psychic uniformity of man was thought to have led automatically to parallel and similar cultural developments in all parts of the world, the observed differences being the result of different geographical environments. This conception of numerous parallel developments has long since sunk into the grave of obsolete theories. Yet, curiously enough, the belief in the independent origin of American Indian cultures, which had its only justification in such a theory, was not only retained, but hardened into a veritable dogma with all that this term implies, such as closing one's eyes to manifest but unwelcome facts and the anathematizing of non-conformist heretics. This means that the theory of the so-called psychic unity of mankind and of parallel developments is no longer considered as valid, but is nevertheless tacitly allowed as far as correspondences between the Old and the New World are concerned.

This inconsistency leads to a truly paradoxical situation. Any archaeologist who today would still contend that the prehistoric and protohistoric peoples of Europe duplicated independently such old achievements of the ancient Near East as agriculture, bronze casting, iron work, the wheel, the calendar, or writing would simply make himself ridiculous. In a remarkable book, published by the Wenner-Gren Foundation a few years ago, Margaret Hodgen showed that all those industrial innovations which can be noted in England from the earliest times to the 18th century were introduced by immigrants from the European continent. But what is not conceded to the inhabitants of the British Isles nor to Europeans in general is willingly conceded to the American Indians who are supposed to have repeated independently the very same complicated inventions that had already been made elsewhere. Is there perhaps some mysterious law according to which the probability of independent reinvention increases in proportion to the geographical distance? If any one believes in such a law, I wish that he would say so and explain it to us.

The logical incompatibility between the rejection of the doctrine of parallel development and its tacit admission as far as conformities between Old and

New World cultures are concerned has for many years been the skeleton in our closet. No one spoke of it, everybody tried to forget it, and yet it was ever present. Like a secret cancer it has poisoned our ethnological thinking without our being aware of it. If in this one important case our current theories prove inapplicable, what assurance then have we that they are more correct in others? I have little doubt that it was largely this unconscious feeling of insecurity which in ever increasing measure has caused scholars to abandon thinking about large-scale ethnological problems and to turn to less ambitious, scientifically less relevant, but less risky tasks.

It is high time that we throw the skeleton into the open in order to exorcise and lay this spectre. We must have the courage, either to apply our general theoretical principles in all cases without exception or, if the existence of pre-Columbian ties between the Old and New World cultures can really be disproved, to accept the consequences and return repentantly to the 19th century concepts. For, whatever can be said about the errors of the doctrine of parallelism, it was at least a system logically consistent in itself. We simply cannot have it both ways.

The same inconsistency that we find in general theory can be noticed in the various arguments put forth in the attempt to prove the independence of American Indian cultures. I shall restrict myself to a few examples.

It has often been emphasized that to demonstrate convincingly the existence of cultural relationships the respective correspondences must be highly specific and must concern complicated inventions or concepts, the repeated independent origin of which can only be imagined with difficulty. This is a perfectly reasonable principle. Yet, the very same scholars who make these statements silently drop the subject as soon as it comes to concrete and, from the prevailing point of view, embarrassing facts. Of course there will always be marginal cases where opinions vary as to whether we are confronted with simple or complicated traits or with sufficiently or not sufficiently specific similarities. There are, however, numerous cases in which no reasonable doubt in this respect is possible. Could we, for instance, conceive of greater specific similarity than the practically complete identity, in eastern Asia and in America, of such complicated metallurgical techniques as lost-wax casting and the chemical coloring of gold? Are the contriving of cosmological systems in which specific colors are attributed to the various directions and the concept of a succession of worlds, alternatively destroyed by fire, water, and wind, really things so obvious that we are justified in assuming without further thought that they were independently duplicated in Asia and in America? Where would the natural sciences stand if zoologists and botanists were as timid as ethnologists and as reluctant to face the facts and to draw the inevitable conclusions from manifest correspondences? They would still have to assume that the monkeys of America developed locally from marsupials or even lower orders, quite independent from the monkeys of the Old World. As compared to those of the natural sciences, not to speak of the old and well-established humanities, our methods and our theoretical approaches seem to be still in their infancy.

The fact that cultural traits are never borrowed wholesale by one people from another, that invariably some kind of selection takes place, has often been emphasized. Yet, the very same scholars who uphold this principle immediately forget it as soon as the question of Old and New World relations comes up, and unhesitatingly cite the absence of various Asiatic culture-traits in America as alleged proof for the independent origin of American civilizations. This lack of consistency in applying their own principles is frequently combined with an almost incredible lack of familiarity with Old World cultures. I shall cite only a few instances. Thus, the absence of coined money in America has been mentioned as one of these alleged proofs. I wish the scholar who wrote this would rather have explained to us why coinage was not adopted by most of the ancient hinduized countries of southeastern Asia despite the close connections they had with India where coinage was used since the time of the Mauryan kings. The absence of the true arch in America is often stressed by Americanists. They obviously believe that the arch was known in eastern Asia since hoary antiquity. Actually, it became known in China only at the time of the Han dynasty, due to the contacts with the Iranian and Hellenistic world which were made in that period. Again, I wish that those Americanists who think that the absence of the arch in America disproves Old World influence would rather explain to us why it was never adopted by the peoples of Champa, Cambodia, Java, etc. who were in close contact with the Chinese. Even more popular is the case of the wheel and the plow. Since, however, those Americanists who cite their absence in America as proof of the independence of the American Indian civilizations have so far failed to tell us how they could have been used in America without the necessary draft-animals, I need not go further into the subject.

The lack of logic in the various arguments put forth in order to save the dogma of the independence of the American Indian cultures proves that we are faced here, not with a rational theory, but with a predominantly emotional conviction. This renders reasonable discussion particularly difficult. It is much the same as if we were to try to convince a confirmed Christian, or Mohammedan, or Buddhist that his religion is wrong. Indeed, the belief in the independent origin of the American Indian cultures has become a kind of religion, certainly not of all, but of a large number of Americanists. It is significant that now that it becomes increasingly difficult to deny the existence of ancient trans-Pacific links between Asia and America, one finds occasional statements to the effect that such links are not inconceivable, but that it is still too soon to speak of them. If one cannot prevent the destruction of the cherished dogma one tries to postpone it at least as long as possible.

I need say little about one argument for the independence of American Indian cultures which, until not so very long ago, appeared fairly reasonable. I refer to the alleged disparity of cultivated plants in America and the Old World. Ever since the publication of Hutchinson, Silow and Stephens' famous book on the evolution of Gossypium, nine years ago, botanists have been taking care of that subject. Today, we know that at least four cultivated species, cotton, the sweet potato, the bottle gourd, and the coconut palm, were common to America and the Old World in pre-Columbian times, with the number likely to increase in the near future. Nor can that other once formidable argument be cited any longer, the alleged time gap between certain American and Asiatic art styles, and especially the supposed gap between the first use of copper and bronze in South America and the end of the Bronze Age in eastern Asia. American chronology has become extremely fluid. Dates are constantly being pushed back, and although Americanists are still accepting the results of carbon^{14} dating with some reluctance, we can say that in some instances at least the gap has practically been closed.

A few months ago a professor at an American university jokingly remarked that the worst term of abuse known to American anthropologists is that of "extreme diffusionist." I do not know what an extreme diffusionist is, except that the term is used as a bogy to frighten undergraduates. There is no such thing as moderate or extreme diffusionism. The only thing that counts is whether in a given concrete case a diffusionist explanation is scientifically reasonable or unreasonable. Completely fantastic diffusionist hypotheses have occasionally been offered by scholars who certainly would be considered as extremely "moderate" diffusionists. I shall cite but one case.

A rightly famous archaeologist, for whom all of us have the greatest admiration, was impressed by certain similarities between the Maya and Far Eastern cultures. In a book published only two years ago he suggested that some of the ethnic groups which later amalgamated into the Maya, coming from Asia via the Bering Strait, may have brought with them not only certain cosmological concepts derived from Hinduism or Buddhism, but also the knowledge that their ancestors had once practiced agriculture. The author himself says that his view is unorthodox. It is indeed, not only from the prevailing point of view of Americanists, but from that of any reasonable ethnological theory. Imagine those ancient proto-Maya, after having migrated for generations through eastern Siberia, Alaska, Canada, the United States, and Mexico, still remembering that in the far-off days when they had lived in China or southeastern Asia their ancestors had practiced agriculture and, on the basis of that dim tradition, successfully introducing it in Meso-America.

This is a typical case of that terrific fear of the Pacific Ocean—one might almost say that kind of hydrophobia—from which many anthropologists and archaeologists seem to suffer. Moreover, it is again a case of that lack of familiarity with Old World cultures which I mentioned before. We have literary indications of the high development of boat building and navigation in coastal China at least as early as 500 B.C., and Chinese sources make it perfectly clear that in the first half millennium A.D. the ships of southern Asia were superior in size and to a certain extent in equipment to those of Columbus and Magellan. Nor should we forget the large number of East Asiatic junks which were driven to the shores of America in historic times, on the average of about one every five years. This indicates how the existence of the American continent could have become known to the peoples of Asia. Of course, one cannot consider all this as proof of the reality of ancient trans-Pacific connections, but it shows that they were technically feasible.

Although much valuable preliminary work—usually ignored by Americanists—has been done on the subject of such relations, most of it lacks that strictly methodical basis and that concern for chronology which might carry final conviction. To cite just one instance, it is not difficult at all to show that the American blowgun shares enough highly specific features with the blowguns of Asia to make its Old World origin appear practically certain. But we still would not know when, nor in what cultural context it was introduced. I do not wish to imply that the diffusion of such single cultural traits should not be studied, but from the point of view of sound methods it is not our primary task. Therefore Ekholm and I have been investigating and comparing large, well-defined complexes, such as art styles and metallurgy, which in America, as well as in Asia, are restricted to definite regions and periods and can be dated by archaeological or historical methods. So far, our results have been encouraging. We hope that when in this manner a chronological framework will have been built up, it may eventually be possible to fit into it also cultural traits involving perishable materials, customs, myths, etc.

It has occasionally been said that there is no evidence of turning points within the continuous prehistory of Mesoamerica and the Andean region that are marked by the injection of Asiatic or Oceanic traits. These were rather rash statements since the matter had never been systematically investigated. A continuous and unbroken sequence of cultures does not in itself imply continuous and unbroken internal development. We must realize that we are facing here a possible source of error. It is obvious that if changes in culture which are believed to be the result of local development should really be due to the arrival of some new wave of influences from beyond the ocean, our whole view of the cultural history of the region would be out of focus. Therefore the problem of the existence or non-existence of trans-Pacific contacts between the Old and the New World should be of primary importance to Americanists and should be given more thought than is the case at the present time.

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ETHNOLOGY AND HIGH CIVILIZATION, EXEMPLIFIED BY ANCIENT EGYPT

Anna Hohenwart-Gerlachstein

Until not so very long ago ethnologists dealt almost exclusively with the cultures of so-called primitive peoples. In recent years, however, it has become increasingly clear that this restriction is no longer tenable if we wish to attain a real understanding of the history of culture as a whole and of cultural processes.

The following points seem the most relevant for the ethnological study of the higher civilizations:

First: The respective basic world-view and its reflections in the predominant values and in the various spheres of culture;

Second: Urbanization, the stratification of society, the administration with its hierarchy,[1] the political organization and jurisdiction;

Third: The definite and conscious organization of religious cult;

Fourth: The possibility of perpetuating traditions of any secular or religious kind, of laws and of legal and commercial transactions through an enduring medium—the script, the only exception to this being Peru.

In order to avoid misunderstanding it must be stressed that ethnological research in the sphere of any high civilization can never claim to replace the specialist's work. The ethnologist's sphere of research will have to begin where that of the specialist usually ends. This implies a threefold task.

(1) The ethnologist will have to focus his attention on the phases of transition from primitive to higher culture and on the persistence of primitive traits within the latter.
(2) He will have to trace the influences of higher civilization within the areas of more primitive culture, such as, for instance, Egyptian and Meroitic influences in Africa.
(3) Since specialists in ancient oriental civilizations usually restrict their interests more or less to their own circumscribed field of research, the ethnologist will have to deal with the problems of culture contact and cultural relations between the various higher civilizations.

These are the basic ideas on which co-operative work of ethnologists and students of higher civilizations can be founded. This may be exemplified by ancient Egypt.

A few years ago, Walter Cline[2] pointed out that Egyptologists and historians like Erman and Ranke did not sufficiently consider the fact of cultural change. He complains that they depicted people who lived centuries apart just as if they had belonged to a single community. This complaint is very definitely out of date. On the contrary, the problems of culture change in ancient Egypt have constantly been discussed by Egyptologists in recent years.[3]

The Vienna School of Egyptology, represented mainly by the late Professor Czermak, came to the following conclusions:[4]

In the beginning, Egypt was divided into three parts: the East Delta, the West Delta, and the South, with distinct systems of economy and religion and distinct cosmological ideas. The East Delta was first inhabited by fishermen and only later herdsmen became prevalent. The male principle was prominent, Osiris being the representative of the patriarchal organization. The West Delta was inhabited by agricultural people. Here there seems to have been a center of the Mother-goddess complex with Isis and all her mysteries, and traces at least of matriarchal order have been noticed. In the South—in Upper Egypt—climate, fauna and flora were different from our days. The forests were inhabited by local groups of hunters.

Each of these primordial districts was a world in itself, being at the same time in constant contact with the neighboring countries. While the East Delta tended toward Asia and was affected by Semitic influences, the West Delta and the South were in close connection with the African world. Even in these remote times the perpetual movement of cultural influence and change seems obvious, and thus, from prehistoric times onward, the complete isolation of any culture seems to be illusory. Of course, when a high civilization appears and spreads, it seems so powerful and unique that it gives the impression of having emerged by itself. But as soon as we come to its roots and basic elements, the picture changes.

The Egyptian civilization arose around the year 3000 B.C. From time immemorial the two lands of the Nile valley, Upper and Lower Egypt, were conscious of their distinctness and were traditionally competitive. It was the person of the Pharaoh who united them into a single nation. The God-king became the incorporation of authority and responsibility for both countries. While Upper Egypt was tied to the desert and to Africa, Lower Egypt faced the Mediterranean Sea and Asia. Mesopotamian influence, however, reached all of Egypt.[5]

To cite just one instance of the ideas common to Egypt and southwestern Asia, let me quote a passage from one of the Pyramid Texts:

> "Thou are welcome in peace, oh N.N.,
> Thou art welcome by your Father,
> Thou art welcome in peace, oh N.N.
> Thou art welcome by your Son.
> Opened are the porches of Heaven,
> Opened are the porches of the starry Sky."[6]

The text will sound quite familiar to many of you because the wording resembles that of the Psalms.

The leading idea of Egyptian civilization finds its outstanding and unchangeable expression in its cosmology. Here, symbolism and belief are worked out to a rich religious and philosophical system which, later on, influenced the Greeks and Romans. We know that Plato went to Egypt in order to be initiated into the beliefs and mysteries of this life and the life hereafter. Many ideas later found in New Platonism[7] and even in medieval Scholasticism appear for the first time in ancient Egypt. Here it was, actually, that for the first time in the history of the world, thought became philosophically formulated and written down for the generations and civilizations to come.

The principal subjects for the co-operation of ethnology and Egyptology can be summarized as follows:

First: Ethnological topics of world-wide importance and their manifestations in Egypt.

Second: The spread of Egyptian cultural elements into Africa.

Among the ethnological questions of world-wide importance the megalithic problem deserves special attention, not, of course, in the sense of Elliot Smith and Perry.

Even though megalithic monuments[8] in the strict sense are exceedingly rare in Egypt so that we can be sure that megalithism did not originate there, Egyptian culture seems to have absorbed strong megalithic influences which, however, were immediately transformed and raised to a higher level. Many of the traits which, according to Professor Heine-Geldern, are characteristic of the megalithic complex, can be recognized in ancient Egypt, including even the forked wooden posts so frequent in the megalithic cultures of Asia and Africa. In Egypt their presence is definitely proved for the middle of the 2nd millennium B.C. This may eventually provide an important chronological clue to the age of African megalithic cultures in general.

We come to the second point: Egypt's position as an intermediary for cultural elements beyond its borders. Here it is particularly important to distinguish carefully between trends belonging to Near Eastern ancient civilization in general and typical Egyptian features.

Within the limits of this paper I can cite only a few instances of Egyptian influence in Africa.

Let us turn to mythology first. Among the myths of creation, many themes from Egypt seem to have spread into Africa. So the Nuer believe that the world-mother sprang from a tree like the Egyptian goddess Hathor. The Yukun of the Central Sudan believe in a world-mother who created the world by lifting up the sky from the earth just like the Egyptian air-god Shū. The celestial cow-goddess in Egypt finds an equivalent among the Shilluk who believe that the primordial cow created the first man by spitting him out of its mouth.

The mother-son legend among the Bachama of the Central Sudan corresponds to the Isis-Horus complex in ancient Egypt. The Egyptian cosmology of Hermopolis knows four couples of primeval gods, the male gods being frog-headed and the goddesses snake-headed. We meet the same reptiles in the mythology of the Baulé where they represent primordial beings. The best-known example for Egyptian influence in Africa is, of course, the complex connected with divine Kingship.[9] We can find it right across the Sudan from Abyssinia to the Atlantic Ocean and from Uganda and Nyoro to Shonaland.

The leading idea in all these kingdoms is the same—the divine king being the earthly appearance of God, the entire creation depending on him. He is supposed to marry an equivalent woman; therefore the king's sister ranks first as such. In this connection the complex of brother-and-sister marriage[10] has to be taken into consideration. On the basis of a careful analysis of texts concerning those Egyptian women who used to be described as brothers' wives I came to the conclusion that in ancient Egypt not a single case could be proved of a full brother-and-sister marriage. It was always a question of either half-brothers and half-sisters or even cousins. The institution of the so-called brother-and-sister marriage is widely known in Africa, but characteristically only in higher cultures with strongly marked social and economic stratification. We meet it in Darfur, among the Shilluk, in Lunda, among the Fulani, in Dahomey and Monomatapa and many other countries.

Just as in ancient Egypt, in the above-mentioned areas of Africa a similar prevalent position is granted to the Queen Mother, the King's Wife and the King's Sister—a typical sign for a certain influence of mother-right.

Recent research is coming more and more to the conclusion that these and many other instances of Egyptian influence were transmitted to the southern parts of Africa mainly through the ancient Nubian kingdoms of Meroe and Napata from where they spread either along the Nile toward the south or from the eastern Sudan by way of Darfur to West Africa.

The study of the survival of ancient Egyptian customs in the living folk-culture of Egypt is another important task in which ethnologists and Egyptologists will have to cooperate. This has repeatedly been pointed out by a number of authors. The fact that Egyptian ethnologists, with their comprehensive knowledge of the folklore of their own country, are at present taking particular interest in this subject will no doubt yield important results. For instance, present funerary rites and customs[11] resemble the ancient Egyptian in many ways: the ancient Egyptian Ka-priest who recited the prescribed text, has his equivalent in the present religious man who recites the Koran. The ancient Egyptian wailing women are replaced by professional women who cry for the dead. The body of the dead was rubbed with hennah among the ancient Egyptians; the modern Egyptians colour their faces with indigo and put dust and mud on their heads. It is extraordinary that even the provision of the dead with food is practiced in our days among the Egyptians.

It is certain that the study of an early high civilization in its different stages and in the diffusion of its cultural elements within a wide area will help to work out data important for further ethnological resemblances.[12]

Wien, Austria.

Notes

1. Wilhelm Czermak, Vom grossen Gedanken Aegyptens (Archiv fuer aegyptische Archaeologie, 1 : (10), Vienna, 1938); Gertrud Thausing, Zum Sinn der Pyramiden (Anzeiger der philosophisch-historischen Klasse der Oesterreichischen Akademie der Wissenschaften, Nr. 7, Vienna, 1948).

2. Walter Cline, Notes on Cultural Innovations in Dynastic Egypt (Southwestern Journal of Anthropology, 4 : (1) 1, 1948).

3. Wilhelm Czermak, Der Seth der Hyksoszeit (Mélanges Maspero 1 : 721–738, Cairo, 1935–38); Gertrud Thausing, Der aegyptische Schicksalsbegriff (Mitteilungen des Deutschen Institutes fuer aegyptische Altertumskunde in Kairo VIII, Berlin, 1939); Gertrud Thausing, Religioese Revolution im alten Aegypten (Wissenschaft und Weltbild 3 : 9, Vienna, 1950); John A. Wilson, in Frankfort, Wilson, Jacobsen: Before Philosophy, pp. 110–111, Chicago, 1946).

4. "Wiener Totenbuchkommission" with Wilhelm Czermak as Chairman. Wilhelm Czermak, Aegypten und das übrige Afrika (Koloniale Voelkerkunde. Tagungsband I der Beitraege zur Kolonialforschung, p. 110, Berlin, 1943).

5. Cf. Henri Frankfort, The Birth of Civilization in the Near East, pp. 101, 109. Bloomington, 1951.

6. Pyramid Text 412, Resurrection Text. Cf. Henri Frankfort and Mrs. H. A. Frankfort, Before Philosophy, p. 241.

7. Gertrud Thausing, Die Philosophie der Aegypter (not yet printed).

8. Anna Hohenwart-Gerlachstein, Some Problems of Megalithic Culture in Ancient Egypt (Wiener voelkerkundliche Mitteilungen 2 : (2) 126–131, 1954).

Robert Heine-Geldern, On Megalithic Cultures (not yet printed).

9. Paul Hadfield, Traits of Divine Kingship in Africa (p. v, London, 1949); Gertrud Thausing, Altaegyptisches religioeses Gedankengut im heutigen Afrika (Wiener Beitraege zur Kulturgeschichte und Linguistik V, Vienna, 1943); cf. Henri Frankfort, Kingship and the Gods (Chicago, 1948).

10. Anna Hohenwart-Gerlachstein, Zur Geschwisterehe im alten Aegypten und in Afrika (Wiener Beitraege zur Kulturgeschichte und Linguistik IX, Vienna, 1952).

11. Mohamed Riad: Funerary rites and customs among Modern Egyptians (Wiener voelkerkundliche Mitteilungen, 23 (2) 112–118, 1954).

12. Anna Hohenwart-Gerlachstein: Hochkultur und Ethnologie (Die Wiener Schule der Voelkerkunde. Festschrift zum 25-jaehrigen Bestand des Institutes fuer Voelkerkunde 1929–1954, pp. 101–110, Vienna, 1956).

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RIVALRY AND SUPERIORITY: TWO DOMINANT FEATURES OF THE SUMERIAN CULTURE PATTERN

Samuel Noah Kramer

It has long been my conviction that the ancient literate cultures brought to light in the course of the past hundred years or so by the combined efforts of the archaeologist and the philologist can be fruitfully analyzed and significantly interpreted from the point of view of comparative cultural anthropology. This is particularly true of the psychological aspects of their behavioral pattern: the motives, drives, values, aspirations, attitudes and beliefs which determined their world-view and their way of life. To be sure, the source material at our disposal is of a different character than that to which the cultural anthropologist is accustomed. In the case of the ancient literate cultures, our information derives from the "dead letter" rather than from the live informant. But, given an adequate quantity of written records together with reasonably sound translations, the ancient literary sources can to no little extent be made "to talk" as informatively and authoritatively as the living non-literate native. Especially is this true of the Sumerian culture which thrived in the third millennium B.C. in the southerly half of the country now known as Iraq.

As no doubt all of you are aware, only a century ago nothing at all was known of the Sumerians and their civilization; there was no recognizable trace either of the land Sumer or of the Sumerian people in the entire literature available to the scholars and archaeologists who some hundred years ago began excavating in Mesopotamia in search of the Assyrians and Babylonians. Today, as a result of the excavation of a number of significant Sumerian sites, the Sumerians are one of the best-known peoples of the ancient Near East. We now have a fair cross-section of their material culture: their public buildings and private homes, their architectural devices and building techniques, their tools, utensils, weapons, and all the varied products of their arts and crafts. In addition, there have been excavated in Sumer hundreds of thousands of clay tablets, inscribed with their economic, legal and administrative records. And though only a small proportion of these have been published to date, and in spite of the terse and ledger-like form of their contents, they do give a fair, though far from complete, picture of the Sumerian economic structure and social organization. One highly significant feature of many of these economic and administrative documents is the "date formula" which records one of the important events of the year to which the document is dated. The contents of these date formulae, together with those of numerous votive and commemorative inscriptions, and of a few precious historiographic documents, make possible the reconstruction, at least to some extent, of the Sumerian historical background. Finally—and from the point of view of cultural anthropology, these are the most significant—there have been excavated a considerable number of tablets and fragments inscribed with Sumerian literary works: myths and epic tales, hymns and laments, proverbs and essays. It is the texts of these documents which, once reconstructed and translated, will reveal the ideas and ideals, the drives and motivations, the attitudes and beliefs—in short, all of the behavioral traits and characteristics—which helped to make the Sumerian culture function as it did.

The present paper is an initial attempt to utilize the information furnished by some of these Sumerian literary documents in order to isolate and describe one of the motivating forces of Sumerian behavior, one which, unless I am very much mistaken, played a major rôle in the growth and development of Sumerian civilization—the drive for superiority and preëminence.

The idea that the will to superiority, the driving ambition for victory over a rival, was a predominant source of motivation in Sumerian behavior came to me first in the course of relatively recent attempts to piece together and translate a group of Sumerian poems and essays which the ancient scribes themselves categorized as "contests" or "disputations." The major ingredient of these literary debates consists of an argument between two rivals, in the course of which each of the opponents "talks up" his own importance in glowing terms without shame or inhibition, and "talks down" his opponent with sneers and scorn. The protagonists of these disputations are usually personifications of obviously contrasting pairs of seasons, animals, plants, metals, implements, and occupations, such as Summer and Winter, Cattle and Grain, Bird and Fish, Tree and Reed, Copper and Silver, Farmer and Shepherd. To cite just a few typical examples of the style used in these "contest" dialogues, here is one of the more intelligible portions of a speech addressed by Copper to Silver in the "Copper–Silver debate":

> "Silver, only in the palace do you find a station, that's the place to which you are assigned. If there were no palace, you would have no station; gone would be your dwelling-place. . . . (*Four lines unintelligible*) . . . In the (ordinary) home, you are buried away in its darkest spots, its graves, its 'places of escape' (from this world). When irrigation time comes, you don't supply man with the stubble-loosening copper mattock; that's why nobody pays any attention to you! When planting time comes, you don't supply man with the plough-fashioning copper adze; that's why nobody pays any attention to you! When winter comes, you don't supply man with the firewood-cutting copper axe; that's why nobody pays any attention to you! When the harvest time comes, you don't supply man with the grain-cutting copper sickle; that's why nobody pays any attention to you! . . . (*Four lines unintelligible*) . . . Silver, if there were no palace, you would have neither station nor dwelling-place; only the grave, the 'place of escape,' would be your station. Silver, if it were not for these places, you would have no place to be assigned to! . . . (*One-and-a-half lines unintelligible*) . . . Like a god you don't put your hand to any (useful) work. How dare you then to assail(?) me like a wolf(?)? Get into your dark shrines(?); lie down in your graves!"

Thus ends Copper's speech. The author then continues:

> The taunts which Mighty Copper had hurled against him made him (Silver) feel wretched; the taunts filled with shame(?) and bitterness made him smart(?) and wince(?) like water from a salty well. . . . (*One line unintelligible*) . . . Then did Silver give the retort to Mighty Copper: . . .

(There follows Silver's bitter address to Copper, much of which is unintelligible at the moment.) . . .

Or, to take a passage from the "Dispute between Summer and Winter":

> Then did Summer give the retort to Winter who had hurled taunts against him: "Winter, don't brag about your extraordinary strength! I know your lair(?). Let me tell where you 'hole up' in the city; you cannot find enough cover(?). You are a sickly(?) fellow, and weak-kneed! The fire-place(?), the very edge of the fire, the oven, that's your mountain(?)! Your shepherds and herdsmen with (their) heavy (flocks of) ewes and lambs, the weak-kneed fellows, run before you like sheep from fire-place(?) to oven, and from oven to fire-place(?). During the height of the storm you sentence them to constant coughing(?). Because of you, the city people set up a constant chattering of teeth. During the water-drenched(?) days, no one walks the streets. The slave rejoices with the fire-place(?), and spends his days inside the house. The slave-girl does not go out into the downpour, and spends her time with clothes. During the winter, the fields are not worked, their furrows are not attended to. . . . (*Three lines unintelligible*) . . . Don't you boast of your extraordinary strength; let me keep you straight on the rules and regulations (which govern you)!"

Finally here is a sample of a bragging speech by the shepherd-god Dumuzi, whose plea for marriage has just been rejected by the goddess Inanna in favour of the farmer-god Enkimdu:

"The farmer (more) than I, the farmer (more) than I,
the farmer, what has he more (than I)?
Enkimdu, the man of dike, ditch and plow,
(More) than I, the farmer; what has he more (than I)?
Should he give me his black garment,
I would give him, the farmer, my black ewe for it!
Should he give me his white garment,
I would give him, the farmer, my white ewe for it!
Should he pour me his prime beer,
I would pour him, the farmer, my yellow milk for it!
Should he pour me his good beer,
I would pour him, the farmer, my *kisim*-milk for it!
Should he pour me his seductive beer,
I would pour him, the farmer, my . . . (?)-milk for it!
Should he pour me his diluted beer,
I would pour him, the farmer, my plant-milk for it!
Should he give me his good portions(?),
I would give him, the farmer, my *itirda*-milk for them!
Should he give me his good bread,
I would give him, the farmer, my honey-cheese for it!
Should he give me his small beans,
I would give him, the farmer, my small cheeses for them!
After I shall have eaten, shall have drunk,
I would leave for him the extra cream,
I would leave for him the extra milk!
More than I, the farmer, what has he more than I?"

The competitive drive for superiority and preëminence played a large rôle in Sumerian formal education which entailed many years of school attendance and study. Together with the whip and the cane, it was consciously utilized by both parents and teachers to make the student exert himself to the utmost to master the complicated but far from exciting curriculum in order to become a successful scribe and a learned scholar. Here, for example, is a passage taken from an essay on the daily life of a Sumerian school-boy, in which a teacher reassures, in semi-poetic words, an ambitious and aspiring student whose father had just lavished upon him a number of gifts:

> "Young man, because you did not neglect my word, did not forsake it, may you reach the pinnacle of the scribal art, may you become perfect in it! . . . (*Five lines omitted*) . . . You will be the leader of your brothers; you will be the chief of your friends; you will rank as the highest of the school-men!"

That rivalry and competition were rampant in the Sumerian school is attested by another essay, the text of which is only now in the process of reconstruction and translation. This essay consists largely of a disputation or verbal contest between two scribal aspirants actually mentioned by name: Enki-mansi and Girni-ishag, each of whom takes turns in extolling his own scribal competency and belittling that of his opponent. Now there is little likelihood that these two scribal competitors had actually existed in real life, in spite of the naming of names. On the other hand, there is no reason to doubt that the essay reflects faithfully the keen competition and bitter rivalry which marked Sumerian school life.

This particular essay ends in a sentence which prompts a rather startling, but not illuminating, conjecture concerning another important facet of Sumerian culture, the emphasis on law and legality, the penchant for compiling law-codes and writing legal documents, which has long been recognized to have been a predominant feature of Sumerian economic and social life. This sentence reads:

> In the dispute between Enki-mansi and Girni-ishag, the teacher gives the verdict.

The Sumerian word here used for "verdict" is the same term used for verdicts at court trials, and one cannot hold back the thought that the extraordinary importance which the Sumerians attached to law and legal controls is due, at least in part, to the contentions and the aggressive behavioural pattern which characterized their culture.

Turning to the political scene, we now have at least two epic-tales celebrating the victory of the head of the Sumerian city-state of Erech over a presumptuous rival who ruled the city-state of Aratta, which was situated not in Sumer but probably in northwestern Iran. To judge from the contents of these two poems, it was the driving ambition of each of these rulers to break down the morale of his rival by a kind of "war of nerves" and thus make submissive vassals of him and his subjects. The tales are replete with taunts and threats carried back and forth by messengers and heralds, as well as with challenges and contests involving highly tempting mutual gifts, spell-binding sceptres, fights between champions, and struggles between magicians. Finally it is Enmerkar, the "lord" of Erech, who emerges as victor, and to whom, according to one of the poems, his defeated rival, the "lord" of Aratta offers his abject submission in these rather revealing words:

"You are the beloved of Inanna, you alone are exalted
Inanna has truly chosen you for her holy lap;
From the lower (lands) to the upper (lands) you are their lord;
I am second to you,
From the (moment of) conception, I was not your equal,
you are the 'big brother,'
I cannot compare with you ever!"

That the bitter rivalry between city-states and their rulers depicted in these two poems is not a mere literary motif, but a genuine reflection of prevalent conditions, is attested by what has long been known of the turbulent political history of Sumer, which was marked by frequent and disastrous struggles for mastery and supremacy between such states as Kish, Erech, Ur, Lagash and Umma.

By this time it may have occurred to some in this room, as it did to me, that the rather extraordinary Sumerian preoccupation with rivalry, contests, and prestige is reminiscent, to no little extent, of another culture, more than ten thousand miles distant in space and some four to five thousand years removed in time, that of the Kwakiutl Indians of Vancouver Island on the northwest coast of North America. To be sure, the competition for superiority and preëminence did not take the all-pervasive and obsessive turn in the Sumerian culture that it took in that of the Kwakiutl; the Sumerians were neither quite so paranoid in temperament and attitude, nor so seemingly wasteful and destructive in their prestige-motivated practices as were the Kwakiutl. Nevertheless, there is a marked parallelism between this facet of both the Sumerian and Kwakiutl behavioural patterns, one which should prove of no little value for comparative purposes. For example, when I read about the hymns full of inflated and bombastic self-glorification which the Kwakiutl chiefs recited unblushingly and without inhibition at their potlatches, it occurred to me at once that they are paralleled by a type of Sumerian literary composition which has always been somewhat of a psychological enigma: the self-laudatory royal hymn in which the Sumerian king recites his own virtues and achievements in the most extravagant and hyperbolic language. Characteristic, too, of the hymnal lore of both cultures is the constant use of expressions denoting and connoting the idea of "one and only," "the first," "the one who neither has nor brooks a rival." Nor, to judge from the hymns, is the coveted prestige limited to the "here and now"; both the Kwakiutl chief and the Sumerian king sing of far-distant fame and name. The Sumerian hymns, in particular, abound in references to poets, singers and musicians who will never cease glorifying the king and his achievements.

The preceding parts of this paper have tried to present some fairly cogent evidence that the rivalry motive and the drive for superiority deeply coloured the Sumerian general outlook on life, and played an important rôle in their educational, political and economic institutions. All of which suggests the tentative hypothesis that, not unlike the rôle of competition and success in modern American culture, the aggressive penchant for controversy and the ambitious "hankering" for preëminence provided no little of the psychological motivation which sparked and sustained the material and cultural advances for which the Sumerians are not unjustly noted: irrigation expansion, technological invention, and monumental building, and the development of a system of writing. Sad to say, the passion for rivalry and superiority carried within it the seed of self-destruction; it helped to trigger the bloody and disastrous wars between the Sumerian city-states. By seriously impeding the unification of the country as a whole, it exposed Sumer to the external attacks which finally overwhelmed it. All of which provides us with but another historic example of the poignant irony inherent in man and his destiny.

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PLOW COMPLEX, CULTURE CHANGE AND CULTURAL STABILITY

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I

The plow, together with a number of other culture traits, seems to be a good example of what American anthropologists call a "culture complex." In a book which, I assume, is no longer widely used, Wilson D. Wallis (1930: 21) defines the culture complex as a "cluster of traits which function as a unit." Others use similar definitions, e.g. Jacobs and Stern (1947: 300) call it "any functionally integrated . . . cluster of traits . . ." However, it is also used in a somewhat different meaning, e.g. by E. Adamson Hoebel (1949: 498): "An integrated system of culture traits organized about some nuclear interest." Anthropologists frequently have used the horse complex, sociologists the automobile complex, as examples (Gillin 1948: 492; Cuber 1947: 114 f.).

Agriculture necessitates a great number of implements in addition to the plow. According to the older agricultural textbooks, the minimum required equipment before the introduction of modern machinery consisted of the three traditional implements: plow, harrow and roller (Fischer 1919: 27). After the plow has done its work, it is absolutely necessary that the clods be broken up by the harrow. The harrow performs the additional function of pulling the weeds out of the soil which has been loosened by the plow (Fischer 1919: 37). Then, the roller completes the work of crushing the clods; the roller is needed also to pack down the soil in order to fill in the air space left in the lower soil after plowing (Fischer 1919: 39; Manninen 1933: 78, n. 1).

We may confidently state that the harrow and the roller are so indispensable in supplementing the work of the plow that they must follow its introduction just as inevitably as traffic laws and filling stations will follow the introduction of the automobile. In neither case is diffusion necessary. If the nuclear trait, the automobile or the plow, is diffused, the cluster of traits which function with it as a unit must be and will be introduced, i.e. invented if they have not been borrowed together with the nuclear trait.

The only trouble is that this is not the case—the claims of the agriculturists notwithstanding. The statement I have just made is historically wrong as far as the plow complex is concerned. In Egypt the use of the plow can be documented as early as the Old Empire (e.g. Leser 1931: 249 ff.) while the harrow was never used in pre-Roman Egypt (Leser 1931: 492, n. 135, and 541, n. 60; Leser 1928: 439, 480). It was never invented there, although it was needed and all prerequisites for its invention were present. These prerequisites are: domesticated animals able to draw agricultural implements; the knowledge of how to harness animals; the use of implements drawn by animals; and hand implements used by human labor to break the clods and smooth the soil. Throughout the history of old Egypt, the work of the harrow was done by scores of laborers using hand implements, besides the hoe tools probably especially designed for breaking the clods (Leser 1931: 262, 542, 568). Just as the work of scores of laborers digging up and loosening the soil had been superseded by the work of a pair of oxen in front of the plow, so also the work of the laborers who had to break the clods and smooth the soil could have been superseded by the harrow and the roller. To claim that human labor was expendable would be beside the point; the plow, a labor-saving device, *was*, after all, used in Egypt.

In "newly cleared soil . . . still full of humus" which falls "apart almost at a touch" soil pulverization is hardly a problem (van Wagenen 1954: 222). But as soon as the "decrease in the amount of decaying vegetable matter has resulted in clods . . . the need of more efficient cultural implements" becomes paramount. Soil that did not require the harrow and the roller after plowing existed only in areas which had never been plowed in pre-Columbian times, as in America (van Wagenen 1954: 222). Theoretically, Europe, Asia and North Africa could not possibly do without the harrow and the roller. But Egypt did do without them.

Outside of Egypt, the harrow is widely used. The roller, however, seems to be rare. It is quite old, definitely pre-Roman in the Middle East (Leser 1931: 487, n. 110), but seems to have become generally used only in very few areas before recent times. Sigurd Erixon assumes that it was introduced in Sweden during the transition period from the Middle Ages to the modern era (1935: 315 f.; see also Jirlow 1936: 14). Although it seems to have been widely used in Scandinavia as early as the 16th century, it remained unknown in some districts of Denmark even in the beginning of the 19th century (Jirlow 1936: 14). In Estonia, it was very rare in the last part of the 18th century (Manninen 77 f.); in Germany, until the beginning of the 19th century (Leser 1928: 429). Also in other parts of Europe where it was not unknown, it was still not accepted generally.

Thus we are faced with the fact that in some parts of Europe the work of the plow was not complemented by the work of the roller through a period of at least 3000 years, from 1500 B.C. to 1500 A.D. (using time estimates which may be called extremely cautious and utterly conservative).

Plow, harrow and roller, then, do not constitute a "complex" in the American sense of the word. On the other hand, they and a goodly number of other traits seem to constitute a "complex" in the sense in which the concept is used in continental anthropology.

II

It seems to me that implements like the harrow and the roller belong to technology and to that part of culture for which Kroeber introduced the term "reality culture" (1952: 157; 1955: 301). In a paper read before the American Anthropological Association (Patai 1954) and in an article in the *Middle East Journal*, Patai (1955: 12 f.) has used Middle Eastern material to test the hypothesis that "the reality ingredients of a culture can be lent and borrowed much more readily than its value ingredients" and that "very often there is a lag in the acceptance of value culture, while reality culture is accepted readily." In the case of Estonia and Germany, there was a lag in the acceptance of the roller during centuries in which value ingredients were borrowed readily from other cultures. In the same areas there was a similar lag in the acceptance of a considerable number of other reality ingredients. The cases of the roller and the harrow which I have used as examples seem to me to be especially illuminating because the results of the work of these two is immediately convincing. Any farmer who ever saw a harrow or a roller in operation and observed their effects, not to mention a man who used them himself, must realize their importance. But although their technological merit was obvious and they were badly needed, they were not accepted. Even where they were well known from neighboring countries or from neighboring farmers they were not taken over. We are in these cases confronted with a strong resistance to technological ingredients, the advantages of which were overwhelming and convincing.

On the other hand, there are certain technological changes in agriculture which seem to have been brought about by a change in value culture, not by technological superiority. I am referring to the incredible story of the threshing drag-board and the threshing roller in Sweden.

Throughout the Middle Ages and up to approximately 1700, threshing was done in Central and Northern Europe nearly exclusively by means of the flail. In the Mediterranean area other methods were used. Each method was suitable for its area. In Central and Northern Europe where straw was indispensable for the home and for the stable, the advantage of the flail was that the straw was not destroyed by the threshing process. The use of the flail involved work which was hard but in a cool or a cold climate by no means unpleasant (van Wagenen 242). I have, in my youth, spent many hours in fall or winter swinging the flail, and these are no unhappy memories. The very idea, however, of being forced to swing the flail on a threshing floor in the open, in the merciless heat of a summer day in North Africa is a terrifying one. There, where the use of the flail would have been unbearable, it was not necessary because the straw was not needed.

During the 18th century, a large part of Europe was fond of experimenting with agricultural innovations. In Sweden, at that time, threshing rollers were introduced and the threshing drag-board was imported from Bulgaria. While either kind of threshing implement was very useful in the southern climate and for the general conditions of Mediterranean economy, they were quite unsuitable for the north. Still the threshing roller was accepted readily in Scandinavia. Its acceptance was due, it seems to me, not to technological superiority or even usefulness, but to a change in the value culture.

The prestige which China enjoyed in Europe during the 18th century can be compared only with that which European and American technology enjoyed in the 19th century in many of the so-called "underdeveloped" countries. It is impossible to understand the 18th century without realizing the magnitude of the subservient willingness of Europe to imitate foreign cultures in general and Chinese culture in particular. China was praised in every respect. Chinese government and economic theory were idolized. Chinese art was adored. Chinese philosophy was worshipped. Chinese technological superiority was glorified. The Chinese way of life was admired and imitated even to such intimate matters as emotions and sex (Reichwein, passim; Leser 1931: 449 f.). This adoration resulted in a conscious effort to copy and to take over Chinese achievements; and this, in turn, led to the willingness to adopt anything that was not European. Especially in agriculture, no era ever was so eager to experiment with foreign ideas and to accept foreign techniques.

It was this attitude that paved the way for the European agricultural revolution. It was this attitude that led a Swedish reformer who had lived as a diplomat in Turkey to pay for the travel of a Bulgarian farmer and his equipment to Sweden in 1750 and to have him demonstrate the use of the threshing drag-board which was (and is to this very day) so widely used in the Middle East

(Leser 1928: 425, 443 f.; Berg 1931a: 169–173; 1932: 110–111). We are, in this instance, faced with a case of diffusion which is documented by historical sources.

The case of the threshing roller is not so clear (Trotzig 1943: 152). From 1730 on, or even from an earlier date (Trotzig 159), Swedish reformers invented several kinds of threshing rollers apparently without directly imitating or importing similar instruments from those areas where rollers were used for threshing (Berg 1931a: 188; Trotzig 152–164). I have to confess that I am not absolutely certain that foreign rollers really were not the direct models for these Scandinavian inventions. After all, we know not only the instance of the Bulgarian farmer being brought to Sweden; Chinese winnowing machines likewise were brought there (Berg 1928; Berg 1931b: 2 f.; Berg 1932: 113 f.; Jirlow 1936: 25–32). It seems possible, even probable to me, that rollers too were directly imported or at least that travelers who had seen them in other countries described them to the reformers. Gösta Berg (1931a: 188 f.; 1932: 112) assumes that the stimulus to the invention came from the Bible, not from a contemporary culture. He may be right, and in this case we would be faced with what Kroeber (1952: 344 ff.) calls "stimulus diffusion." But the Bible was just as widely read and as well known before 1730. Yet before that time the spark did not ignite. Anyhow, whether it was stimulus diffusion or direct import, the threshing roller in Sweden goes back to some non-Swedish prototype. Its appearance in Sweden was due to the eagerness to reform the traditional type of agriculture, the fervor to make any kind of changes, and the ardor to borrow anything that smacked of the exotic—traits characteristic of the century (Trotzig 163). The diffusionist zeal, the passionate enthusiasm for foreign cultures, especially for China, was something new in Europe. It was a change in value culture. First Chinese influences changed European value culture. Only then did the reality culture, in the case which we are discussing—agricultural techniques and implements—change.

One of the Swedish threshing rollers (Berg 1931a: 185 and 189; Trotzig 155a) seems to me to be almost identical with the so-called "Wooden Nigger" used in New York State (van Wagenen 240 f.). I do not believe that it was independently invented in this country. It would be equally ridiculous to believe that there has been independent invention or multilinear evolution on every New York farm where such a roller was used or in every Swedish province where it existed (see map, Trotzig 158).

Berg (1931a: 190 f.) and Trotzig (1943: 163) think that the threshing roller offered advantages such as the saving of manpower. But so did the threshing drag-board which was not accepted in Sweden. The disadvantages of either, primarily the loss of straw, seem to me to have been more important than the advantages. Where threshing can be done in the open, as it is in the Mediterranean area, and where straw is not used, threshing rollers and similar instruments are suitable. In Sweden, at least in the case of some of the roller types, an especially built, very long barn was required to permit their use, a considerable outlay of labor and capital (Berg 1931a: 177–188). The attempted introduction of the drag-board and the actual introduction of the threshing roller were not due to necessity, but to a fad; not to a real need, but to a fancy.

The ideal threshing machine, no doubt, was not one of the roller type implements but an axle to which short flails were attached, the axle being turned by animals harnessed to a winch; they moved in a circle outside the barn, while the machine did the threshing inside. This machine saved labor and permitted the preservation of the straw and was superior to both the roller and the simple flail. It was, perhaps following a French precedent, suggested in Sweden as early as 1671—but was not adopted (Erixon 326).

III

This brings us to the last point. The tenet that culture is an integrated whole, and that it functions, seems to have led some anthropologists to the unwarranted assumption that culture is a harmonious whole and that it functions well; "whatever is, is right." The history of agriculture is full of details which show that many things which "were" functioned very poorly, did not satisfy the needs which existed, and were forced upon poor sufferers without necessity. The plow itself is a point in question. It was and *is* to this very day in wide use even in soils where its effects are plainly detrimental (Faulkner, passim). The modern plow, considered superior to older type plows, never should be used in certain climates and on certain soils. Still the Germans introduced it at the beginning of the century in Tanganyika. The results were disastrous (Leser 1931: 432). The sad experience did not prevent a later generation from repeating the mistake. After World War II American plows were exported in large numbers to Greece and India, areas where that type of plow does harm, not good.

The material I have presented leads to the following conclusions: sometimes culture elements for which there is a definite need are neither invented nor diffused and even not accepted where they are known. On the other hand, sometimes culture elements are accepted which are not better but poorer than the elements which they replace.

Frank Lynch (1955) has told us that Sol Tax prescribes from time to time for his students a heavy dose of Malinowski. That's good, I think, and I do it also. But perhaps it would widen the horizon of some of our students if we would prescribe once in a while a heavy dose of Spitteler.

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THREE HUNDRED YEARS OF CHUKCHI ETHNIC IDENTITY

Dorothy Libby

Throughout the three centuries of recorded contacts with Europeans, the Chukchi native inhabitants of northeastern Siberia have been reported as proud, self-confident people, who felt themselves and their way of living to be different from those of other peoples. I have considered here the justification for this feeling of ethnic identity by examining the durability of various elements of Chukchi culture and the uniqueness of them.

Information on the Chukchi begins in the middle of the seventeenth century, although it is not until the end of the eighteenth century that there are more than brief statements on them. This information varies in scope, dependability and in quantity over the years, from the meagre statements by early traders, Cossacks, or minor officials, most of whose reports are available to us through the later collections and publications of Pallas and Müller;[1] casual observations in the writings of adventurers, traders, and whalers;[2] more weighty reports and notes of explorers and government officials like Merck, Vrangell, Maydell, and Sverdrup;[3] and the writings of Bogoraz[4] and of Soviet investigators and officials like Gurvich, Semushkin, Sergeev, and Vdovin,[5] to name only a few.

From such sources which cover many aspects of Chukchi life, a listing can be made of some elements in Chukchi culture which were present in the earliest comparatively full description by Merck from his trip in 1791, and some of the reports of Pallas and Müller, and which Soviet writers still mention as being present, at least in the 1930's and sometimes in later years. Some of these elements are the following: their location in the northeastern part of Asia; their division into two basic economies—the settled maritime, sea-mammal hunters, and the wandering reindeer breeders and hunters, together with their general cycle of existence based on these two modes of life; their dependence on animals for food, shelter, clothing, and various tools and implements; their language, with its different pronunciation of certain sounds by men and women; some of their beliefs in shamanism and in spirits; some of their attitudes toward the natural world; some of their ceremonies; their attitude toward death; their interest in trade activity; their interest in practical elements from foreign areas; their lack of much social organization beyond the family group and kinship ties; their practice of sharing their wives with certain friends; their strongly independent character; their pride in being Chukchi and feeling that the Chukchi are a separate people.

The continuity of such a number of important cultural elements has made descriptions of Chukchi culture seem much the same from one period to another. However, Chukchi culture has been receptive to influences from neighboring peoples, so that although the general pattern of the culture has remained for these three centuries, the content of even the more stable elements has changed.

I shall now describe some of the changes in the stable elements just cited. Chukchi interest in trade has long been noted.[6] Since the main impetus for the

Russian expansion to the east in the seventeenth century was their search for furs, this is not surprising. It is interesting, however, to see how the Chukchi reacted to this demand by the Europeans and expanded their trading activities. By 1791 Merck[7] could describe the extensive trade in furs and European goods carried on by Chukchi middlemen and note that many of the furs which they traded at the Russian fairs in the Kolyma or the Anadyr river areas had been gotten by them from American natives. This inter-continental trade had reached such economic importance that when Zagoskin made his survey of the country around the mouth of the Yukon River in 1842–44 for the Russian American Company he was given special instructions to determine where company posts could be established to attract these furs which were being traded to native middlemen and going to the trade fairs in Asia.[8] These native patterns of trade proved quite enduring, though their importance diminished considerably after the middle of the eighteenth century when whalers and traders appeared on the northern Pacific coasts and as more trading points were set up in the interior of both North America and Asia.[9] Inter-continental contacts and trade continued at least until 1926,[10] and probably sporadically up to post war times,[11] though independent Chukchi traders have been considered hostile elements by the Soviets.[12]

In the realm of ideas, attitudes, and beliefs there is less obvious change, but even here European elements have been incorporated into Chukchi patterns of thought. For example, Merck observed that the Chukchi defense against illness and disease was the practice of shamanism, with drumming, songs, and the use of formulas, of sacrifices to the spirits, or of shunning contact with sick persons.[13] The Soviets report the presence of the same practices in the 1930's in their discussions of their efforts to introduce more modern medical practices into the area.[14] Merck at the end of the eighteenth century describes the efforts of a father to try to cure his boy through the sacrifice of a reindeer and the purifying effect of their household fire in much the same terms that Bogoraz used in the first decade of the twentieth century.[15] According to Bogoraz, diseases introduced by contact with the Europeans were visualized in the same way and were thought of as specially virulent kinds of evil spirits of the same type as their former spirits of sickness and were treated in the same way. Syphilis was visualized as a small red spirit, driving small red reindeer and stopping to camp in the red cloud berries, ever hunting for human prey.[16]

Sacrifices to the sea, various spirits, or to the sun to propitiate them and to make a journey or other undertaking successful are reported from the earliest descriptions.[17] Bogoraz describes the same kinds of sacrifices and notes that the spirits were considered to especially like alien foods, liquor and tobacco. He also describes a ceremony of this kind held over the furs the Chukchi were about to take to the fair for sale.[18]

Lavrov, a Soviet investigator, describes in 1947 some drawings by a former Chukchi shaman of a shaman performing various feats, including his ascending through several heavens and dealing with different spirits. His drawings of these feats and of the spirits were still recognized by his fellow Chukchi to be the spirits and events he intended them to be. This shows that at least part of the ideas concerning spirits and shamanism still exist. Indeed he reports more elaborate training and equipment for shamans than Bogoraz did. The same Chukchi shaman also drew the middle-sea-bird, a huge mythological bird which is met with far out from land in stormy and misty weather, swallowing a steamship. The ship would travel through the bird's alimentary canal and come out the anus. This treatment of the steamship is the same as that of native boats recounted in their folk tales.[19]

Persistence of beliefs about spirits is also seen in their mythology. A study made of the tales collected by Bogoraz in 1901–02 showed that despite the contacts with Russians since the middle of the seventeenth century less than five per cent of the Chukchi tales showed Russian influence.[20] More recent collections show much more Russian influence,[21] but these have been made largely around the areas where the native cultural bases have been organized and where the natives have been subject to Soviet education and training. In these later collections, for example, airplanes, and political themes play a part; Lenin and Stalin are compared to the sun, poor oppressed herdsmen decide to join a collective farm and confound their former oppressor, the rich reindeer owner, and so on. However, even in these tales much of the older style and idea content remains.

In the field of ivory carving, too, much of the subject matter remains traditional—animals, hunting scenes, mythological creatures—although some new themes have been introduced in Soviet times. The carving of ivory by the Maritime Chukchi also expanded greatly during the nineteenth century through sales to whalers and traders. These purchasers would often tell the Chukchi the kind of objects that they wanted and sometimes they would show them new techniques. The Soviets have continued to support this art and have introduced new tools and taught new methods to the Chukchi, including realistic portrait engraving, perspective, and flat and bas-relief work.[22]

In the matter of settlement patterns, the Maritime Chukchi have had village sites along the Pacific and Arctic coasts since the times of early contact.[23] These have continued to the present day, and the Soviets have chosen some of them for their own settlement sites and for scientific stations.[24] Many maritime Chukchi have been organized into combines and cooperatives for economic pursuits and now have begun to live in wooden buildings instead of their former semi-subterranean type of house and their tents.[25] Some of the coastal natives had begun to buy wooden structures as early as 1912.[26] Most of the Reindeer Chukchi continued their wandering life, following the movement of their herds at least into the 1940's. Large collective farms have been established now which many of the reindeer people have joined. On these farms there is usually a settlement where women and children may stay, where a school, storehouses, and other buildings are located, and where families may live in wooden houses instead of tents. The herders of reindeer, however, still live in their tents as they follow the reindeer.[27]

Even such a basic item as reindeer breeding has changed considerably. Leaving aside the question of origin of the Chukchi and their original form of culture, it has been shown that reindeer breeding increased in importance for them in the nineteenth century, the herds becoming larger. Maritime Chukchi are reported as owning more reindeer also. At the same time wild reindeer became more scarce and the Chukchi had to depend more on their herds.[28] Also, the Chukchi continued to learn in historic times various aspects of reindeer culture and techniques from their southern and western neighbors.[29] Today instead of herds belonging to individual owners, many of the reindeer are on large collective farms where they are looked after by herdsmen belonging to the farm, but with the advice and help of Soviet technicians and veterinarians.[30] One element that the Soviets introduced quite early was the use of dogs to help in the herding of the reindeer.[31]

Thus, when we consider the more permanent elements in Chukchi culture we see that the Chukchi have not been equally receptive to alien influences in all parts of their culture. What could be seen as practical and useful to them in their way of living has been adopted by them—domestication of reindeer, metal tools, and European foods to tide them over famine periods are some of these. The basic family grouping has been a conservative element, and the rentention of the Chukchi language another. Elements dependent on the natural environment have also been conservative. Other objects (except those with prestige attached to them) and different ideas have had less acceptance. Some of the reasons for this non-acceptance of alien ideas in the past may be that foreigners who did visit them were not obviously on a higher plane of civilization than they. Also, most of them had to depend on Chukchi help and advice to exist in the country.

Chukchi culture, apart from the language, seems to have little that is unique about it. Much of their reindeer economy was derived from their neighbors. Their maritime economy is very similar to that of their Eskimo neighbors. Their mythology was shown by Bogoraz and others to be closely related to that of the northwest coast of North America. Birket-Smith and others have shown the wide distribution of many other of their culture elements in the circumpolar area. From western culture they accepted and incorporated into their way of life methods of trapping, metal tools, pots, implements, guns, matches, motorized whale boats, tobacco, tea, and foods.

There is only a slender basis in the facts presented here to justify Chukchi belief in their uniqueness.

Another result of this study has been the demonstration of the flexibility of the Chukchi, who have adjusted to changing living conditions while retaining, at least until recent times, their sense of cultural separateness. I should like to suggest on the basis of this study, that this feeling of ethnic identity was not dependent on any particular manifestation of Chukchi culture, but that in part it was dependent on their own belief in it. I believe that this may also be the case with other small native groups who have persisted as separate peoples.

Washington, D.C.

Notes

1. Vdovin also quotes and abstracts early documentary sources.
2. E.g., Ashton, Brahe, Cochrane, DeWindt, Niedieck, Swenson.
3. Merck; Maydell; Sverdrup, 1921, 1926; Wrangell, 1839, 1841.
4. Bogoraz, 1904–9.
5. Gurvich, 1952; Semushkin, 1936, 1948; Sergeev, 1947; Vdovin.
6. E.g., Pallas, Vol. I, pp. 245–248, Vol. IV, pp. 105–111; Müller, pp. 6–7, 43, 56–60, 119, 515; Vdovin, passim.
7. Merck, pp. 188–189.
8. Zagoskin, pp. 506–507.
9. E.g., Bogoraz, 1904–9, Chap. 3; Maydell, passim, especially Chaps. 2, 3.
10. Jenness, p. 78, Plate XIB.
11. Kolarz, p. 92.
12. Semushkin, 1948; Sergeev, 1947, pp. 135, 146, 156; Zelenin, 1938, p. 22.
13. Merck, pp. 56–57, 67–69.
14. A., I.; A., V.; Semushkin, 1936.
15. Bogoraz, 1904–9, Chaps. 12–16; Merck, pp. 68–69.
16. Bogoraz, 1904–9, p. 297.
17. Merck, pp. 8, 56–57; Vdovin.

18. Bogoraz, 1904–9, pp. 370, 388, 391.
19. Lavrov.
20. Nikiforov.
21. E.g., Sergeev, 1951, 1952.
22. Antropova; Ivanov; Lavrov.
23. Bogoraz, 1904–9, pp. 1–32; Vdovin.
24. Sergeev; Zelenin.
25. Gurvich; Sergeev.
26. Kallinikov, pp. 178–179.
27. Gulin and Kravchenko; Gurvich; Shamshurin.
28. Bogoraz, 1904–9, pp. 70–97.
29. E.g., Bogoraz, 1904–9, pp. 70–97; Sverdrup, 1921, passim.
30. Sergeev, 1947; Shamshurin.
31. Bogoraz, 1929.
32. Bogoraz, 1902.
33. Birket-Smith, especially Part 2, pp. 234–380.

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NORTH EUROPEAN SHAMANISM

Nils Lid

In giving some features of Nordic magic with special regard to the sorcerer's spiritual habitus and his spiritual journey, my aim is to try to build a basis for interpreting symbolic pictures on the membranes of conjuring drums, and symbolic figures which were traditionally tied to or put in those drums themselves, and to interpret corresponding figures on rock carvings.

Nordic ecstatic magic is mainly preserved in Norse legendary tradition. The sorcerer fell into a trance, and his body was said to pass into other planes of existence—for instance, in the shape of a wolf or a bear. Ecstatic transfiguration before a battle is also linked to certain warriors, called in Old Norse *berserks*. Their condition was characterized by fits of rage, the warriors behaving like the beasts of prey they were supposed to represent. According to the Norse poems of the 9th century, which are contemporary sources of information, these warriors could behave and roar as bears or as wolves, depending upon their disguise and their name, which later was derived from either bear or wolf. In their ecstasy they apparently wore part of, or the whole of, a bear's or a wolf's skin, thus appearing more or less as man-bears and werewolves.

In the legends we have a parallel tradition, corresponding to beliefs in many parts of the world. In a saga there is, for example, a story told about a Viking who was very fierce, fighting in a battle in the shape of a bear, his body lying unconscious in his home at the same time.

This capacity for transfiguration belonged to particular families. Thus it is told about the famous Egil Skallagrimsson, his father and his grandfather, that they all were liable to become spellbound and to receive another shape—in other words, become berserks. From this habit the grandfather got his name "Evening Wolf," *Kveldulf*, as he got his spells in the evening. Many other Old Norse personal names had a similar origin. There are still many Norwegians having the name "Nightwolf," *Nottulv*. Several Old Norse sorcerer's terms and stories show that the sorcery was conducted in the evening or the night.

Regarding these personal names we must go back to an epoch when the person got his name because he was just what the name implied. The meaning of the name was there in an appellative, which gave the personal name its significance.

The habit of becoming transfigured into a beast through ecstatic magic is very similar to the usual shamanism. A conjuring drum with a special name, used as an ecstatic medium in the same way as the well-known Lapp drum, was endemic to the Norwegians, both of them being a special form of the Arctic or Subarctic drum. The Norwegian and the Lapp traditions are often mixed up.

It is related in the oldest history of Norway, written in Latin in the 12th Century, that Lapps used a conjuring drum which contained miniatures of the vehicle needed by the sorcerer for his spiritual journey, the names of them partly given in Norwegian but having Latin diminutive endings. It is here told that the conjuring drum which the sorcerer lifted up in the air during his dance and song was filled by small figures of whales, harnessed reindeer, skis, and a boat with oars. The history continues: "The spirit of the sorcerer should use these vehicles on his journey over snow heights and steep mountain sides or deep seas." It is clear that he is imagined to be in his boat on the sea, driving with reindeer, or running on skis on land. This tallies with his cosmological thoughts of mountains and seas in his other world. But the whale-figures indicate that he also can be in whale's shape on his journey through the sea. It is told in the text that the sorcerer was in a whale's shape in the sea where he was wounded and then killed by other sorcerers' sharp poles, the name indicating that this was a special Norwegian form of sorcery well known in Norway even today.

As is well known, in the Arctic and Subarctic drums there were corresponding figures. The shaman's belief was that this world was his microcosmos, and the other world which he could reach by his spiritual flight was his macrocosmos. The Siberian shaman for instance, as is well known, could perform rites in the tree trunk which was the center pole of his shaman hut before he was transported in trance to another world.

Especially important for the understanding of the vehicular figures in the conjuring drum which I have described is a Norwegian report from Finmark, dated 1767, to the following effect: "the Lapp shaman should shout just before falling in his trance: 'Prepare the reindeer! Launch the boat!'"

The Lapp conjuring drums had more elaborate figures on the membrane than the corresponding Arctic drums, depicting places in this and in the outer or upper world. The membrane was in this way a kind of map of the areas the sorcerer was to pass on his spiritual flight. But some figures depicted subjects connected with his intended spiritual movements. Thus you will find skiers, reindeer, and boats. Figure 1 shows such skiers from various Lapp drums, taken from Dr. Manker's great work on the subject. They should illustrate the symbolic implication very well.

When we turn to the rock carvings we have to remember that there is a long interval of time between what I have been talking about and the Stone Age rock-carvings which date back about 4000 years before our own time, whereas the medieval drum mentioned dates back only 800 years. The skiers in these carvings are also very important, as they are in fact the first fixed points in ski history.

Figure 2 is one of the figures on a carving in Norwegian Nordland county. This figure is a skier. You will recognize him from the curve of his knees and from the whole position. He has something in his hands which looks like an ax or perhaps a ski pole. The horns or long ears on his head are peculiar. Dr. Gjessing, who studied these carvings in the thirties and who himself uncovered this figure which was overgrown, concluded that this skier had his headgear as a magic hunting device. This man is in a downhill position compared with the other figures which are found to the left.

Figure 3 shows the figures in the middle. Here is also a skier in the same position, but something is lacking in the middle, where it is unfinished. To the left are a species of deer, an elk, a whale, and an unfinished boat. That this is a boat will be seen from the last picture (Figure 4) of this carving, where we find a complete one, a seal following the unfinished boat on the other picture, a whale again, and a small figure of a deer.

On the coast of Norway the seal is the most common subject of transfiguration in recent folklore. It is curious to note that the other figures of this carving all correspond to those contained in the conjuring drum which has been described: the deer, the whales, the boats, the skiers. Both in the case of the complete boat and the skis the man is portrayed in the carving as on the membrane of the drums.

The motifs in these carvings must be closely related to the descriptive scenes carved on stone along the coasts of Lake Onega and the White Sea at approximately the same period.

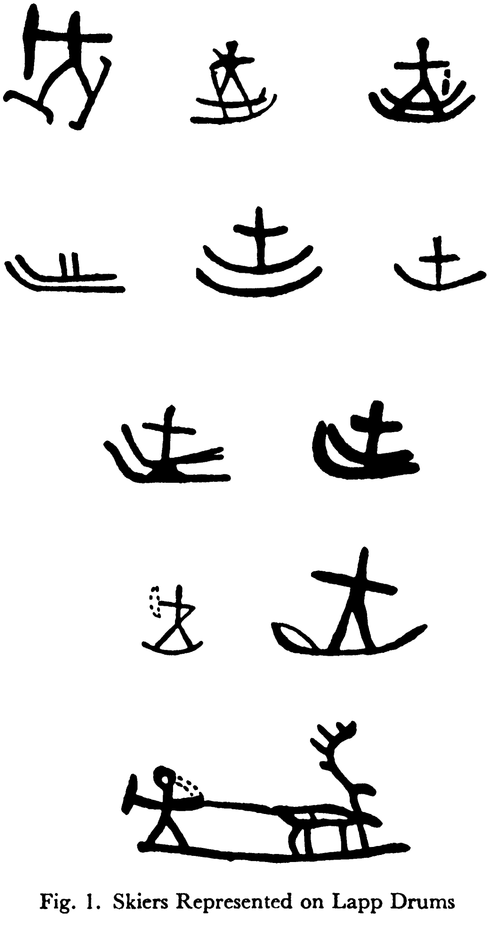

Fig. 1. Skiers Represented on Lapp Drums

Figure 5 depicts a man in the shape of a beast, a bear or a wolf, following on skis after a deer. The man's headgear is characterized by the ear and the gaping mouth. Note the tail. The peculiar thing the man has in his hands could be compared with the corresponding object in the hands of the skiers from north Norway.

These eastern rock carvings, which were described by the Russian archeologist Ravdonikas, have many figures in association with elk and reindeer, which may be interpreted as hunting scenes. Reindeer, elk, and red deer have played an important role in the prehistoric hunting life of northern Europe as a whole.

One of the White Sea carvings (Figure 6) contains nude skiers on a cross-country run, with ski poles. Their faces are peculiar. If you consider all the symbolism in these carvings, and especially all birds depicted there, you may interpret them as birds' faces! A birdlike appearance is quite common in shaman tradition as symbolizing his spiritual flight.

In the Onega carving the important thing is the man's shape of a beast of prey. When the hunter was in such a disguise he had in some way persuaded himself that this was a reality; he imagined himself to have the power of the beast, as did the berserks also. Often the disguise was very naive, as is well-known in different parts of the world. A characteristic Arctic example is given by Knud Rasmussen in his work on the Eskimo of the Coronation Gulf. The sorcerer there induced people to think that he was in a wolf's or bear's shape by letting real wolf's or bear's teeth appear to grow out of his mouth.

The use of skis in the sorcerer's preparation for his spiritual journey may have a special meaning. There is a medieval Norwegian tradition about the ski hero *Heming*, whose famous ski run was down a steep mountain. His name means "He who easily gets into a transfiguration." He was, according to the legend, initiated by the Lapps when he was seven years old. The legend says he then "lost his mind"; in other words, he could attain a state of ecstasy. In the medieval ballads he is given another name when he is himself, a use of double names known also elsewhere in Norse tradition regarding transfiguration.

Reality and imagination are mixed up in the tradition of this ski hero. In studies of this subject I have tried to show that the reason for his ski run is a widespread belief that one will be a good sorcerer or the like often letting himself swoop down from a steep mountain, and in that way acquire a magical ability to fly into another world.

Finally, I want to stress that the legends presented here are explained by the general tendency in folklore and religion for simple information about habits, customs, and rites to develop in the course of time into legends about single events. Cult myths should be, I suppose, the best examples of this.

Oslo, Norway.

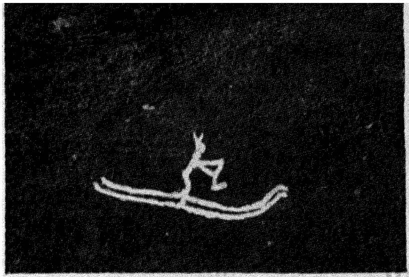

Fig. 2. Rock Carving of Skier

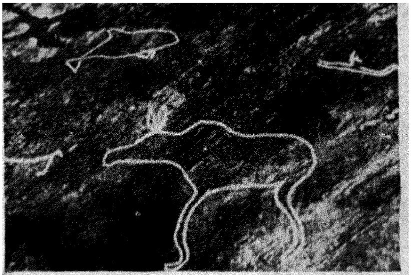

Fig. 3. Rock Carving of Skier and Animals

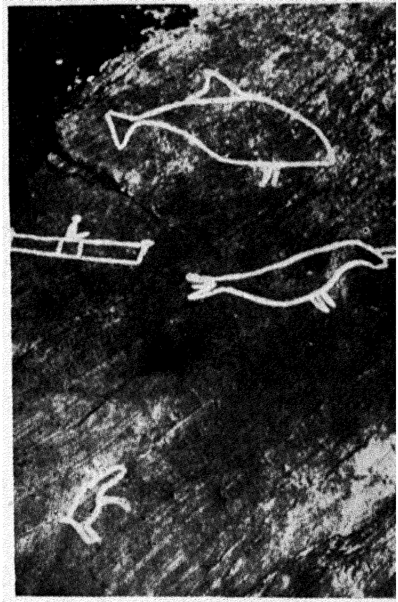

Fig. 4. Rock Carving of Boat and Animals

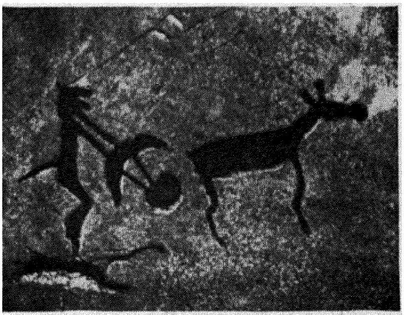

Fig. 5. Rock Carving of Skier Following Deer

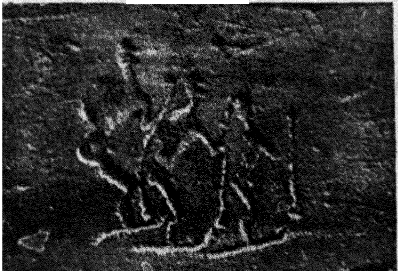

Fig. 6. Rock Carving of Group of Skiers

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THE ASSUMED EARLY MEDITERRANEAN INFLUENCE AMONG THE KUANYAMA AMBO BANTU OF SOUTH WEST AFRICA

Edwin M. Loeb

At present ethnological research uses primarily two complementary methods: one, that of the functional school of England, and the other, that of the cultural historical school of Germany and Austria. The functional school explains cultures on the basis of factual material in a single culture as that culture exists today and attempts to show how the traits in this culture function in relation to one another. Thus, C. D. Gibson,[1] a functionalist, found that among the Herero the effect of the patrilineal system of descent is disjunctive, whereas the effect of the matrilineal system is conjunctive. This fact is said to account for the origin of the dual descent system among the Herero, but only among the Herero, for this school of thought believes that traits in each culture are unique and that features of Herero culture, for example, do not explain and should not be compared with similar traits in Fiji culture, for instance, or in fact in any other culture. This philosophically speaking, is a nominalistic approach to culture since it eschews universals. The method has proved to be useful to government officials and to psychologists. The cultural historical or comparative school of ethnology, first makes certain chronological assumptions about all cultures and then investigates single cultures in order to test the assumptions made. This, philosophically speaking, is the realistic approach to culture because it assumes that cultural factors are entities that can be compared and classified, and that their distribution can be mapped. This school assumes also that, even where dated history is lacking, a scientist can reconstruct historical sequences and events by means of a scientific analysis of the pertinent data concerning plants, animals, and cultural traits, and their distribution.

The cultural historians claim that their assumptions are an aid in approaching the truth, that the validity of such assumptions depends upon the amount of material collected and accurately evaluated and utilized. After an extended cooperative survey by many ethnologists, C. Murdock[2] was able to write: "In all societies with full-fledged double descent the matrilineal kin groups were the first to be evolved, the rule of patrilineal descent representing a secondary development." In accordance with his method, Murdock believes that the same principle holds true for the Ashanti of the Gold Coast, the people of South India, and the Herero of South West Africa. He makes the assumption that the matrilineate or mother-right precedes the patrilineate or father-right. In a paper entitled "The Kuanyama Ambo and Other Tribes of South West Africa,"[3] I have made the assumption that the father-right and cattle cultures diffused together and are later in development than the early planting culture and the mother-right.[4]

The purpose of this paper is to show that certain evidence in regard to the cultural history of Africa may cast new light upon the influences that various features of early Mediterranean culture had upon Africa, and especially upon the Kuanyama Ambo of South West Africa and South Angola; and, on this basis to consider the Ambo culture as a geographically isolated enclave of early Mediterranean survivals. If this purpose is accomplished, we can learn something of our own early civilization and of the forces that molded our present manner of thinking.

Before proceeding to the main discussion it is necessary first to identify the Kuanyama Ambo and also to mention the terminology employed in this paper.

The Kuanyama is the main Ambo, or Ovambo, tribe among the Bantu-speaking peoples of South West Africa and South Angola. All Ambo are related in language, custom, and origin to their southern and western neighbor, the Herero. Both the Ambo and the Herero are patrilocal, but the Herero, like the Ovimbundu of Central Angola, have a dual descent system, while the Ambo and related northern tribes have only matrilineal clans. A twenty-inch average seasonal rainfall and the periodic overflow of northern rivers permit both agriculture and fishing among the Ambo. The Herero, on the other hand, live on semi-desert velds as migratory cattle-raisers: the men herd the cattle, while the women do the milking and gather wild foods. The Ambo also are cattle-raisers, and like the Herero they have sacred and profane cattle; but the Ambo are more settled in habitat than the Herero, and their women cultivate sorghum and millet and take care of the kraal's dogs and chickens. Their male youths tend the sheep, goats, and cattle. Until the end of the nineteenth century, the Kuanyama Ambo had sacred kings.

The terms used in this paper will include *the planting culture*, *grain culture*, *cattle culture*, *the long house*, *the kraal*, *the divine kingship*, *sacred cattle*, *vestal virgins*, *the antiphonal love song*, and *bundling*.[5] Although in original monographs native terminology should be given as well as the generic English terms, nevertheless such descriptive terms as these when adequately defined,[6] as well as those used by Frazer and other comparative ethnologists, have scientific validity both in nomenclature and classification either in or out of cross-cultural context.

This paper will first call attention to five important assumptions and then briefly describe six traits that indicate an early Mediterranean influence on South West African Bantu Culture.

1. *African culture is stratified.* The same stratification is present in Africa as in the remainder of the marginal, or primitive, Old World: that is, one finds the hunters, gatherers, and fishers; the horticulturists and agriculturists; live stock raisers; and urban populations. This stratification originates primarily through acculturation, war, and conquest. Among the Ambo the main stratification is further accentuated by the division of labor between cattle-raising men and agricultural women.

2. *The early Mediterranean influences were carried by cattle-raisers.* With the exception of the original hunting, gathering, and fishing cultures, African cultures south of the Sahara probably did not develop uninfluenced *in situ.* Planting cultures, seed cultures, the domestication of small and then of large domestic animals, the long house and then the round house—including the kraal for human beings and animals—the matrilineate and the patrilineate, and the feudal system—including the divine kingship—all of these probably came via the early Mediterranean World. The top layers of culture migrated with the cattle-raisers.

3. *The present distribution and breeds of African cattle confirm the ethnic migrations into Africa.* Through questionnaires and genetic tests on native cattle, scientists in Africa[7] are able to approximate the periods of domestication and the migra-

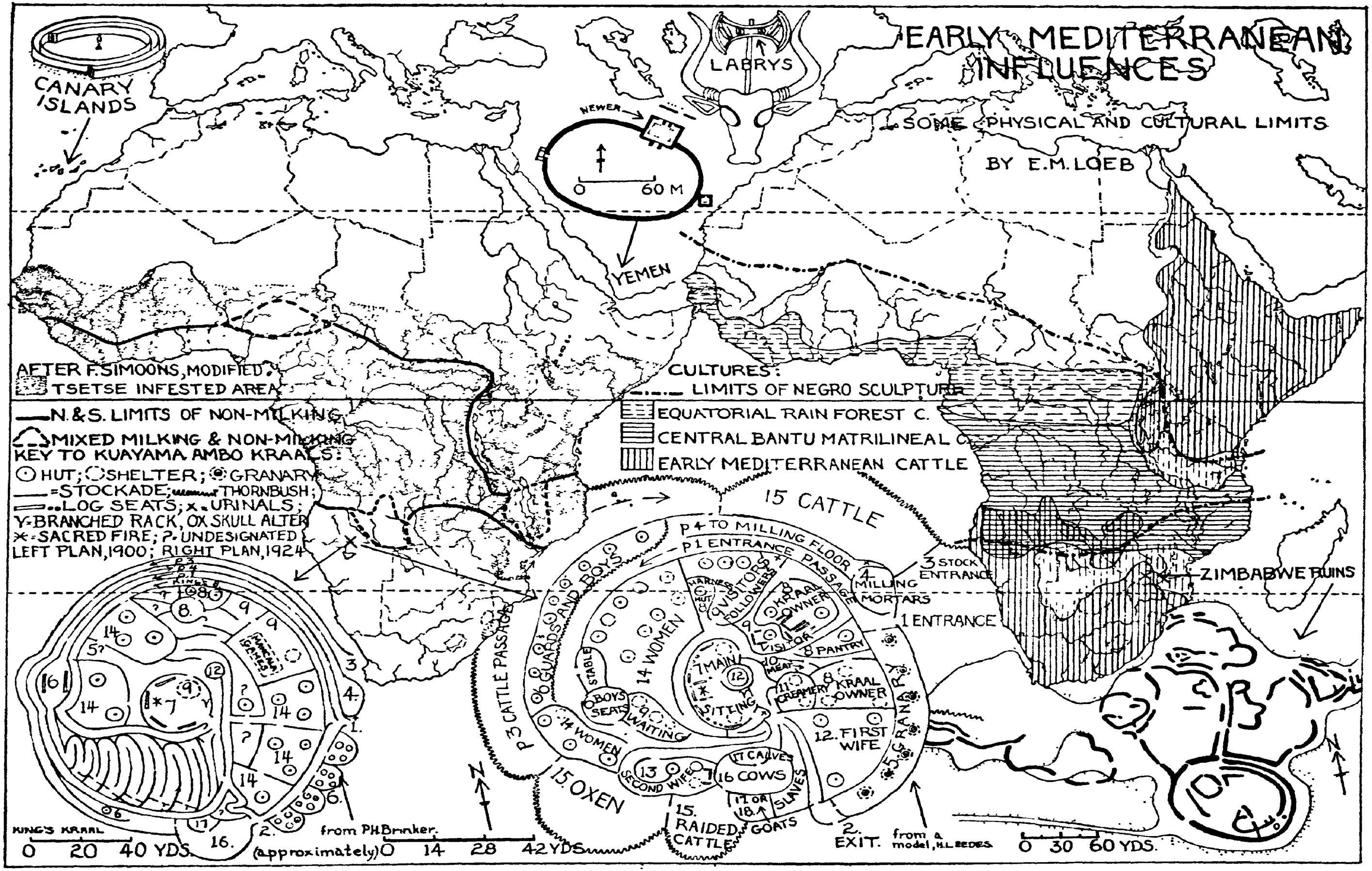
EARLY MEDITERRANEAN INFLUENCES
SOME PHYSICAL AND CULTURAL LIMITS
BY E.M.LOEB
CANARY ISLANDS
LABRYS
NEWER
0 60 M
YEMEN
AFTER F.SIMOONS, MODIFIED.
TSETSE INFESTED AREA
N.&S. LIMITS OF NON-MILKING
MIXED MILKING & NON-MILKING
KEY TO KUAYAMA AMBO KRAALS:
HUT; SHELTER; GRANARY
STOCKADE; THORNBUSH;
LOG SEATS; x. URINALS;
Y-BRANCHED RACK, OX SKULL ALTER
✕-SACRED FIRE; ?. UNDESIGNATED
LEFT PLAN, 1900; RIGHT PLAN, 1924
CULTURES:
LIMITS OF NEGRO SCULPTURE
EQUATORIAL RAIN FOREST C.
CENTRAL BANTU MATRILINEAL C.
EARLY MEDITERRANEAN CATTLE
ZIMBABWE RUINS
15 CATTLE
P4 TO MILLING FLOOR
P1 ENTRANCE PASSAGE
3 STOCK ENTRANCE
4 MILLING MORTARS
1 ENTRANCE
GUARDS AND BOYS
VISITORS FOLLOWERS
KRAAL OWNER
HARNESS
14 WOMEN
7 MAIN
SITTING
10 MEAT
8 PANTRY
11 CREAMERY
8 KRAAL OWNER
12. FIRST WIFE
5 GRANARY
STABLE
BOYS SEATS
9 WAITING
14 WOMEN
13 SECOND WIFE
17 CALVES
16 COWS
SLAVES
17 OR 18 A GOATS
15. RAIDED CATTLE
P3 CATTLE PASSAGE
15 OXEN
2. EXIT.
from a model, H.L.BEDES
0 30 60 YDS.
N
from P.H.Brinker.
(approximately) 0 14 28 42 YDS.
KING'S KRAAL
0 20 40 YDS.
MANCALA GAMES
16.
17
2.
3
4.
1.
6.
14
?
9
8.

Fig. 1. Early Mediterranean Influences in Africa

tion paths of early pastoral peoples into Africa. The Hamitic Longhorn is thought to have been domesticated north of the Sahara in the 4th millennium B.C. It is believed that towards the end of the Neolithic period the Sinai Peninsula was a main migration route through which early Mediterranean peoples brought the short-horned cattle (*Brachyceros*) to Africa. Circa 1,000 B.C. Semites were in Ethiopia with the Long-horned Zebu. Perhaps they crossed into Africa at the Strait of Bab-el-Mandeb. The Ambo cattle are primarily a mixture of the Long-horned Zebu and the Hamitic Longhorn.

4. *A comparison of present African architectural ground plans with archeological sites indicates a probable cultural diffusion by way of South Arabia.* The accompanying illustrations show a similarity between the ground plans of the elliptical temple of Marib in Yemen[8] and the elliptical stone ruin of Zimbabwe in South Rhodesia, and further similarities are evident in the plans of the Ambo kraals of South West Africa. Some parts of the prototype of the almost round, labyrinthic, wood-stockaded kraal of South West Africa, where there is no stone suitable for building, may have first entered East Africa from Arabia, together with Arabian cattle; presumably such a structure did not come from North Africa[9] since the Nilotic tribes lack this feature. Possibly the Yemen temple was constructed by Hamitic (not Semitic) speaking peoples, for the early Hamites of the Canary Islands had a round labyrinth.[10]

In Yemen the early (agricultural) moon cult was displaced in archeological sequence by a cattle-raisers' bull cult.[11] The Kuanyama have both a moon cult and a bull cult. Along the Mediterranean perimeter there is a correlation between cattle-raising, the bull cult, the double-bladed axe and the Sky God cult. The double-bladed axe of Crete is said to be connected with the labyrinth, for in Caria in Asia Minor, the double-bladed axe, a sign of Zeus, was called *labrys*. This implement, however, apparently did not penetrate into Negro Africa.[12] The Kuanyama Ambo also have a cattle complex including cattle-raising, the labyrinth, the bull cult, and the Sky God (Kalunga). Recent studies suggest that the ground plans of the Kuanyama labyrinthic stockaded kraal and the high cultural elements associated with it came through South Rhodesia.[13]

5. *The early Mediterranean cattle-culture traits entered Negro Africa from the north and the east.* Although it is possible to discover the migratory routes of cattle breeds and architecture and other facets of culture it is not, at least as yet, possible to trace the exact path of the following cattle-culture traits: the Sky God with a divine king as his reincarnation; the sacred fire attended by vestal virgins; sacred cattle; an overlay of patrilineal structure on matrilineal structure with the resulting symmetrical cross-cousin marriage; the so-called "bride-price" with its attendant delayed marriage and certain courtship customs correlated with the delayed marriage; the lack of plastic figurine arts; and finally, a peculiar attitude towards twins of different sex. The remainder of this paper will deal with a few of these traits as exhibited by the Kuanyama Ambo and other cattle people under early Mediterranean influence.

1. *The sacred cattle.* The cult of sacred cattle is found throughout the Mediterranean and its sphere of influence, extending into Celtic Europe and down to Nilotic and Bantu Africa. The literature on the subject indicates that cattle were originally domesticated as sacred animals and came into Negro Africa as sacred animals.[14]

Melville Herskovits was one of the first anthropologists to apply the principle of cultural stratification to Africa. He regarded the cattle complex of East

Africa as a cultural layer superimposed on an underlying agricultural complex.[15] Writing on East Africa, he stated that the natives own cattle primarily for power and prestige.[16] However, there was an underlying trait of sacred cattle even in East Africa. Some Xhosa tribes had sacred cattle held by the chief in trust for the tribe and used as sacrificial offerings to their ancestors.[17]

The Kuanyama kings always had their sacred herds, and, as was true among early Mediterranean cattle-raisers, cattle even when not sacred are the most important possession of the Kuanyama men. Today, the main men's ceremony is the cattle ceremony, in which after the rainy season a kraal owner's cattle are brought from the grazing outposts in a big round-up and are displayed to the clan members and the spirits of the clan's ancestors. In earlier times cattle were rarely killed except for sacrifice. The Kuanyama name their cattle and endow them with human qualities, whereas they show contempt for the small domestic animals such as dogs and chickens, which are cared for by the women. Cattle are still the chief exchange medium and standard of value. In the past cattle raiding was the major form of warfare. Moreover, when a new Kuanyama king came to the throne, he had his father killed and took his cattle. These stolen cattle formed the nexus of the king's sacred herd.

That the Ambo and the Herero have preserved the custom of keeping sacred cattle is probably due to their laws of inheritance. Most goods are inherited in the matrilineal line, but the sacred cattle, since they are strictly male possessions, are inherited patrilineally.

2. *The lack of plastic figurine arts.* The accompanying maps show that the area of African Negro sculpture (and therefore often of masked secret societies) and the area of the tse-tse fly and non-milking agricultural Africa tend to coincide. The maps also indicate that the routes of early Mediterranean cattle cultures were along the Great Rift highlands.

The Kuanyama are not a nomadic people. True nomads have horses and camels rather than cattle. Although the Kuanyama young men and boys practice transhumance—they graze cattle up north during the driest season and the floods of the following rainy season—it is obvious that mobility of population does not account for the lack of idols among the Kuanyama. Moreover, the Kuanyama do have pottery and wood carving. The reason for their having neither idols nor fetishes is that the intrusive Mediterranean cattle-people did not bring these into Africa. The Mediterranean agricultural peoples, like all early agriculturists, had idols and images, but the Homeric Greeks and the Hindus of the Rig Veda did not, nor do the orthodox Jews and Arabs of today.[18]

3. *The delayed marriage and prolonged courtship customs.* Among people with early cattle culture the position of women, especially young women, is quite high. Owing to the knowledge that early anthropologists had of Arabic, Greek, Roman, and Hindu customs they usually stated that the position of women deteriorated when cattle people gained the upper hand over matrilineal agricultural people. Even in 1953 Narr wrote that with this event the influence of women was severely diminished, and the right of free marital choice abrogated.[19] Authors of the Kulturkreise school dramatize the subjection of women by claiming that the secret societies passed from the hands of women into those of men.[20] Political influence and marital free choice are not, however, necessarily connected, and secret societies are found among agricultural peoples and not among cattle-raisers. Among the agricultural people of Negro Africa, marriages at an early age are arranged for the women and the bride has little voice in

choosing her first husband. Among cattle people of Africa, however, there is usually a long period of courtship ending with the payment of the so-called bride price which is actually the payment of a fee for the children that are to be born. The girl has some choice in the matter of a husband. Features of the long courtship are bundling and the singing of antiphonal love songs. These songs are similar in form and function throughout the cattle area from the Kuanyama Ambo throughout pastoral Africa, Europe, and Asia. They came to South West Africa, together with the other courtship customs, from early Mediterranean cultures.[21]

4. *The belief of the supernatural origin of twins among cattle raisers.*[22] Generally, cattle-raisers like twin calves and twin children. Both are signs of fertility. As do all marginal peoples, however, cattle-raisers believe also that twins are supernatural and dangerous, and they kill them unless they can render them innocuous by elaborate and costly ceremonies. Tribes who have the necessary wealth for such birth ceremonies even venerate twins. In South West Africa the rich eastern Ambo preserve twins, but the poorer western Ambo kill them.

The Ambo formerly killed one or both twins born to royalty. The twins would have had royal names and powers and thus would have upset the laws of matrilineal succession.

In their treatment of twins, peoples with a divine kingship often differentiate between those of like and of unlike sex. As among the Kuanyama, twins of unlike sex are said to have had intercourse in the womb and therefore, if they are allowed to live, they must remain married for life. Formerly in India and at present in Bali this form of incest is allowed only to royal twins; among the Kuayama, however, only twins of commoners are allowed to live and remain married. The Kuanyama rejoice at the marriage of twins since they are considered to be two spirits of different sexes with one personality, and thus to form a complete being. The Kuanyama conceive of their High God as a bisexual being, and also think it good luck to have intercourse with a hermaphrodite. This attitude toward the "bisexual being" is said to be Mediterranean in origin because, according to a recent work by Hermann Baumann,[23] in this whole area the bisexual gods preceded the male gods.

5. *Vestal virgins*, and 6. *the sacred fire.* Throughout the Old and New Worlds the vestal virgins and the sacred fire were formerly connected with the divine kingship.[24] The sacred perpetual fire that the virgins tended had to be kept alive for the sake of the king and his kingdom. In later times the idea of the sacred fire tended to become obliterated. In West Africa the vestal virgins perhaps became temple priestesses or Amazons; in Babylonia they probably became temple prostitutes, and in Ireland they became nuns.[25] In Africa all the vestal virgins were below the age of puberty so that they might not contaminate the sacred fire at the time of their menstruation.

The sacred fire was found formerly in South Angola and South West Africa.[26] Among the Kuanyama Ambo in former days a young girl from the Roan Antelope Clan lit the king's sacred fires. When the king died this girl was buried alive with him in order to light his fire in the next world. A daughter of his head wife could also light his fire. Today a native kraal owner has his head wife light his sacred fire in the main sitting place.

During the times of their migrations, the Ambo, like the ancient Greeks, always carried their sacred fire with them. They thus preserved their bond with the mother settlement.[27]

THE CONCLUSION

Granting that ethnographic traits have cartographic value, the Kuanyama Ambo appear to belong to a group of tribes in South Angola and South West Africa who are an enclave of ancient Mediterranean survivals. This assumption has been made on the bases of cattle migrations, the sacred cattle, the labyrinthic round kraal for people and cattle, the lack of plastic figurine arts among the cattle people including the Kuanyama, courtship customs featuring bundling and the unique and widely dispersed antiphonal love song, the attitude towards twins of different sexes, and the connection of the divine king with vestal virgins and the sacred fire. The migrations of early Mediterranean cultural influences are assumed to have come into Negro Africa both by way of the north and by way of South Arabia.

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Notes

1. Gibson, C. D., "Double Descent and its Correlates among the Herero of Ngamiland," *American Anthropologist,* 58: (1) 135, 1956.
2. Murdock, C., *Social Structure,* New York, 1949, p. 218.
3. Loeb, E. M., *Anthropos,* 41–44, 1946–1949, pp. 848–852.
4. Owing to the absence of an indigenous true cattle culture in the New World, American anthropologists have often omitted in their studies a consideration of primitive social stratifications or correlations between economy and social organization. Recently, however, three foremost scholars, a geographer, a historian, and a German ethnologist, have indicated their belief in the correlation between forms of economy and social organization.

The geographer, Carl Sauer, writes, "In Kamchatka, the Alaskan Peninsula, Kodiak, and on the coast of British Columbia, there were matrilineal societies, living in multifamily houses, with notions of property, prestige, and art forms which are about what might be left of Southeast Asiatic culture from which an adverse environment had eliminated certain possibilities, in particular agriculture." (*Agricultural Origins and Dispersals,* New York, 1952, p. 55.)

The historian, C. Quigley, believes, on the basis of the distribution of fish poisons, that there was a diffusion of the matrilineal root-planting, rain-forest cultures "across the South Atlantic from Africa to South America." ("Aboriginal Fish Poisons and the Diffusion Problem," *American Anthropologist,* 58 : (3) 513, 1956.)

The ethnologist, K. Narr, likewise indicates that the early planting culture was matrilineal. "The characteristic house of the root-planting people is the rectangular house. . . . Planting culture is mostly a mother-right culture, and differs from the culture of father-right cattle people." ("Hirten, Pflanzer, Bauern: Produktionsstufe," in *Historia Mundi,* Bern, 1953, Vol. II, p. 91.)

5. *The antiphonal love song* and *bundling* are fully described in "Courtship and the Love Song," *Anthropos,* 45 : 821–851, 1950, by the author.
6. A similar discussion with space devoted to defining terms will appear in the book now under preparation, *The Kuanyama Ambo of South West Africa* by the author.
7. Curson, M., and Thornton, R., "A Contribution to the Study of African Native Cattle," *The Onderstepoort Journal of Veterinary Science,* 7 : 613–747, maps 738–739, 1936.
8. Phillips, W., *Qataban and Sheba,* New York, 1955; plan on p. 285.
9. Deedes, C. N., in Hooke, S. (Ed.), *The Labyrinth,* London, 1935, p. 7. Deedes, however, believes that the labyrinthic king's tomb originated in Egypt.
10. Torriana, L., *Die Kanarischen Inseln und Ihre Urbewohner,* Leipzig, 1940, Plate XII a.
11. Phillips, W., *op. cit.,* p. 227.

12. Mathews, W., *Mazes and Labyrinths*, London, 1922, p. 34. "The German archaeologist Schliemann during his researches at Mycenae on the mainland, unearthed from one of the graves an ox-head of gold plate with a double axe between the upright horns." (Mathews, W., *loc. cit.*, p. 34.)

13. Loeb, E., "The Kuanyama Ambo and Other Tribes of South West Africa," *Anthropos*, 41–44, 1946–49, p. 851. This publication states, "The Ambo kraal appears as a wooden model of the Zimbabwe stone fortified city."

Fuller, C., "Can the Living Explain the Past in Rhodesia?" *African Studies*, 11 : 182, 1952. In apparent agreement with the above observation, Fuller states, "The Rhodesian walled habitations, constructed of natural rock with dry mortar technic at some undetermined date by an unknown people, find a modern counterpart of floor plan in the present habitations of the Ovambo and other peoples of Southern Angola and the Northwestern part of South West Africa."

14. Towne, C., and Wentworth, E., *Cattle and Men*, Oklahoma, 1955, p. 45. The Egyptians are said originally to have regarded all cattle as sacred, cows as well as bulls.

Rose, M., *Ancient Greek Religion*, London, 1946, p. 57. In the 12th book of the Odyssey mention is made of a herd of cattle sacred to the sun god, Apollo. This is reasonable, since in his earliest form Apollo appears to have been a god of herdsmen.

MacCulloch, J., *The Celtic and Skandinavian Religions*, London, 1948, p. 137. The Germanic tribes also had herds of sacred cattle, as those sacred to the god Fosite in Heliogoland.

Roscoe, J., *The Bakitara or Banyoro*, Cambridge, 1923, pp. 77, 78. The sanctity and worship of cattle among the African Nilotic peoples is a well attested fact. This is especially marked among the Nilotic Bakitara of Uganda.

Loeb, E. M., India is noted for the sanctity of its cattle and their four products. The Toda also have their herds of sacred water buffalo. Perhaps the Minotaur of Crete indicates bull worship, as the Apis cult of ancient Egypt certainly did.

15. Herskovits, M., "The Cattle Complex in East Africa," *American Anthropologist*, 28 : 636, 1926.

16. *Ibid.*, p. 650.

17. Schapera, I., *Western Civilization and the Natives of South Africa*, London, 1934, p. 14.

18. Sollas, W., *Ancient Hunters*, New York, 1924. Marginal agricultural peoples are not alone in their possession of plastic arts. Cro-Magnon as well as circumpolar peoples are noted for fine plastic arts, while the Bushmen had realistic paintings and stone engravings. As already mentioned, the circumpolar plastic art extended down to the Northwest Coast of North America. This fine art of early hunters and fishers has been attributed by Sollas and others to a single complex.

19. Narr, K., *op. cit.*, p. 81.

20. Schmidt, W., *Origin and Growth of Religion*, New York, 1931, p. 67. Robert Lowie has objected to this assumption, calling it evolutionary. (Lowie, R. E., *The History of Ethnological Theory*, New York, 1937, p. 190.)

21. Loeb, E. M., "Courtship and the Love Song," *Anthropos*, 45 : 821–859, 1950.

22. Loeb, E. M., "The Twin Cult in the Old World and the New World," *The Rivet Anniversary Volume* (in press).

23. Baumann, H., *Das Doppelte Geschlecht*, Berlin, 1955, p. 366. Baumann did not realize that natives sometimes distinguish in nomenclature and treatment between twins of like and different sex and hence he failed to bolster his theory of the importance of bisexual beings in archaic Mediterranean culture by using twins as examples.

24. Frazer, J., *The Golden Bough*, London, 1932, Vol. II, pp. 195–265.

Frazer apparently was correct in his theory that the vestal virgins of Rome were at first part of the king's family, and later were simply under the *patri potestas* of the king, and still later of the Pontifex Maximus. He was also correct in assuming that the early Latins lived in round clay huts (cattle-raisers' huts). The temple of the vestal virgins was round. Frazer was probably wrong in assuming that the sacred perpetual fire was derived from the custom of the most marginal peoples of maintaining fire continuously before they were capable of kindling it.

Roscoe, J., *The Baganda*, London, 1911, p. 275. Among the Baganda, it was not only the king who had a sacred fire and vestal virgins, for vestal virgins took care also of the sacred fires in most of the temples.

Wölfel, D., *Die Religionen des Vorindogermanischen Europa*, Vienna, no date, p. 345. A sacred perpetual fire was kept burning in the king's palace at Tara, the ancient capital of all Ireland.

Glacken, C., *The Great Loochoo*, Berkeley, 1955, p. 286. In Okinawa the *noro* priestesses guarded the sacred fire. This cult is supposed to have come from ancient Japan where still today there is an excellent example of a divine king.

Frazer, *loc. cit.*, pp. 243–245. In the New World, in Inca Peru, the home of llama herding, there is the same complex of the divine king, the vestal virgins, and the perpetual sacred fire.

25. Frazer, *loc. cit.*, p. 240. In Kildare, Ireland, nineteen nuns of St. Brigit tended a perpetual fire down to the time of the suppression of the monasteries under Henry VIII,

26. Vedder, H., "The Berg Damara" in *The Native Tribes of South West Africa*. Capetown, 1928, p. 48. The perpetual sacred fire tended by vestal virgins who belong to the divine king's family is a complex belonging to higher cultures. Some of the traits have spread to more marginal peoples. For example, in South West Africa the black-skinned Berg Dama[ra] have taken the sacred fire from the Herero, but not the vestal virgin.

27. The perpetual sacred fire belongs definitely to the cattle culture of the Old World, and will not be found among the primitive hunters, fishers, and gatherers, such as the Pygmies and Bushmen. The reason for the occurrence of this trait in the New World remains to be discovered.

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IRRIGATION, SETTLEMENT PATTERN, AND SOCIAL ORGANIZATION

Henry Orenstein

The theoretical problem of the impact of water works on sociopolitical organization is not a new one. In modern times it was first adumbrated by Karl Marx. Since then such culture historians and anthropologists as Childe, Sapper, Breasted, and Strong have commented upon the relation between irrigation and society. The most elaborate theory on the subject has been put forth by K. A. Wittfogel.

Wittfogel contends that large scale irrigation produces a specific type of society to which he has ascribed various terms: Asiatic Society, Oriental Despotism, Hydraulic Society, Oriental Society.[1] The size of this type of society may vary greatly; it may be national, regional, or possibly even communal in scope.[2] It is characterized by a highly centralized bureaucratic political organization, in which absolute power is exercised over the population.[3] It is also associated with a greater emphasis on males in the affected societies[4] and a tendency to accentuate the nuclear family as against the joint family.[5]

I will test one feature of the Wittfogel theory; that which has to do with the breakdown of the joint family. I will consider the effects of the introduction of irrigation on fifty-nine villages in one administrative division in the district of Poona; Bombay, India. About one half of the villages in this division receive water from the left bank canal of the Nira River—constructed in 1894—while the remainder depend solely on well irrigation.

Before presenting the data, some comment should be made on the suitability of India as a testing ground for the theory. The theory was formulated with reference to societies in which irrigation was an indigenous phenomenon; and our data are taken from a land in which the significant modern irrigation projects were introduced by a foreign conqueror. (Although many existed prior to the British, these are not important in the contemporary economy.) Furthermore, the British unified most of India under a single administration, and only after this were irrigation projects of appreciable size constructed. Thus, in most cases in India, one could not attempt to evaluate the political significance of irrigation. However, the portion of Wittfogel's theory which I intend to test refers to the family, a sociological phenomenon, so it would not be unreasonable to assume that if the theory is correct, this feature of it should apply to modern India.

There is one other possible objection. Contemporary India has been subjected to so many influences from the West—ideas as well as technical devices—that the effects of irrigation on family organization (if there are any) may not be discernible. The only response that can be made to this is to try to establish relationships, to see if they are, in fact, discernible.

As I see it, the possible effects which irrigation may have on the joint family are due to its immediate economic consequences. Irrigation often accentuates the importance of cash crops and a money economy. Where income is primarily for direct consumption, the joint family stores its produce in one unit and uses it when needed. But when a large part of income is in cash, its joint use becomes complicated, and it is a fact that a number of joint families are divided because of quarrels over the disposition of money income.

In my survey of the fifty-nine villages, I attempted to find out whether those villages which have more irrigation are the ones which have a lower percentage of joint families.[6] A summary of the data is presented in the contingency table below. The figures in the table give the number of villages.

		Average acreage under irrigation for households of village			
		0–.9	1.0–1.9	2.0–13.0	Totals
Per cent of households in village owning land jointly	0–25%	5	5	15	25
	26–60%	7	17	10	34
	Totals	12	22	25	59

A cursory glance at the table shows that the relationship is precisely what might be expected from Wittfogel's theory; those villages which have a greater mean acreage under irrigation per household tend to have a smaller percentage of joint families. This relationship is established by the chi-square test. Chi-square is approximately 9.5; and when this is checked against Fisher's table (Ref. 2, Table III; $P = 0.05$), we see that it is considerably greater than the value he gives (5.99). Thus, we may assume that the relationship between mean irrigated acreage per household and per cent of joint families is significant.

In an effort further to explicate this aspect of Wittfogel's theory, I have formulated two hypotheses, placing an intervening variable between irrigation and the attenuation of the joint family. (1) Local territorially based social groups will be weakened under the impact of irrigation. In the fifty-nine villages, this involves a dispersion of the households of each community. (2) Accompanying this there will be a tendency to weaken kinship based on groups such as the joint family.

The first hypothesis was conceived while doing an ethnographic study of one village in the area. It was noticed that almost half the population lived outside the main settlement near their farms. This was clearly related to a breakdown of social cohesion in the village. Among those who had left the main settlement, the tendency was to participate less in community life. Many came to the main settlement only when it was necessary to do so for payment of taxes, examination of records, and other official matters.

It appeared likely that the movements to the farms were caused by irrigation. Irrigation results in an increase in land value, and farmers are thus more likely to move nearer to their lands. Furthermore, the introduction of a new crop, sugar cane, resulted in year-round agricultural labor, for the crop requires almost continual watering. Unirrigated crops usually involve only a few months work out of the year. So, where there is irrigation in this area, it is, as one author put it, "to the immediate interest of each farmer to get out on his own soil." The author goes on to say, "Nothing could have distributed them but irrigation."[7]

This hypothesis was first tested by ascertaining the approximate date on which each move in the village was made. For a total of seventy-seven households, the movements are dated as follows.[8]

pre-canal (1894)	12
1894–1905	17
1906–1925	14
1926–1945	28
1946–1955	6

Out of the seventy-seven households about sixteen per cent existed prior to irrigation. This indicates that there are factors operating other than irrigation which cause such movements. However, the remaining data suggest that irrigation may have been of some importance in inducing dispersion. Twenty-two per cent of the moves were made in the twelve year period immediately following the introduction of irrigation. In the following twenty-year period there were relatively few moves, a fact which may be explicable by reference to the doubts with which many farmers first greeted the canal. In the next twenty-year period about thirty-eight per cent of the moves were made. About eight per cent were made in the last ten years. All in all, it would appear that in this village irrigation may have been one factor which brought about the dispersion of households.

I will now turn to an examination of the data from the fifty-nine villages. The contingency table below gives information pertaining to the relation between the amount of irrigation and the extent of dispersion.[9] As in the last table, the figures in the table give the number of villages.

		Percentage of households living on farms			
		0–24	25–49	50–76	Totals
Average acreage	0–.9	6	5	1	12
under irrigation	1.0–1.9	7	10	5	22
per household	2.0–13.0	5	11	9	25
	Totals	18	26	15	59

The table shows that the tendency is to have a larger percentage of households living out of the main settlement in those villages in which average acreage under irrigation is greater. The tendency is particularly notable where more than fifty per cent of the households in a village have already made the move. However, the data are not conclusive; they also show that a substantial percentage of households have moved in villages where irrigation is negligible. The chi-square test demonstrates that the association between the two features is not very significant. Chi-square is approximately 5, while Fisher's table gives it at 9.48. Thus no definite conclusion can be stated here; the evidence is merely presumptive.

The second hypothesis, that joint families will less often remain intact when spatially isolated, suffers an even worse fate than the former when tested in the fifty-nine villages. About twenty-five per cent of the households in the main settlements own land jointly, while thirty per cent—five per cent *more*—do so on the farms. Even this negative finding cannot be held with confidence. The figures in the table below give the numbers of households expressed as percentages of the total number of households in all fifty-nine villages.

	Farm	Village	Totals
Joint	12	15	27
Nuclear	28	45	73
Totals	40	60	100

Chi-square is about 0.30, while Fisher's table gives chi-square as 3.84. The association between the two features, is, therefore, of no significance.

In conclusion, it was found that of the three hypotheses tested, only one gives results with a satisfactory degree of confidence, and these results support Wittfogel's theory. They show that irrigation is associated with a smaller incidence of joint families. The other two hypotheses are not proved. There is some evidence that irrigation causes village dispersion, but this is merely presumptive. The evidence for the association of a low incidence of joint families with dispersed settlements is negative.

Despite the absence of clear confirmation, the presumptive evidence for the weakening of territorially based groups should not be overlooked. Possibly a more intensive household by household survey, instead of the village by village one, would give more positive conclusions. The hypothesis, I think, should be tested again in Maharashtra and in other areas.

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	Total households	Farm households	Total joint families	Farm joint families	Mean irrig. acres
1.	109	66	35	27	1.8
2.	257	146	87	52	1.6
3.	89	32	27	13	1.5
4.	80	5	29	3	0.5
5.	63	10	25	2	1.3
6.	248	101	86	56	0.7
7.	509	72	93	18	8.6
8.	146	53	39	13	1.6
9.	148	42	44	14	1.2
10.	60	5	18	4	0.6
11.	67	44	19	14	1.2
12.	188	27	44	4	1.4
13.	147	99	75	55	0.7
14.	714	378	182	85	1.5
15.	65	10	16	5	0.6
16.	130	51	33	16	1.4
17.	89	27	13	3	1.6
18.	224	97	61	33	1.0
19.	438	152	90	32	7.8
20.	100	1	37	1	1.2
21.	74	4	16	1	3.6
22.	630	261	158	61	5.9
23.	139	38	54	18	0.9
24.	205	94	64	25	1.3
25.	139	20	36	7	0.8

	Total households	Farm households	Total joint families	Farm joint families	Mean irrig. acres
26.	88	7	26	3	0.3
27.	116	48	32	15	3.3
28.	105	10	32	6	1.3
29.	182	51	33	14	0.8
30.	105	0	20	0	0.4
31.	12	0	3	0	3.5
32.	110	29	28	10	0.6
33.	111	61	37	18	3.8
34.	81	27	20	7	0.4
35.	118	52	23	10	2.9
36.	270	115	28	6	2.4
37.	191	33	67	14	1.0
38.	159	50	58	19	1.2
39.	104	57	30	20	13.0
40.	261	177	89	61	1.8
41.	171	127	89	69	8.9
42.	291	40	75	20	1.9
43.	242	185	55	45	6.6
44.	213	103	71	39	5.9
45.	78	3	17	0	8.8
46.	67	37	12	7	9.1
47.	260	149	73	46	5.6
48.	232	94	58	16	4.5
49.	410	202	79	41	2.4
50.	116	32	37	11	7.9
51.	226	126	66	39	5.5
52.	132	83	44	32	10.5
53.	498	219	117	42	5.0
54.	143	67	30	18	1.4
55.	108	9	30	3	1.7
56.	104	30	10	4	1.5
57.	413	296	164	117	7.0
58.	230	2	53	2	2.9
59.	128	73	14	10	8.0

Notes

1. Eberhard, Wolfram, *Conquerors and Rulers; Social Forces in Medieval China.* Leiden, Brill, 1952, p. 20 Wittfogel, Karl A., *New Light on Chinese Society; an Investigation of China's Socio-economic Structure.* New York, The International Secretariat, Institute of Pacific Relations, 1938, pp. 7–8.

2. Goldfrank, Esther S., "Irrigation Agriculture and Navaho Community Leadership: Case Material on Environment and Culture," *American Anthropologist.* 47 : (2) 270, 1945; Wittfogel, Karl A., and Esther S. Goldfrank, "Some Aspects of Pueblo Mythology and Society," *Journal of American Folklore,* 56 : (219) 20, Jan.–Mar., 1943.

3. Wittfogel, Karl A., and Esther S. Goldfrank, *op. cit.*, pp. 20, 28; Goldfrank, Esther S. *op. cit.*, p. 1; Eberhard, Wolfram, *op. cit.*, p. 22.

4. Wittfogel, Karl A., and Esther S. Goldfrank, *op. cit.*, pp. 36–38.

5. Wittfogel, Karl A., "The Foundations and Stages of Chinese Economic History," *Zeitschrift für Sozialforschung,* 4 : 42–43, 48–49, 1935. Wittfogel, Karl A. "New Light . . .," *op. cit.*, pp. 7–8.

6. In this paper, the term "household" will refer only to landowning households. The term "joint family" will refer to common land ownership by a group of relatives.

The information on the number of joint families was taken from the land records in each of the fifty-nine villages. There is some possibility of inaccuracy in using this method with no additional information, for people sometimes do not trouble to have records amended when family lands are divided. However, the information was checked, name by name, with the headman in each village, so the margin of error is likely to be small.

7. Deakin, Alfred, *Irrigated India; An Australian View of India and Ceylon, Their Irrigation and Agriculture.* London, W. Thacher and Co., 1893; p. 168.

8. Each household on the farm area was dated by asking the oldest inhabitant for an estimate. Estimates were taken in terms of remembered events, such as epidemics, which were dated. The estimates were then checked with one informant, an elderly man who had been the official record-keeper for the village forty or fifty years ago and who had been born in the village. The total number of households was eighty-two, but I could not get reliable estimates for five of them.

It is possible that the number of pre-canal moves is slightly greater than twelve. There are somewhat more than twelve households said to be pre-canal. However, some of them were closely related and said to be derived from a single ancestor who made the original move; these I counted as single units. This may give some slight inaccuracy, but it would not disturb the general picture presented.

9. The information on whether each household is found in the main settlement or on the farms was taken from the headmen in each of the fifty-nine villages. The land records were used; and each landowner's name was mentioned; the headman then informed as to whether the household was on the farms or in the main settlement.

THE CHRISTIAN HYMNOLOGY OF THE NORTH AMERICAN INDIANS

Willard Rhodes

Since the earliest days of exploration and colonization of the New World, Christian missionary activity among the Indians has been both intensive and extensive. Explorers, settlers, traders and trappers were accompanied or followed by men whose consuming goal was the conversion of the native to Christianity. The first seal of the Massachusetts Colony bore the figure of an Indian with the legend, "Come over and help us," and the first Royal Charter affirms that "to wynn and incite the natives of the Country to the Knowledge and Obedience of the onlie true God and Savior of Mankind is our Royall Intencion, and the Adventurer's free profession is the principall ende of this Plantacion." The heroic endeavors and experiences of these dedicated men have been well chronicled in the seventy-odd volumes of the Jesuit Relations and a sizable library of reports and personal accounts which they have left us. It is to these primary sources that one must turn for the ethnography of tribes that have long since become extinct or been absorbed by other tribes.

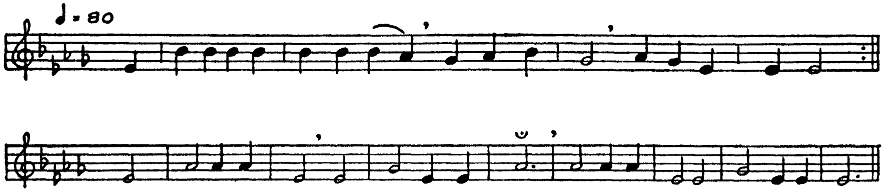
The first problem that confronted the pioneer missionary was that of language. Before he could communicate with his native charges and instruct them in the basic tenets of Christianity he had to learn the language and much of his time was given to the compiling of dictionaries and grammars, and the translation of catechisms, Bibles, the Book of Common Prayer and hymns. Very early he recognized the all-pervading role of music in the culture of the Indian and took advantage of its potential as a medium for the evangelization and instruction of the native. At first hymns and *cantiques* were taught by rote. At this stage the missionary depended upon various "aids to memory" such as the quipu which the Jesuits in Peru used to teach the Latin prayers, and one may conjecture that hymns were taught by the same means. Pictures suggesting the subject of each hymn stanza served to preserve the order of the stanzas. The use of the notched stick among the Kickapoo and Potawatomi as a guide for their prayers and hymns is reported by Father de Smet, the Jesuit missionary. Another mnemonic device was the rebus, that system of hieroglyphics, idiograms and pictographs which made possible the recording of the Lord's Prayer, *cantiques*, the mass and other offices of the church.[1] Chrestien Le Clercq, a Recollect, a member of a Franciscan Order, is acknowledged as the innovator of this system among the Micmac in Acadia (Nova Scotia) in Eastern Canada.[2]

The development of syllabaries and the reduction of the native languages to writing made possible and practical the printing of hymnals, for with the help of the missionaries the Indians rapidly became literate. It is not within the scope of this paper to review the many hymn books printed in Indian languages but a few general observations regarding this historical material will serve as a background for the study of contemporary Indian hymnology. In an excellent monograph which will serve as a guide for further research in this field, "*Hymnody in the American Indian Missions*," J. Vincent Higginson writes: "In many of the books the hymn text is given in the Indian language with the tune indicated in a modern language. A majority of the Catholic hymnals are of this type. Hymns from Latin originals have the tune indicated in that language, but with the others, the *cantiques*, many of which appear in most hymnals, the tune is not always named since they are largely drawn from a common source. The Protestant hymn books in some cases parallel their European equivalents. One can also trace in these books the transition from psalmody to hymnody and the gradual introduction of the hymns of English hymn writers."[3]

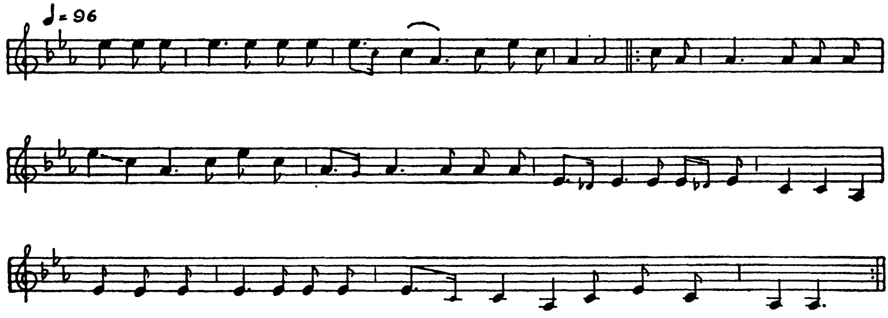
Among the few hymnals which printed the music with the Indian words are Father Vetromile's *Prayer Song Book* published in 1859, with a section of some twenty pages in English explaining the theory of Gregorian chant and several hymns in chant notation with Passamaquoddy words.[4] Others to be mentioned are *The Cherokee Singing Book*, with its Anglican tunes presented in traditional four-part harmony, the melody in the tenor as was the custom of the day,[5] *Dakota Hymns*, edited by the missionaries, John P. Williamson and Alfred L. Riggs,[6] and *Hopi Gospel Songs for Church and Street Services in Hopi-Land*, compiled by the missionaries under the direction of the Mennonite and Baptist Mission Boards (1931).[7] From her study of the Jesuit Relations Lota Spell notes that at first Indian melodies were used for the hymns, but after the Indians had learned the Christian words, traditional hymn tunes were introduced.[8] Occasionally an Indian melody gained special favor with the natives and became established as a classic in the hymnody of the mission. Since these melodies were rarely notated and were passed on by oral transmission, we have no way of recovering this music except as it may still live today among some of the more remote and less acculturated tribes. In the introduction to the fifth revision and enlargement of *Dakota Hymns*, the editors state, "Many of these hymns are the compositions of the Dakotas themselves. Some of our tunes are also Dakota airs. Thus the book has grown with the growth of the Dakota Mission, and is a token of the development of our Indian churches." From this statement it is evident that the editors in pointing so proudly to "the compositions of the Dakotas" are referring to the words, not the melodies. The three or possibly four native tunes found among the 152 hymns have lost much of their original beauty in being harmonized and fitted into a metrical structure. The foreword of *Hopi Gospel Songs* carries this statement: "Most of these songs have been made by the native Christians to some tune they learned from the missionaries at home or away in some Government Indian School." The tunes in this book have been borrowed from popular gospel song books of the day and betray, at least in their printed form, not the slightest influence of Hopi melos. One of the most interesting of Indian hymnals, *Indian Melodies*, was published for the Methodist Episcopal Church in 1845.[9] The melodies by a Narragansett Indian, Thomas Commuck, have been harmonized by Thomas Hastings. The words are in English and the tunes have been named after Indian chiefs, Indian females, and Indian names of places. The degree of acculturation of the composer may be estimated from his sincere and naive introduction, written in elegant English and dated at Manchester, Wisconsin Territory, March 7, 1845, and also from the melodies which are more Anglican in form and style than Indian.

But enough of the past. Let us turn our attention to the present and examine a few examples of Indian hymns as they may be heard today in the mission churches, and also some of the songs of the nativistic cults which have incorporated Christian elements into their practices and rites. The first example is a Christian hymn, recorded at Oglala, Pine Ridge Reservation, South Dakota, during the summer of 1942. The hymn is sung by a group of neighbors under the leadership of the elderly native Presbyterian minister, the Reverend Joseph Eagle Hawk.

This simple melody with its narrow range of a fifth, its tetratonic scale with a minor third between the ground tone and the next tone above it, and its plodding but very strong isorhythmic pattern, suggest some Gregorian chant as its ultimate source. Certainly the melody bears little resemblance to those in the Dakota hymnal, and it is equally remote from the style and structure of Dakota tribal music. If we are correct in assuming the origin of this Indian hymn in some Gregorian chant, then our problem is to determine when, where, and how it found its way into the hymnody of the Dakota Presbyterians. Reverend Eagle Hawk attributes the authorship of this hymn to a woman, Toti Luta (Red Lodge), one of the first Sioux converts to the Christian religion. Further research in the history of the missions of Minnesota and the Dakotas may throw some light on this interesting hymn.



Example No. 1. Dakota Christian Hymn

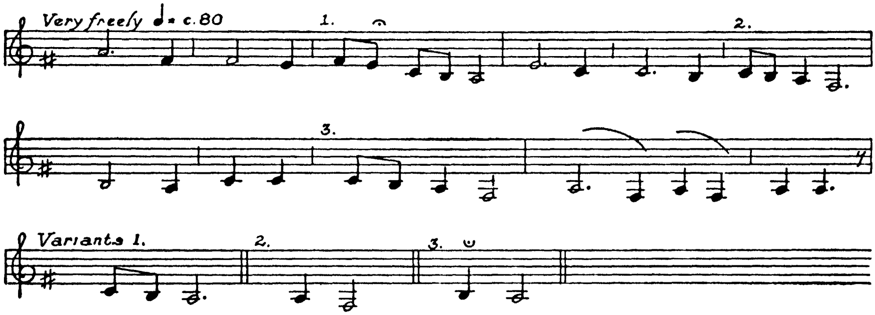


Example No. 2. Kiowa Christian Hymn

The second example is a native hymn recorded in August, 1951, at the Big Tent, a camp meeting conducted by the Rainy Mountain Baptist Church in conjunction with the annual American Indian Exposition at Anadarko, Oklahoma. It is sung in the Kiowa language.

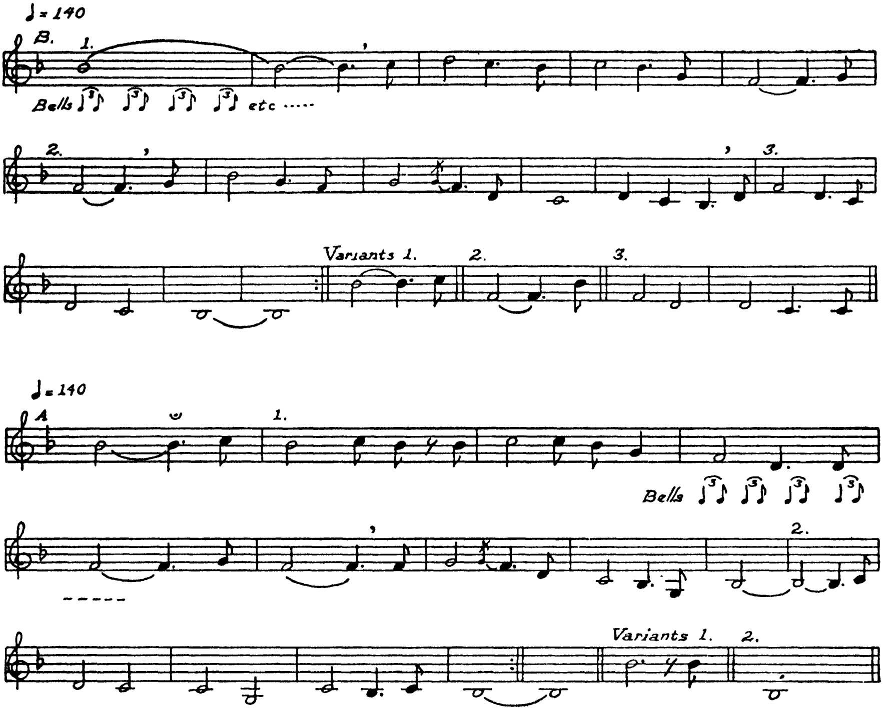
The wide range of an octave and a fifth, the triadic tonal pattern, and melodic contour with its sequence of cascading phrases finally coming to a point of repose on the ground tone, identify this melody as an example of Plains Indian melos. The Kiowa have been taught hymns and gospel songs in their native language to the traditional Euro-American tunes, but unlike most other tribes they have maintained their tribal identity and found their religious satisfaction in hymns of their own making. Though the words are little more than a translation of a simple Christian sentiment into the Kiowa language, the melodies are original and thoroughly Indian in style. The hymn offered here is but a single example from a rich and growing collection of living music.

Among nativistic Indian religions of today the Peyote cult claims first attention because of its wide pan-tribal diffusion and the quiet but intense vitality with which it has propagated itself. The Native American Church, by which name the Peyote cult is incorporated and chartered in a number of western states, possesses a constantly expanding song literature that is distinctive in style, and to a large degree independent of the prevailing musical style of the tribes where it is found. A number of Peyote songs reflect Christian influence in their texts. References to God, Jesus, and Christian ideas are to be found in Peyote songs from the Ute, Dakota, Iowa, Cheyenne, Fox, Winnebago, Gosiute and Pawnee. But the style of Peyote music with its prevailing down curve and final flattening out of the melody on the ground tone, its uneven time divisions, and accompaniment of water drum and gourd rattle, is distinctly Indian and derives little or nothing from Christian hymns.[10]



Example No. 3. Shaker Church Song

The syncretic Shaker religion of the Indians of the Northwest coast, which had its beginning in 1882, is a curious blend of features drawn from the old guardian spirit religion and the Christian religion, Protestant and Catholic.[11] Like the music of the Peyote cult Shaker songs have achieved an individuality of style that makes them readily recognizable. The texts of the songs, sung either in the native language or in English, are short and simple and are reminiscent of those found in *Hymns in the Chinook Jargon*, a little pamphlet written and compiled by the missionary, Myron Eells, and first published in 1878. Like the religion itself the music of the Shaker songs is a syncretic fusion of elements from the old guardian spirit songs with rhythmic and melodic fragments of gospel hymns of evangelical sects that have been elaborated according to native principles and techniques of melodic development. Examples 3 and 4 are typical Shaker Church songs. The first song (Example 3), very lyrical in its melodic contour, is a prayer song, recorded at Neah Bay, Washington in the summer of 1950. Example 4 gives the notation of the first and last of a group of four so-called "work songs," used for the healing of the sick and accompanied by dancing. They were recorded at La Conner, Washington in the summer of 1950. These two songs are so much alike in melodic movement and structure that either one could be considered a variation of the other. The melodic fragment in measures 3, 4 and 5 (Example 4B), is very suggestive of a similar passage in the gospel hymn, *In the Sweet Bye and Bye.*



Example No. 4. Shaker Church Songs

In a summary description of the musical style of Shaker songs based on an analysis of twenty-nine melodies, I have listed sixteen features, some of which are diagnostic.[12] Most arresting is the accompaniment of hand bells replacing the traditional accompaniment of drums and rigid percussion instruments. Rung in strict coordination with the regular impulse of the song, the bells sound a succession of triplet figures of a quarter note followed by an eighth note, the short note resulting from the reflex movement of the hand and arm after the down stroke. The emotional involvement and absorption of the singer, and the singing style with its relatively clear intonation, refined and expressive vibrato, the employment of the diminuendo at the close of a song, and the deep, heavy, audible inhalations which punctuate the phrases are easily traceable to the guardian spirit songs. The preponderance of quadruple meter and a more or less consistent adherence to it throughout the song, and the clear-cut symetrical structural forms composed of four, three, or two phrases, with a decided preference for the three-phrase form, are more likely derived from the music of the missions than from the native songs. In attempting to trace and evaluate influences in the analysis of a musical style the ethnomusicologist is handicapped by the lack of an adequate methodology. No theory comparable to that of harmony and counterpoint in Western European music has been developed to deal with music in its most basic form, melody. It may be too much to ask for a theory and methodology that will satisfy all needs, but our scientific and humanistic interests impel us to seek more precise tools and concepts than those employed at present.

In elevating the Indian woman to a position of equality with that of men the Christian missions opened to her a medium of expression in which her rights theretofore had been strictly limited and defined. Ceremonial and cult music had been almost exclusively the prerogative of men. Under the new dispensation women could participate on an equal basis with men in the singing of sacred songs. McAllester has reported the attitude of one Navaho woman who in her testimonials at the Rimrock Galilean Mission frequently expressed "her thankfulness that the new religion had given her the courage to sing right out in front of everybody."[13] When the Shaker Church was incorporated in 1910 under the laws of the State of Washington, it was stated in the record that one of the objects of the corporation was "the elevation of the female Indian, they to be equal in the government of the church. . . ."[14] To credit this declaration entirely to the influence of Christianity would be unfair to the Indians, for Eells noted that "they (women) are by no means so near in a state of slavery as some Indian women in the Interior, but are treated with considerable propriety by their husbands."[15] Once the area of sacred song was opened to women they were free to demonstrate their innate musical creativity in the making of songs. Many of the most beloved hymns of the Kiowa have been made by women. The Shaker Prayer Song (Example 3) was composed by a woman, Mrs. Lyda Butler Hottowe. The change from a native tribal religion to that of Christianity or one of the Christian-influenced nativistic cults has not altered the traditional pattern of musical composition. Christian hymns and Shaker Church songs are "received" in dreams, just as guardian spirit songs were acquired in the past.

In this paper I have not dealt with the contemporary hymns of the Catholic Indian missions. To my knowledge no work has been done in this field and source material in recorded form has not been collected. This is an area of investigation that needs to be worked for the state of this music may throw additional light on the process of acculturation. The volume of the autochonous Indian hymnology is very small in comparison with the bulk of hymns that the missionaries translated into Indian languages and taught the natives. In their zeal to make the Indian like the white man and to impose an alien culture on him, the missionaries disregarded the native music, one of the most precious and meaningful elements in Indian culture.

In retrospect it appears that the missions might have promoted their cause and smoothed the acculturation process for the Indian by recognizing the value of native music and employing it more fully in their work.[16] McAllester, in a discussion of music as an aid to rapport in field work, writes, "I have found the exchange of songs, the discussion of their meaning, and the appreciation of music in general to be an excellent avenue of approach to this problem in several cultures. . . . There seems to be something more acceptable about a stranger who wants to learn songs than about one who wants to know how long babies are nursed."[17] If there is anything to be learned from a study of the function of music in the Indian missions, it is in the recognition of the value of music in the socio-cultural relations of Indians and whites. European governments have long maintained ministries and bureaus of fine arts for the promotion and cultivation of the arts within the country and for cultural exchange with other nations. The United States is belatedly recognizing the importance of the arts in international relations and is presently conducting a program through the State Department in association with the American National Theater Association. But this program has been one of exportation and limited to the art music of the professional musician. To contact the masses of people such a program should include folk and ethnic music and folk arts and crafts. Might we not improve our relations with the under-developed countries of the world by learning about their culture from them? And who is better prepared to do this in the field of music than the trained ethno-musicologist?

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Notes

1. Washington, D.C., U.S. Bureau of American Ethnology, *Annual Report, 1888–89,* pp. 152, 231–232, 263. "Picture Writing," p. 224.
2. Le Clercq, C., *First Establishment of the Faith in New France.* Translated with notes by J. G. Shea, New York, J. G. Shea, 1881. Vol. I, p. 15.
3. Higginson, J. Vincent, *Hymnody in the American Indian Missions,* Papers of the Hymn Society, No. XVIII. New York, The Hymn Society of America, 1954, p. 10.
4. Vetromile, E., *Attiamihewintuhangan* (*The Prayer Song Book*). New York, Dunigan, 1859.
5. *The Cherokee Singing Book,* Printed for the American Board of Commissioners of Foreign Missions, by Alonzo P. Kenrick at C. Hicklings Office, 20 Devonshire St., Boston, Mass., 1846.
6. *Dakota Odowan* (*Dakota Hymns*). Published by the Dakota Mission of the American Missionary Association and the Presbyterian Board of Foreign Missions. Printed by the American Tract Society, 7 West 45th Street, New York. 1925. Copyright, 1879.
7. *Lomatuawh-Tatawi,* Hopi Gospel Songs for Church and Street Services in Hopi-Land. Compiled by the missionaries under the direction of the Mennonite and Baptist Mission Boards. 1931.
8. Spell, L., "Music in New France in the 17th C.," *Canadian Historical Rev.,* 3 : (2) 129, June, 1927.
9. *Indian Melodies,* Published by G. Lane & C. B. Tippett, for the Methodist Episcopal Church, N.Y., James Pollard, Printer, 1845.
10. McAllester, David P., "Peyote Music", *Viking Fund Publications in Anthropology,* 13 : 43–45, 85–88. New York, 1949.
11. Gunther, Erna, "The Shaker Religion of the Northwest," in Marian W. Smith (Ed.), *Indians of the Urban Northwest.* New York, Columbia University Press, 1949, pp. 37–76.
12. Rhodes, Willard, *Music of the North American Indian Shaker Religion* (unpublished manuscript).
13. McAllester, David P., "Enemy Way Music—A Study of Social and Esthetic Values as seen in Navaho Culture. *Papers of the Peabody Museum of American Archaeology and Ethnology,* 41 : (3) 69. Harvard University, Cambridge, Mass. Published by the Museum, 1954.
14. See note 11 above; p. 61.
15. Eells, Myron, *Ten Years' Missionary Work at Skokomish,* Boston, 1886.
16. See note 13 above; pp. 84–85.
17. See note 13 above.

Discography for the Musical Examples

Example 1. Music of the American Indian-Sioux, recorded and edited by Willard Rhodes, AAFS L40, Washington, D.C., Library of Congress. A3.

Example 2. Music of the American Indian—Kiowa, recorded and edited by Willard Rhodes, AAFS L35, Washington, D.C., Library of Congress. A7.

Example 3. Music of the American Indian—Northwest (Puget Sound), recorded and edited by Willard Rhodes, AAFS L34, Washington, D.C., Library of Congress, B1.

Example 4. Music of the American Indian—Northwest (Puget Sound), recorded and edited by Willard Rhodes, AAFS 34, Washington, D.C., Library of Congress, A5.

*

THE ACCULTURATIVE PROCESS IN JAMAICAN REVIVALISM[1]

George Eaton Simpson

The Afroamerican religious cults of Jamaica include Revival Zion, Pocomania, and Cumina. Apparently Cumina, the most African of these cults, is found only in the eastern part of the island, with the main concentration in the Morant Bay area.[2] It is difficult to draw clear-cut distinctions between Pocomania and Revival Zion, but groups of the former type seem: (1) to place more emphasis on singing and "spiritual" dancing than on preaching and Bible explanations; (2) to make greater use of conjuring and of extreme techniques of healing; and (3) to have somewhat more emotionally unstable leaders than the Revival Zion groups. These are not infallible criteria because Revival Zion leaders sometimes use evil spirits to harm others, and their healing methods are not always limited to such simple procedures as the laying on of hands, praying, and reading the Bible. None of my informants call themselves Pocomanians, although a number of them were so labelled by others. Pocomania is now a derogatory word, and the religious cultists of West Kingston prefer to think of themselves as "Revivalists" or "Baptists."

The Jamaican revivalism of today is descended from the Baptist fundamentalism of the late eighteenth and early nineteenth centuries, West African religious and magical traditions, and non-African magical procedures. In the present study, "revivalism" refers to the whole complex of Revival Zion-Pocomania-Obeah beliefs, formulas, and rites.

In considering the revivalist cults, it is well to keep in mind the nature of the acculturative situation in Jamaica. We are not dealing here with "cultural change that is initiated by the conjunction of two or more autonomous cultural systems."[3] Rather, we are discussing individuals whose ancestors were uprooted from their societies and cultural settings and placed in a new culture.[4] For nearly two hundred years in Jamaica, most of the contacts among the carriers of different cultures were occasioned by the slave trade and the institution of slavery. Acculturation was forced upon the Afro-Jamaicans in technology and economic life, and, to some extent, in language and social organization. Generally, West Indian planters were opposed to preaching the Christian gospel to the slaves because of their fear that it would make them harder to manage and give them ideas of rebellion and freedom. They seem not to have been impressed with the argument that Christian doctrine would cause their slaves to be more obedient and docile.[5] No missionary work was carried on in Jamaica for a century after the English conquest of 1654,[6] and the official missionary movement did not get underway until the 1820's. Thus for forty years after 1780 the Native Baptist movement, led in part by slaves and freedmen who had lived in the United States, was without serious competition. Curtin credits these "unofficial missionaries" with much of the early development of Jamaica's Afro-Christian cults,[7] and he concludes that the Native Baptists were stronger in 1860 than European orthodoxy.[8] Jamaica was swept by an emotional religious revival in 1861–62, and the demand for religious leadership resulted in the formation of hundreds of new "churches."[9] Eventually Jamaica became overchurched. In 1942, J. M. Davis wrote that "Jamaica is, or has been, well evangelized. Several strong church groups and missions, well supplied with men and funds, have worked faithfully for many generations in this small island with its limited population. Churches and chapels have been provided in almost every valley and almost every hamlet of the island. . . ."[10] Despite (a) the evangelizing efforts of the orthodox denominations and of such sects as the Church of God, Jehovah's Witnesses, and the Pentecostal groups, and (b) the models provided by foreign whites and middle and upper class Jamaicans, thousands of lower-class Jamaicans are affiliated today with revivalist cults. Other thousands, at least nominal members of more conventional religious organizations, are significantly involved in the Revival Zion-Pocomania-Obeah complex.[11]

But to return to the middle third of the nineteenth century. Obeah (witchcraft) practices, largely of African origin, paralleled by Myalism, or rites which were believed to counteract the work of Obeah men, were widespread in Jamaica.[12] As we have seen, the Native Baptist movement came into existence during the latter part of the eighteenth century. According to Curtin, the Great Revival of 1861–62 "turned African." It became "more and more a mixture of myalism and Christianity, ending as a permanent addition to the Afro-Christian cults."[13]

In studying acculturation in Jamaica, we share Herskovits's point of view concerning the investigation of New World Negro life in general, namely—

> that our essential problem is not to seek out, as cultural curiosities, the segments of African forms of behavior and of African institutions that have been retained in the course of the New World experience of the Negro. It is rather, we now recognize, our task to analyze the cultures of New World Negroes wherever and in whatever state we find them, in terms of the manner in which they represent the transmutation of African, of European, and of American Indian elements into the cultural totality we study. What has been taken over from non-African cultures is, at the present state of our analysis of the field, just as important as what of Africa has been retained . . .[14]

THE BELIEF SYSTEM, RITUAL PARAPHERNALIA, AND CEREMONIES OF JAMAICAN REVIVALISM[15]

These cultists believe strongly in the existence of many spirits. Among the favorite spirits in the revivalist pantheon are: Old Testament prophets, such as Jeremiah, Ezekiel, Isaiah, Samuel, Daniel, Solomon, and Joshua; New Testament Apostles and Evangelists, including Matthew, Mark, Luke, John, James, and Peter; God (also called Jehovah and Omnipotence), Jesus Christ, the Holy Ghost, and Mary; archangels Michael, Gabriel, and Raphael; Moses, Miriam, Caleb, David, Shadrach, Meshach, and Abednego; Satan and his chief assistant, Rutibel; such beings from Hebrew magical tradition as Uriel, Ariel, Seraph, Nathaniel, and Tharsis;[16] Casuel (probably Casziel, one of the four main astral spirits associated in astrology with the malefic planet Saturn); such assorted figures as Constantine, Melshezdek, and the Royal Angel; and, finally, but by no means the least important, the dead. Most revivalists "travel under" one spirit, but some of the leaders claim that they receive messages from and are protected and assisted by many spirits.[17] In the view of lower-class Jamaicans, the dead retain an interest in their living relatives and are capable of aiding or harming them. In addition, skilled operators can summon and utilize the duppy, a kind of second soul, of a deceased person, and they can nullify the aggressions of duppies which are troubling them or their followers. Untutored Jamaicans fear snakes, frogs, and lizards because of the possibility that they may be duppies which conjurers have turned into these forms of life.

Dreams and visions play important roles in the lives of revivalists by validating the positions of officiants, providing warnings about the evil intentions of enemies, predicting coming events, rationalizing conversion to the cults, and in reenforcing traditional beliefs about the spirits.

While it is possible analytically to distinguish revivalism as religion from obeah as magic, the present writer thinks that these two sets of beliefs and activities are almost always associated to some degree in the actual programs of a West Kingston cult. This is not to say that obeah is not practiced exclusively by a number of persons who are not prominent revivalists. Those who attend revivalist meetings are well informed about obeah, and a number of revivalist leaders do not limit themselves to conducting religious services. Devotees of many of these cults look to their leaders for advice and assistance in recovering from illness, putting on or taking off duppies, insuring success in hazardous undertakings, and so forth.

The ritual paraphernalia of a revivalist church is concentrated on and near an altar or a table which serves as an altar. The principal ritual objects include: one or more Bibles, white candles, flags made of colored cloth, wooden crosses, jars of leaves, vases of flowers, glasses of consecrated water, a shepherd's crook or a conjurer's rod, swords and machetes (wooden or steel), and, in some cases, polished stones. In West Kingston, it is more common to find sacred stones of one kind or another in the homes or yards of leaders, or near pools of consecrated water or elsewhere inside the churches, than on altars or altar-tables. At a "table," a combined religious service and feast, loaves of bread, cakes, fruit, unsalted rice, goat meat or lamb, and bottled soft drinks are arranged on long, narrow tables.

During a ceremony, a revivalist "Captain" or "Shepherd" wears a crown or a turban, and a robe, frequently an elaborate one. Most of the revivalist women wear white dresses and turbans, but a minority come to ceremonies in red, blue, or figured dresses. If they can afford them, the men wear white suits or white trousers and shirts.

Drums, usually in sets of two or three, outrank all other musical instruments among the revivalists, but they are usually supplemented with rattles and tambourines. A whistle may be used to restore order during a meeting, but it is a less important object than its counterpart in Haitian *vodun*.

The most frequently held revivalist ceremony, referred to as "divine worship," occurs on Sunday evening and one or two week-nights. Drumming, hand-clapping, singing (both collective and antiphonal), praying by the leader and individual prayers by the members speaking simultaneously, Bible-reading, testifying, counterclockwise "spiritual" dancing around the front part of the church, preaching interspersed with songs, spirit possessions and, in some cases, public healing constitute the main features of this type of service. Special revivalist ceremonies include baptismal rites, the numerous death rites, the dedication of a new church, the installation of a new officer, healing rites, divinatory and conjuring rituals. The "tables" mentioned previously are given for several purposes: to offer thanks to the spirits, to seek deliverance ("uplifting") from trouble; to mourn the dead (memorial services); to raise money; and to injure an enemy ("destruction" table). Since each service lasts from three to eight hours, and since many revivalists attend two or more services weekly, a believer devotes a considerable amount of his free time to revivalist activities.

ACCULTURATION IN JAMAICAN REVIVALISM

Jamaican revivalists do not equate old African gods such as Ogoun, Oshun, Legba, Obatala, Shango, Agwe, Erzilie, Damballa, and Azaka with Catholic saints as do the members of syncretistic cults in Haiti, Cuba, Brazil, Trinidad, and other parts of the New World which have sizable Negro populations and where the Roman Catholic church is a dominant influence. As previously indicated, cultists in Jamaica believe that there are many spirits, including Old Testament prophets, New Testament saints, God, Jesus, the Holy Ghost, Mary, angels and archangels, Satan and his assistants, other assorted supernatural beings, and the dead, who take an active interest in the affairs of the living and intervene in those affairs. The form of the gods has changed in the shift from West Africa to the New World, but the polytheistic orientation and the belief in the constant and direct intervention of the gods in human concerns have been retained and reinterpreted. The African tradition that nothing is entirely good or entirely bad, and, specifically, that "each Vodu has his gifts to man and his punishment when angered"[18] has been retained in the revivalist belief that most of the spirits are capable of aiding or harming men, depending upon the way they are treated.

The traditions and rites of the West African ancestral cults and of Protestantism have been syncretized in the beliefs about the dead and the death rites of Jamaican revivalism. In Dahomey, for example, there are partial and definitive burials, a wake after the definitive burial, a mourning period of three months, and the feeding of the familial gods at ritually stated times.[19] Jamaican revivalists observe a wake before the burial, a funeral, and, if finances permit, a Nine-Night service, a Forty Days mourning "table" (for a leader), and a "memorial table" at the end of one year, or every year, after a family member's death.[20] Supernatural sanction of common-law marriage in Jamaica is seen in the belief that all of the "wives" and all of the children must attend and participate in all of a man's death rites if his spirit is to be prevented from returning to disturb them. This aspect of Afro-Jamaican culture is a reinterpretation of the supernatural sanctions, derived from the ancestral cult, of the African patterns of plural marriage.[21] The persistence of the multiple soul concept of West Africa is seen in the Jamaican belief that each person has a duppy (shadow) in addition to a soul. Also, the ability of a conjurer to use a captured soul[22] has been reinterpreted in Jamaica in the power of an operator to summon the spirit of a dead person to assist him in his "work."

The height of religious experience in West Africa and in many parts of the New World is possession by a spirit which enters the head of a devotee. These possessions, unlike the trance experiences of European mystics, occur publicly, and there are marked similarities in (a) methods used to induce possession, (b) motor behavior during possession, and (c) ways of controlling possessed persons in West Africa, Haiti, Brazil, Trinidad, and Jamaica.[23]

The Bible, an important part of the apparatus of Jamaican revivalism, is found on every altar, and it is always carried in baptismal and other processions. Besides being a highly regarded ritual object, its teachings constitute important parts of revivalist belief. A superficial acquaintance with revivalism might lead to the conclusion that its theology is entirely that of Protestant fundamentalism. Actually, biblical passages are interpreted in accordance with the total Afro-European belief system of the cults.

Jamaican magic represents a combination of the folk traditions of Europe and West Africa. This merging is seen clearly in the way the importance of the charm has been reinforced by both traditions, with the de Laurence books of magic providing inspiration for many magical procedures.

The words and melodies of most of the revivalist songs are derived from Protestant hymns, but the emphasis on rhythm in singing, the use of drums and rattles, the polyrhythmic drumming, the handclapping, foot patting, and dancing (as a part of religious ceremonies) are African retentions.

Water is a major ritual element in West African religions, and visits to the river or the ocean to obtain sacred water are parts of ceremonies among the Yoruba, the Ashanti, and in Dahomey.[24] The principal water rituals of Jamaican revivalists are those of baptism, and the preparations for these ceremonies are elaborate. Glasses, jars, and pools of consecrated water are much in evidence in West Kingston revivalist churches, and the religious and magical uses of this water include: attracting a spirit to a service, driving away evil spirits, healing, divining, and duppy-catching. Clearly the ritual uses of water in revivalism are syncretisms of African and European elements.

In Nigeria, the Yoruba believe that a certain type of stone (*iproni*) is the material object which represents the power of a deity, and sacrifices are presented to it. A thunder stone is one of the ingredients used in instituting a new shrine to the gods in Dahomey, and stones constituted the shrines of important gods in Ashanti.[25] Polished stone celts, and other types of stones, found in the homes, yards, and churches of nearly all revivalist leaders, are believed to "carry" power. They are used to invoke particular spirits, to guard against evil spirits, to add potency to the consecrated water used in healing, and to accomplish certain ends through sympathetic magic.

Herbs are important in African magic, and leaves are used in making a new *Vodu* shrine in Dahomey. Although the herbs are not as important in Jamaica as they are in Cuba, where they are used to cleanse the sacred stones, they are always found on revivalist altars and "tables" and frequently the remedies prescribed for illnesses and other misfortunes are "bush teas" or "bush baths."[26]

The serpent is a major figure among supernatural beings in West Africa and in certain parts of the New World, especially in Haiti and Dutch Guiana.[27] Jamaican revivalists fear snakes, as well as lizards and frogs, because they believe that they may be duppies in disguise. While there are biblical references to snakes, and serpents are portrayed in books of magic, the belief that snakes may be duppies in disguise is closer to West African supernaturalism than to the ancient Hebrew symbol for temptation or to Moses's feat of transforming a snake into a rod.

In the following chart thirty-eight cultural elements of revivalism are tentatively classified as: (1) full or nearly full African retentions, (2) parallel traditions, (3) syncretisms, (4) reinterpretations of African elements, and (5) European-borrowed traits and reinterpretations of European elements. Retentions here mean African valuation or behavioral elements which show the least degree of cultural change. Items common to both Europe and Africa which seem to have reenforced each other in the New World are called "parallel traditions." Syncretisms represent a degree of synthesis "where old and new are merged into a functioning unified entity of clear bi-cultural derivation."[28] Reinterpretation has been defined as "the process by which old meanings are ascribed to new elements or by which new values change the cultural significance of old forms."[29] The final category refers to European elements which have been taken over in entirety or which appear as reinterpretations in Jamaican revivalism.

ACCULTURATION IN WEST KINGSTON REVIVALISM

Full or nearly full African retentions	*Parallel traditions*	*Syncretisms*	*Reinterpretations of African elements*	*European-borrowed traits and re-interpretation of European elements*
Supernatural beings who intervene in the affairs of men Tradition that nothing is entirely good or entirely bad Importance of the dead Emphasis on rhythm and polyrhythms Drums and rattles Handclapping and foot patting Dancing as a part of religious ceremonies Revelation by the gods in giving remedies to men Throwing food to the gods	Use of dreams in divining Removal of shoes during ceremony Belief in witches	Ritual uses of water Numerous death rites Extensive use of charms	Polytheistic orientation Multiple soul concept Public possession by the spirits Utilization of spirits of the dead Antiphonal singing Belief that snakes may be duppies in disguise Crossroads as a favorite place for magical operations Uses of stones Uses of leaves Use of blood Supernatural sanction of plural marriage Poles bearing cloths of various colors	Bible as a ritual object Words and melodies of songs Books of magic Cross Candles Shepherd's crook, staffs, rods, canes Whistle Key Inscriptions and placards on walls of church Incense Perfumes or "oils"

The Social Science Research Council Summer Seminar on Acculturation, 1953, concluded that "fairly specific recurring sequences of events in acculturation" flow from the conjunction of two or more cultural systems. These processes include: (a) cultural transmission (diffusion), (b) cultural creativity, (c) cultural disintegration, and (d) reactive adaptations.[30] Other concepts suggested by this Committee are intended to cover the "more pervasive and lasting outcomes of acculturation, including progressive adjustment of two types, fusion and assimilation, and the development of stabilized pluralism."[31] Earlier we pointed out that we are dealing with a situation which involves individuals whose ancestors were uprooted from their societies and cultural settings and placed in a new culture rather than with the conjunction of two or more autonomous cultural systems. For the purposes of our analysis, the concepts here employed seem to be more useful.

Differential Rates of Cultural Change. On the basis of his own and other studies, Herskovits has presented a valuable "scale of intensity" of New World Africanisms. Judged on the basis of available data, ten cultural categories are listed in terms of the greatest to the least degree of African retention: Music, Magic, Folklore, Religion, Non-kinship institutions, Language, Social Organization, Economic Institutions, Art, and Technology. This scale designates each category as: (a) very African, (b) quite African, (c) somewhat African, (d) a little African, or (e) trace of African or absent. In its revised form, twenty-one sub-areas in twelve countries are covered, ranging from the most African Dutch Guiana bush to the least African urban North of the United States. As a country, Jamaica is placed fifth on this scale; as a sub-area, Jamaica (general) is twelfth.[32]

In discussing differential rates of cultural change, the members of the 1953 Social Science Research Council Seminar on Acculturation state that

> . . . the conventional categories of cultural description—technology, social organization, religion, etc.—do not readily lend themselves to an analysis of differential change. *All* cultural segments have their concrete aspects, and these more explicit behaviors and apparatus are as a rule more readily mastered than symbolic and valuational aspects. In religion, for example, objects and rituals may be assimilated as rapidly as new tools. They may be integrated as long as they enhance prevailing security and orientational functions. . . .[33]

All of the items in our category of "European-borrowed traits and reinterpretations of European elements," with the exception of the Bible and books of magic, are apparatus, and even these have that phase as well as their symbolic and valuational aspects. As the chart indicates, the full or nearly full African retentions include both basic premises and explicit behaviors. By no means all of the concrete aspects of culture disappear in acculturative situations like those of the Caribbean.

Not only are notable differences in the degree of acculturation seen from country to country, and from sub-area to sub-area within the same country, but there are differentials according to: social class and its correlates of education, income, and occupation; race mixture or the lack of it; age; sex; and urban or rural residence. Of these variables, class is by far the most important. As Herskovits has said: "In those strata of society where there has been full access to the cultural resources of the dominant group, even reinterpreted elements would be idiosyncratic, rarely patterned."[34] Of interest is the tendency in West Kingston for individuals to shift from revivalist cults to religious groups of greater prestige if their socio-economic status improves significantly. Thus a person might change from Revival Zion to the Church of God or Jehovah's Witnesses, and, later, to the Seventh Day Adventist, Baptist, or Methodist church.

THE FUTURE OF JAMAICAN REVIVALISM

Increase or decrease in the size of a given revivalist group, and in the total number of these bands during a given period, depends upon several factors. Individuals are attracted to these cults because the experiences they have in the ceremonies, the answers given to their questions, and the healing, divinatory, conjuring, and other services provided by the leaders afford more satisfaction than they find elsewhere.[35] Children born to ardent revivalists, as well as the relatives and acquaintances of such persons, are exposed to direct and indirect indoctrination. Some of the more successful and ambitious leaders attempt to establish branches in other sections of a city or in other cities. Anyone can start a church, but presumably one should be "called" for this "work" by the Holy Ghost or the spirits through a dream, a vision, or a violent possession. Unlike the priests of the Afrobrazilian cults in Recife, the Jamaican revivalist leader at least pretends to favor the establishment of new churches and to encourage those who receive "calls" to "go out and spread the gospel of Jesus Christ."[36] Intense competition among revivalist groups in West Kingston results in spectacular gains or losses in the membership of a band, often within a short time. Rivalry and quarrels within a church, including disputes concerning succession to the office of leader, may produce secessions and the formation of new bands.

The future of Jamaican revivalism would seem to depend upon whether or not major changes occur in the economic, educational, and social conditions of the lower-class population and on the increase or decrease in appeal of such functional alternatives as the orthodox religious denominations, the Pentecostal sects, and an anti-white, back-to-Africa movement known as Ras Tafari.

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Notes

1. The field work upon which this paper is based was carried out in an economically depressed area known as the West End of West Kingston, on the edge of the capital. This study was made in 1953 with the support of a grant from the American Philosophical Society.

2. See Joseph G. Moore, *Religion of Jamaican Negroes, A Study of Afro-Jamaican Acculturation*, Ph.D. Dissertation, Northwestern University, 1953, Ch. 4.

3. L. Broom, B. J. Siegel, E. Z. Vogt, and J. B. Watson, "Acculturation: An Exploratory Formulation," *American Anthropologist*, 56 : 974, December, 1954.

4. R. Beals, "Acculturation," in A. L. Kroeber, ed., *Anthropology Today*, 1953, p. 628.

5. F. Tannenbaum, *Slave and Citizen*, 1947, pp. 82–83.

6. Phillippo writes: "For upwards of a hundred years after Jamaica became an appendage of the British Crown, scarcely an effort was made to instruct the slaves in the great doctrines and duties of Christianity; and although, in 1696, at the insistence of the mother country, an Act was passed by the local Legislature, "directing" that all slave-owners should instruct their negroes, and have them baptized, 'when fit for it,' it is evident, from the very terms in which the Act was expressed, that it was designed to be, as it afterwards proved, a dead letter—a mere political manoeuvre, intended to prevent the interference of the parent state in the management of the slaves." J. M. Phillippo, *Jamaica: Its Past and Present State*, 1843, pp. 267–268.

7. P. D. Curtin, *Two Jamaicas*, 1955, pp. 32–34.

8. *Ibid.*, p. 168.

9. For accounts of the Great Revival, see: W. J. Gardner, *A History of Jamaica*, 1909; Warrand Carlile, *Thirty-eight Years' Mission Life in Jamaica* (By One of His Sons), 1884; and E. B. Underhill, *Life of James Mursell Phillippo, Missionary in Jamaica*, 1881; and P. D. Curtin, *op. cit.*, pp. 170–171.

10. J. M. Davis, *The Church in the New Jamaica*, 1942, pp. 39–40.

11. See G. E. Simpson, *Jamaican Revivalist Cults*, 1956, Ch. 2.

12. J. M. Phillippo, *op. cit.*, pp. 247–249.

13. P. D. Curtin, *op. cit.*, p. 171.

14. M. J. Herskovits, "Introduction," in Sol Tax (Ed.), *Acculturation in the Americas*, 1952, p. 59.

15. For fuller accounts of these aspects of Jamaican revivalism, see G. E. Simpson, *op. cit.*, Chs. 3 and 4.

16. *The Sixth and Seventh Books of Moses*, pp. 9, 122, 139.

17. Ribeiro's comments about the personality of a devotee and the character of his protecting deity are interesting and they call attention to an aspect of the study of culture and personality which has been neglected. Referring to Afrobrazilian cult members in Recife, Ribeiro says: "The personality of a worshipper is held to be influenced by the 'character' of his patron deity. The behavior of some cult-members seems to fit admirably into the personality patterns of their gods. Worshippers of Ogun are to be found who are hard workers, as the god is supposed to be; those of Shango, who are inconstant and turbulent; of Oshun, who are facetious and inconstant. In naming the patron deities of persons who are not cult-members, several instances have been observed where the mystical character corresponded closely to traits of the individual's personality, a priest thus showing keen judgment of the temperament of the person with whom he may be dealing. A wide field for research would seem to be opened here, so as to determine the extent to which the 'guardian angel' assigned to a person correlates with the personality of the new cult-member, as well as to analyze the question, where the personality of the individual is not like that believed to mark his deity, how far unconscious identification has effected change in him. The hypothesis that, in the case of children who are indoctrinated with cult concepts and educated according to rules directed by their patron deities, an *alter ego* might develop on the basis of that indoctrination, seems equally worthy of investigation." René Ribeiro, *The Afrobrazilian Cult-Groups of Recife—A Study in Social Adjustment*, M. A. Thesis in Anthropology, Northwestern University, 1949, pp. 119–120. In 1940, Simpson wrote concerning *vodun* cult members in northern Haiti: "I was interested to find that three of my informants agreed that approximately half of the *fidèles* in the *vodun* cult have as their protectors one or more gods whose characters are the exact opposite of their own personality traits. We may have here, especially in the case of those who become possessed at ceremonies, an example of compensatory emotional release." G. E. Simpson, "Haitian Magic," *Social Forces*, October, 1940, p. 99.

18. M. J. Herskovits and F. S. Herskovits, "An Outline of Dahomean Religious Belief," *Memoirs of the American Anthropological Association*, 41 : 73, 1933; M. J. Herskovits, *The Myth of the Negro Past*, 1941, p. 253.

19. M. J. Herskovits, *Dahomey*, I, Chs. 19–20.

20. For accounts of revivalist death rites, see G. E. Simpson, *Jamaican Revivalist Cults*, Ch. 4, and G. E. Simpson, "The Nine-Night Ceremony in Jamaica," *Journal of American Folklore* (in press). Of interest here is a statement made by W. J. Gardner: ". . . Many superstitions, originally brought from Africa, were modified, and often blended with others which had been introduced by Christian or Jewish colonists. An idea, certainly not of African origin, is still current—namely, that the room in which a person dies should not be swept or disturbed for nine days. Water and other requisites are placed in it, and, among the Jews, a light is kept burning during the prescribed period." W. J. Gardner, *op. cit.*, pp. 391–392.

21. For similar practices in Porto Alegre, Brazil, see M. J. Herskovits, "The Southernmost Outposts of New World Africanisms," *American Anthropologist*, October, 1943, pp. 506–507.

22. M. J. Herskovits and F. S. Herskovits, *op. cit.*, p. 65.

23. For details on possession, see G. E. Simpson, *Jamaican Revivalist Cults*, Ch. 8.

24. M. J. Herskovits, *The Myth of the Negro Past*, pp. 232–233.

25. W. R. Bascom, "The Focus of Cuban Santeria," *Southwestern Journal of Anthropology*, Spring, 1950, p. 67; M. J. and F. S. Herskovits, *op. cit.*, p. 37; R. S. Rattray, *Ashanti*, 1923, pp. 190–191.

26. See G. E. Simpson, "Culture Change and Reintegration Found in the Cults of West Kingston, Jamaica," *Proceedings of the American Philosophical Society*, April, 1955, p. 90.

27. M. J. Herskovits, *The Myth of the Negro Past*, p. 239.

28. M. J. Herskovits, in Sol Tax, ed., *op. cit.*, p. 57.

29. M. J. Herskovits, *Man and His Works*, 1948, p. 553.

30. L. Broom, B. J. Siegel, E. Z. Vogt, and J. B. Watson, *op. cit.*, p. 984.

31. *Ibid.*, p. 984.

32. *Northwestern University Social Science Al Syllabus, Session 52, Source II*, Table I.

33. L. Broom, B. J. Siegel, E. Z. Vogt, and J. B. Watson, *op. cit.*, pp. 990–991.

34. M. J. Herskovits, "The Present Status and Needs of Afroamerican Research," *Journal of Negro History*, April, 1951, p. 145.

35. An interesting statement concerning the main channels leading to interest in the Afrobrazilian cult groups in Recife, Brazil is given in René Ribeiro, *op. cit.*, p. 108.

36. René Ribeiro, *op. cit.*, p. 123, reports that in Recife: "Sometimes a cult-member feels competent enough to lead his own group, or his god 'asks him' to inaugurate a new cult-house. When his own priest does not support this project, thus keeping this satellite group under his influence, the new house is founded despite the knowledge that magic will be used by the frustrated cult-head to retaliate. This, however, is one of the dangers a priest has to cope with, and this is why he does not rely too much on his subordinate, or volunteer too much information about his control of the supernatural. . . ."

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PLOUGH AND FIELD SHAPE

Axel Steensberg

In 1948 Mr. Ffransis Payne of Cardiff put forth the view that reasons for changes in plough types always have been due to changes in the technique of ploughing. In consequence, the ploughwrights adapted their ploughs accordingly; and they were not creating new field shapes by accidental invention. Such things as coulters, plough-wheels, mould-boards, shares and plough-teams are effects, not causes. They did not of themselves create or alter field shapes (The Archaeological News Letter, November 1948).

This quotation may serve as the theme of my lecture.

The connection between the plough and the field is a functional one. Plough and field shape are together functions of the economic and social situation at a given place and time. They often reflect the essential trends of the culture pattern. If we have the plough or the fields preserved from ancient times a great many misinterpretations of written sources can be avoided. The "International Secretariat for Research on the History of Agricultural Implements" was founded at the National Museum of Copenhagen in 1954, and it is intended to act as an aid for nearly a hundred ethnologists and archaeologists from different parts of the world who are working enthusiastically in the description of ploughing implements and the investigation of datable fields from antiquity to modern times. I believe that these remains constitute a necessary control of written descriptions by the classical authors, Virgil, Varro, Plinius and Columella, as well as of medieval pictures and printed statements from the 16th to the 18th century.

A great many locations of prehistoric fields which were tilled by the plough without a mould-board—the so-called ard type—have been investigated and mapped in different countries of northwestern Europe. But parallel to the fields ploughed by some traction implement we may discern another line of development, the origin of which must be at least a thousand years older than ploughed fields. This type of field must have been tilled with the spade. Van Giffen of Groningen in the Netherlands has discovered the occurrence of such high-backed fields about 600 B.C. Professor Grahame Clark of Cambridge has shown me some fields of nearly the same type from Roman time situated at Cottenham Fens near Cambridge. In Norway Anders Hagen found some narrow ridges running transversely under a burial mound of the Migration period, and a month ago the Museum at Ålborg in Denmark excavated some twenty-five narrow high-backed ridges which could be dated by coins and a house site placed above them to the Viking age about A.D. 900–1050. These field ridges are very narrow, most of them 80 cms, some few not more than 50 cms wide. It is likely that they were tilled by the spade. In the Hebrides and the Faroe Islands north of Scotland high-backed ridges of a similar character are still being dug today by the aid of the spade.

For comparative purposes it would be very interesting if anthropologists would publish detailed descriptions of such ploughless agriculture from other parts of the world accompanied by maps on a large scale with contours and cross sections of the strips and subdivisions of the fields, as well as detailed descriptions of the use of the digging stick or the mattock, and the annual agricultural cycle. Professor Izikowitz's paper on the Lamet people of the upper Mekong river in Indochina may be taken as a model for what is needed.

The methods of ploughing have been described more thoroughly in the past than the methods of digging, but I venture to say not thoroughly enough. At different places in northwestern Europe marks have been found, derived from cross-ploughing which is the characteristic way of tilling fields with the ard. In the periodical *Kuml* at Århus in 1954 Mr. Kjaerum published such a pattern of cross marks found under a burial mound which can be dated to the Single Grave period (Middle Neolithic, c. 2000 B.C.). It is likely that these furrows were cut by the crook-ard, a type which had a separate sole (the Mediterranean type or "Triptolemos Plough"), and the cross-ploughing was a simple way of cutting through the balks between the furrows. At the place mentioned here there was about 20 to 25 cms between the furrows. The practice of cross-ploughing with the ard is common in southern Europe, and in Sweden it was utilized up to this century.

However, we have a description of another way than cross-ploughing tilling with the ard. Columella states that on the light soils of Numidia and Egypt, where the fields are sown with grain and need no deep ploughing, the ploughman must alternately walk upon the broken ground holding his implement slantwise to break the balks or *scamna* between the furrows which he traces in the normal way with the implement running upright.

In modern times Professor Ränk of Stockholm has described the patterns of the furrows in a field in the island Ösel in Esthonia. The crook-ard here caused the nearly square fields to be rounded at the corners in order to reduce the breadth of the headlands along the borders of the field on which the animals turned. So the ploughman would begin the turn a little before the animals reached the headland. The idea is similar to the reversed S-shaped fields of the English open fields (see Eyre in *Agricultural Hist. Rev.* III, 2, 1955) which would result from the desire to avoid having the large plough team damage the neighbouring fields. However, such turnings of the fields near the headlands was not a characteristic feature of the investigated open fields of Denmark.

Proceeding to the fields produced by the real mould-board plough I had hoped to announce the date of a fragment of a plough of the mould-board type found at Andbjerg in Denmark (Glob: Ard and Plough in Ancient Scandinavia, Århus 1951, pp. 73 and 122). Unfortunately the pollen analysis of the 12 m.-deep bog profile into which the remains of the plough are to be fitted has not yet been completed, and moreover Mr. Jörgensen and Dr. Troels-Smith of the Department of Natural History at the Danish National Museum wish to take out samples of peat from the stratum where the implement was found in order to date them by Carbon 14. This procedure cannot be finished before next winter, and until then it cannot be said with certainty whether or not the mould-board plough is prehistoric in Scandinavia.

Of course the climatic change in the early Iron Age which may have caused the growth of open areas covered with grass would have favoured the use of a plough with a mould-board which was capable of turning over the solid turf when or if grassland were ploughed up. However, most new arable land was gained by the slash-and-burning method (see Steensberg in *Kuml* 1955), and we do not know if the ploughing up of grassland was a common practice. One of the best evidences of village fields has been found by Professor Hatt at Skørbaek heath in Jutland. About the beginning of our era a village existed here consisting of four farms surrounded by numerous fields of the "Celtic" type hedged in by low earthen walls. In some parts of the cultivated area there seems to be a recurrence of blocks of four fields corresponding to the four farms in the village i.e., a tendency toward a division of the arable land into shots or furlongs as in the Medieval open fields. It is likely that every farm had one plot in each of these shots, i.e., that a system of noncontinuous holdings much resembling the "open field system" had been developed in this village by about the birth of Christ. My idea is that such a pattern of noncontiguous holdings may arise at any time when a village has occupied all the surrounding arable land as, apparently the Skørbaek village had done. Some of the fields at Skørbaek were rather long and narrow, especially those where a regulation into a four-field system seems to have been introduced. Could it be that such regular long and narrow fields were cultivated by some ploughing implement with a wheel-frame at the front, in which case the ploughman would wish to avoid too many turnings at the ends? But as will be demonstrated now it is not necessary to assume that such an implement had a single mould-board, or at least that a mould-board was attached to it for all cultivating purposes.

I shall now report on my investigations and mapping of the open fields of a village called Borup in Sealand, Denmark, an investigation under the auspices of the Carlsberg Foundation which has been in progress for the last six years and which is not likely to be finished before 1960. Borup village existed from about A.D. 1000 until it was abandoned some time in the 14th century. From about 1400 until the present most of this area has been covered by woods, a smaller part around the former village site remaining pasture, as can be seen from the air photograph. The soil is a rather sandy glacial clay containing a great many small and large boulders. These boulders have been gathered in the furrows between the different strips and lands of the open fields. These belts of collected boulders are mostly covered by soil. Our practice is to sound the area with a prod, mapping on a scale of 1:250 every stone more than 10 cms in section, that is to say several millions of stones. The belts of gathered stones make the field divisions clearly visible. Some of the belts must have constituted visible stone balks, apparently separating the strips belonging to different owners or tenants of the village farms. Where the axis of ploughing has been altered from time to time and the old balks have been cut through, the boulders have been gathered into nearly circular heaps.

Many scholars have held the opinion that the lands, usually called ridges, of the open fields in Medieval time were high-backed ridges (in German "Hochäcker"). But in Borup these ridges and furrows were not introduced until about 1400. In the earlier parts of the fields of Borup the areas between two furrows are all quite flat. One might assume that those lands were ploughed with a plough without a single mould-board, that is to say an ard perhaps with a wheel-frame in front such as was in use in the Swedish province of Scanie until c. 1850, the "Wheel-ard". After the seed had been sown and the land harrowed flat the ploughman ploughed some drainage furrows between the "lands", i.e., in some cases just in the middle of the belt of boulders, in other cases when a belt was filled too compactly with boulders he made one furrow at each side of it. It is important to notice that in both cases the belt of collected boulders served as a sort of stone-drain such as Columella mentions in *Res Rustica*, II, II, 10. The difference in level between the middle of the lands and the dividing drainage furrows does not exceed 10–15 cms. Generally the breadth of the lands is between 3 and 9 meters.

In 1404 the nobleman Anders Lunge of a neighbouring village bought some of the abandoned fields of Borup situated between a hilly ridge and the Tystrup lake. This area he ploughed up into real "ridges and furrows" about 50 cms high. From other localities we know of plain fields right down to the end of the Middle Ages, and the high-backed ridges of Borup are the very first of this type to have been dated. Did a climatic change about 1300 make this kind of ploughing more necessary? For demonstration I have made a map of some characteristic parts of those high-backed fields including the furrows and the contours only, but excluding such details as boulders and irregular depressions. The shots or furlongs run downhill as usual, but where the slope of the hill is too steep the furrows often are placed in an oblique direction to the contours in order not to produce too heavy an erosion. In some cases the ridges have been placed very close to the upper drainage furrow. This served to keep the water in the furrow and to prevent its flow across the land to the lower furrow with consequent erosion. This measure has been used at some places in Finland in recent times according to Professor Vilkuna's statement. Anders Lunge ploughed these ridges and furrows for some few years only, and after his time the woods claimed the high-backed fields.

In Denmark I have mapped another location of medieval fields at Alstrup heath in Jutland. This place seems to have belonged to an isolated farm which was abandoned before 1487. The arable land of Alstrup was hedged in by dikes consisting of earth and boulders by means of which it was divided into four or more fields. Apparently nearly half of those fields were cultivated for four or five years, and afterwards they lay fallow for a similar period. Between two of the dikes a series of very small and short lands, 5 to 7½ meters broad, are preserved, the profile or section quite flat, but the furrows between the flat lands about 7 to 10 cms deep. At another place, near Lund in southern Sweden, there are equally flat late Medieval lands divided by furrows of 10–15 cms depth. The width of the lands is mostly from 6 to 10 meters.

The conclusion is that Medieval fields are not exclusively of the high-backed "ridge and furrow" type, and that there may be balks separating the strips of different owners in the Danish open fields—a statement which corresponds well with that of some old Danish village laws from the 17th and 18th centuries. Apparently the Medieval fields with a flat surface are often a function of the wheel-ard which was in use until modern time in southern Sweden.

I have chosen these examples to show how we may elucidate one trend in the culture pattern by another. The study of field shape, balks, and drainage makes intelligible the method of ploughing and at the same time supplements our knowledge from written documents and pictures.

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A UMATILLA PROPHET CULT: AN EPISODE IN CULTURE CHANGE

Theodore Stern

Within the Plateau area of the American Northwest, the midpart of the nineteenth century was marked by frictions engendered by white immigration.[1] Dislocations of the native population, epidemic disease, and intermittent hostilities were responsible for an atmosphere of tense unrest and suspicion, faithfully revealed by the chronicles of the day. Within that anxiety-laden clime, there flourished what has variously been termed the Washani or Prophet cult, a syncretic religious movement which drew from both native and Christian sources and found manifestation in a number of congregations, each clustered about its prophet and his revelations. That the Washani movement was linked with the native past is clear from Spier's meticulous research,[2] though the extent of that continuity still remains at issue. For the movement as it flourished after 1830, features thus shared with the earlier faith may have served as a bridge for the believer. In a time of stress, the doctrinal appeal of a message, often messianic, always hopeful, is not to be gainsaid. The later, Christianized form of the church, however, incorporated changes of function and structure which encroached upon that segment of the native faith known as the power quest. The present paper passes accordingly to the consideration of those factors, in order to investigate their bearing upon change.

The focus of our view will be principally upon the Umatilla, a Sahaptin-speaking tribe along the middle Columbia. By the time that fully-developed Washani practices are noted among them, the Umatilla had long since been in possession of firearms and the horse, which had tended to create a cultural differentiation within the body of the tribe, such that the Upper Umatilla, extending their contacts east of the Rockies, came to take on a Plains cast, in contrast to the River group, which, wintering in the valley of the Columbia, continued to stress heavily the life they had long known. It was among the latter that the Christianized Washani movement seems chiefly to have flourished, replacing, though it did not obliterate, aspects of the earlier religion.

Among the Umatilla, as throughout so much of the surrounding area, the avenue to supernatural power formerly lay through the power quest, characterized by individual experience and its subsequent public validation. Its major outlines are well known in the Plateau.[3] The boy or girl, from perhaps the age of ten to puberty, and less commonly thereafter, was sent out to a sacred questing place in the mountains, taking along some object to be left atop a pile of stones, as a sign to his mentor that he had faithfully visited that spot. At the sacred place, the novice remained for up to five days, performing certain ritual acts and awaiting the advent of a compassionate guardian spirit that came in the early dawn, taking sometimes human, sometimes natural, guise; and not infrequently making its presence known solely by the song with which it conferred power. The unsuccessful novice returned home, perhaps to go forth again at a later day. Were a vision vouchsafed him, he returned at once to the village, only to fall unconscious at the outskirts. Revived, or "cured", by a shaman, he returned home, eventually to resume a normal existence. He might, indeed, go out on other quests to add to the power within him. That power remained latent—some informants, indeed, doubt that it continued to reside in him during that period—until the time, perhaps ten years later, when at the Winter Spirit Dance he heard someone sing a song appropriate to his own guardian spirit. At once he fell, senseless; and when a shaman revived him, awoke with his own song upon his lips, and rose to dance. Only in this manner did he disclose the identity of his guardian spirit, in a public display that was deemed indispensable to the validation of the power quest. The tale is told of a boy, home from boarding school, who in the midst of the Winter Spirit Dance went appropriately into trance. "He had never been on the quest," explained the informant, "and this was the first sign he had that power had been granted him."

A number of features make plain the essentially individualistic nature of the quest and at the same time how little it was deemed to be subject to the control of humans.

1. The decision to send the child upon the quest was largely optional with the parents, many of whom traditionally declined to expose a son or daughter to the hardships of the quest, or to the danger inherent in acquiring but a weak guardian spirit.

2. Humans had little choice over the form of power acquired. There were, to be sure, occasions when power was directly transmitted. One woman, on her deathbed, told her kinsfolk, "My song will be travelling in the air. You listen! It will come to you." If unworthy, however, the recipient could not retain the song; while there were always those, more powerful than he, who strove to usurp it. Power might be bestowed, again, if the mentor gave the novice a sacred object to hold while on the quest, in the hope that the associated guardian spirit would appear to the boy. Without doubt, however, the most frequent type of power acquisition was that in which the novice might be freely visited by any guardian spirit that chose to do so. If unwilling to be subjected to a spirit that had conferred power upon him, the novice might sometimes have it exorcised; but the act was dangerous to him and the event by no means assured. There was, on the whole, little human control over the selection of power.

3. The qualities so acquired were amoral, and served only to enhance the abilities of the recipient. A man endowed with Weasel power could steal without being detected; but whether he used it to levy horses from the enemy or to thieve from his fellow-tribesmen remained a personal option.

4. The tutelary spirit conferred highly specific powers, often associated with the qualities attributed to the bird, animal, fish, plant, or inanimate object with which it was identified. A guardian spirit might have several powers to convey, in hunting or fishing, in root-digging, in racing, in the amassing of riches or the attraction of women, in war, or in curing, to mention but some among them. In one vision, here narrated of the related Cayuse, but said to have its Umatilla counterparts, the novice received his power from a rotten stump, riddled with woodpecker holes, which appeared to him in the guise of a person bearing many wounds, and declared, "Boy, you see my body! That's the way you'll be; you'll have wounds all over, but they won't kill you. You'll be tough!"

5. By virtue of being specific, the powers were situationally restricted in their exercise; indeed, some were no more than crisis powers, only intermittently useful. There are a number of instances in which the guardian spirit

is said to have enjoined the novice from certain articles, foods, or acts. In that emergency when he must summon up his power, he had recourse to the tabooed item. Thus, a man whose power-substance was the huckleberry might carry some in a bag, that they might be at hand in time of need.

6. Shamans, who secured their powers in much the same manner as did the ordinary individual, were distinguished from him principally by exercising those attributes in such a way as to affect others. An ordinary individual might be induced to try his hand at curing and, discovering that his power was effective, would thenceforth be deemed a shaman. A public demonstration of his ability followed. In point of prestige, there was a gradation from those shamans who merely cured, up through the *waptipäs tiwät*, who was a hunting shaman and gave competitive exhibitions handling hot stones during the Winter Spirit Dance; finally to the *isχi·p tiwät*, who, by virtue of having assimilated the guardian spirit of a deceased person, had access to the powers of others, diagnosed disease, and located lost objects. In practice, there was much functional overlap, since a shaman might acquire a number of powers (*ta·χ*) and these might in turn attract other, power-like elements.

7. Unlike such Plains tribes as the Oglala Dakota,[4] possession of the same tutelary spirit did not lead at Umatilla to the formation of a dream cult. The reason seems clear: in at least some instances, a guardian spirit might confer different powers to two novices; while conversely two such dissimilar spirits as wolf and deer could both bestow hunting prowess. Accordingly, there was no invariant and exclusive identity between a given power and the donor spirit. Moreover, the Umatilla were teticent about broadcasting the identity of a protector: only when he was invoking the tutelary spirit did the individual explicitly refer to his vision. For the shamans, there seem to have been neither schooling nor formal apprenticeship, though a shaman had his assistants; and shamanistic societies are said to have been lacking.

8. As a final instance of the autonomy inherent in the Umatilla spirit quest, there are the dangers which constantly threatened the individual from the powers exercised by his fellows. Very young children, straying into the axis of vital being (*heśin*) which invisibly preceded and followed powerful shamans, were suddenly stricken and died. The *isχi·p tiwät* in particular, said always to have been an aged person, was temperamentally unstable and moody; and children were kept out of his sight, lest he be reminded of his own dead offspring and bring these to harm. A person with but a weak tutelary spirit was at the mercy of more powerful individuals, often of the same tribe, who might appropriate his power and so encompass his death. As a person grew old, his power gradually waned, the song going last, and he became more vulnerable to its theft. A sudden death not resulting from the ordinary accidents of life was conventionally interpreted as being the result of power-abduction. The projection of a power into the body of the victim led often to illness which might, if unchecked, likewise result in death. While there were shamanistic defences to aggression on this plane, the picture is strongly suggestive of intra-group tensions, enhanced by a view which attributes the inevitable events that attend life to the uncontrolled power or malice of one's fellows.

Thus it is that the possession of guardian spirit power was marked among the Umatilla by individualistic autonomy, intermittence and restriction of operation, and an indifference to the moral order that permitted its exercise in acts of aggression against fellow-tribesmen. If much cohesion was imposed by other cultural agencies—ethical behavior for example was taught the child from an early age, while family and band solidarities were distinctive features of Umatilla society—the area of the religious system most directly the concern of the ordinary tribesman remained but loosely structured.

By contrast, the more recent Washani developments present a picture at once highly ordered and compact. While adequate data are not at hand for the earlier Umatilla prophet, Wiletsi, placed by Dubois in the 1850's,[5] the later cult of Luls, a contemporary of Smohalla, will serve us. When Luls slew a shaman whom he suspected of having brought about the death of a beloved daughter, the girl appeared to him in a vision, reproaching him for the act, which, she declared, forever barred him from joining her in heaven. Later, the Unseen Person, the Creator, made himself known to Luls, promising him remission of his sins if he preached to his tribesmen for ten years. On subsequent occasions, the same Being taught him songs and granted him visions of heaven. Luls established his church at the village of Imatillam, and drew to himself a congregation, to whom he detailed his visions and prophesied future events. In major details, the visions, the meeting house, the religious paraphernalia and costumes, the Sunday services and funerals, and the incorporated first-fruit rites all correspond broadly with the Washani cult known among other tribes, although a messianic message is largely absent from the Umatilla doctrine.[6] Here it is sufficient for our purposes to note some of the salient structural contrasts which it presents to the power quest.

1. A single series of related visions, that of Luls, served to validate religious experience for his entire congregation and establish once for all the lines of orthodoxy.
2. An ethically regulated order was supernaturally confirmed, in which the moral worth of the worshipper ensured prosperity in this life and his fate in the next. Implicit in that order was a universal dichotomy, marked by heaven and hell.
3. A short-term periodicity is inherent in the seven-day worship cycle, distinct from the seasonal recurrence of Winter Spirit Dances and first-fruit rites.
4. All power emanated from the Unseen One and, though available to the worthy, was invested permanently in no mortal. As a *supaikani*, one guided by the Unseen One, Luls was able to effect minor miracles, but it is clear that he himself had no special power. Unlike adherents of the Shaker and Feather religions, other syncretic faiths of the Northwest, the Washani does not cure. At most, Luls might direct that the sick be brought into the church to receive with other worshippers the benefit of song and prayer.
5. There was but a single prophet. Luls does not seem to have designated a successor; and when, shortly after his death, the villagers moved back upon the reservation, where Christian missions were firmly established, his church dissolved. During his lifetime, the prophet presided over an egalitarian congregation. Any individual of good standing might serve as preacher in the Sunday services, any person dance, confess, and give testimony.

To the contrast just posed certain qualifications must be made. First-fruit rites in their aboriginal form undoubtedly involved the individual in seasonally-regulated, corporate activities, as much a part of his religious life as those aspects centering upon the power quest. With the development of the Christianized movement, however, a certain exclusiveness set in, such that some adherents stigmatized the power acquired by questing as a thing of the devil. Particularly because the absence of well-defined curing practices in the

Washani church led to recourse to shamans, the exclusiveness was never complete, and a number of individuals continued to adhere to both forms of faith. We are speaking here, then, of a trend, rather than a saltation.

In the development of the Washani church at Umatilla, it is possible to see the prior model, not alone of the missionary and his congregation, but also of the chief and his followers. As Spier has shown, in the early forms of the Christianized Washani movement, it was the chiefs who led Sunday services; and this may in turn have served to reinforce the secular eminence of successful prophets. Luls himself, while his background is obscure, seems to have grown in power with his church, so that eventually he had recourse to the whipper, often a chiefly prerogative, in the enforcement of religious mandates. While he is not spoken of as engaging in the usual warlike pursuits of the chief, he has in the past been termed a mighty man, while the agent on Umatilla reservation could speak in 1872 of "Lalse's band," as if he were indeed the leader at Imatillam.[7] The event is broadly similar to the emergence of the chiefly prophet, Too-hul-hul-sote, of the Nez Percé, and among the Sanpoil in the rise of Skolaskin.[8]

There is much in the transition from power quest to Washani church, in a time of anxiety and cultural stress, that might be described as functionally adaptive. While the immediate agent is the individual, attracted in the religious sphere by such qualities as doctrinal appeal, familiar aspects, and opportunity to identify with a congregation, the self-same act is advantageous for the culture as a whole in the replacement of a system marked by individual autonomy and tension with the Washani church, with its compact and harmonious structure, operating within a supernaturally sanctioned moral order.

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Notes

1. The interpretation set forth in this paper has arisen in the course of field-work still being carried on among the Indians of the Umatilla reservation. It is to be regarded as tentative and subject to modification by further research. Grants from the Faculty Research Fund of the University of Oregon and the American Philosophical Society have thus far supported the project.

2. Leslie Spier, The Prophet Dance of the Northwest and Its Derivatives: The Source of the Ghost Dance. *General Series in Anthropology*, No. 1, Menasha, 1935.

3. See, for example, Verne F. Ray, Cultural Relationships in the Plateau of Northwestern America. *Publications of the F. W. Hodge Anniversary Publication Fund*, 3 : 68–131. Los Angeles, 1939.

4. Clark Wissler, Societies and Ceremonial Associations in the Oglala Division of the Teton-Dakota. *Anthropological Papers, American Museum of Natural History*, 11 : (11) 81–99. New York, 1912.

5. Cora Du Bois, The Feather Cult of the Middle Columbia. *General Series in Anthropology*, 7 : 12, Menasha, 1938.

6. See James Mooney, The Ghost Dance Religion and the Sioux Outbreak of 1890. *Annual Report, Bureau of American Ethnology*, 14 : (2) 708–745, Washington, 1896; and Spier, p. 41.

7. Du Bois, p. 12; letter of N. A. Cornoyer in Report of the Commissioner of Indian Affairs, contained in Report of the Secretary of the Interior (Exec. Doc., H.R., 3rd Session, 42nd Congress, 1872–1873, Vol. 1), p. 747.

8. O. O. Howard, quoted in Spier, p. 48; Spier, p. 56, and Verne F. Ray, The Kolaskin Cult: A Prophet Movement of 1870 in Northeastern Washington. *American Anthropologist*, 38 : (1) 67–75, January–March, 1936.

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CART-USING INDIANS OF THE AMERICAN PLAINS

Omer C. Stewart

The Plains Ojibwa, known also as Saulteurs or Chippewa of the Plains, extended the territory of the forest-dwelling Ojibwa on the Great Plains of the United States and Canada at the end of the 18th century. By 1800 the Ojibwa had accompanied the French and English fur traders as far west as the Turtle Mountains in the present state of North Dakota. Guns and ammunition, and, finally, horses and carts obtained from the fur-traders made possible this Ojibwa conquest of Sioux Indian territory. Ojibwa came to control a great triangle of territory on the treeless plains divided about equally between Canada and the United States. The apex of the triangle was at the Turtle Mountains; the base was along the Red River of the North for almost its entire length.[1]

The extension over this part of the plains by the Indians who had previously been forest and lake dwellers dependent upon the birch-bark canoe has been recognized for over a century. The fact that the Plains Ojibwa exploited the buffalo herds by means of two-wheeled carts has been overlooked. It has come as a shock to a number of anthropologists to realize that one group of American Indians used a two-wheeled wooden cart, instead of a travois, to transport their tents, their dried buffalo meat and hides. The wooden cart of the Plains Ojibwa was, like the horse, acquired from Europeans in post-columbian times, and the two aspects of culture were together used to replace the birch-bark canoe when the Ojibwa left the lakes and rivers and adopted the life of buffalo hunters.

Anthropologists have known of the two-wheeled wooden cart which was used in great numbers along the Red River of the North, of course, because it was described in detail by a number of early explorers beginning with Alexander Henry who taught the Ojibwa how to manufacture the cart in 1802.[2] America's first anthropologist, Lewis H. Morgan, saw the carts in use when he descended the Red River to Fort Gary (modern Winnipeg) in 1861. In his journal, now being prepared for publication by Professor Leslie White of the University of Michigan, Morgan mentioned the use of the cart in the buffalo hunt, but emphasized its role in long-distance transportation. He reported that "eight to nine hundred . . . of these carts crossed the [Red] river in one day at Georgetown." It appears to have been the fâme of the Red River cart for general transportation from Fort Gary, Manitoba, to St. Paul, Minnesota, which obscured its use by the Plains Ojibwa for buffalo hunting. Furthermore, the cart drivers of the Red River became known as the *Metis* or *Bois brulés* or Half-breeds, which allowed people to forget that the vast majority of those along the Red River were Ojibwa Indians of mixed parentage who also participated in the great Plains Ojibwa buffalo hunts. The justification for designating the Plains Ojibwa as "The Cart-using Indians of the Plains" comes with this recognition that the *Metis* or Half-breeds of the Red River area were usually Ojibwa half-breeds and were then and are today Ojibwa Indians.

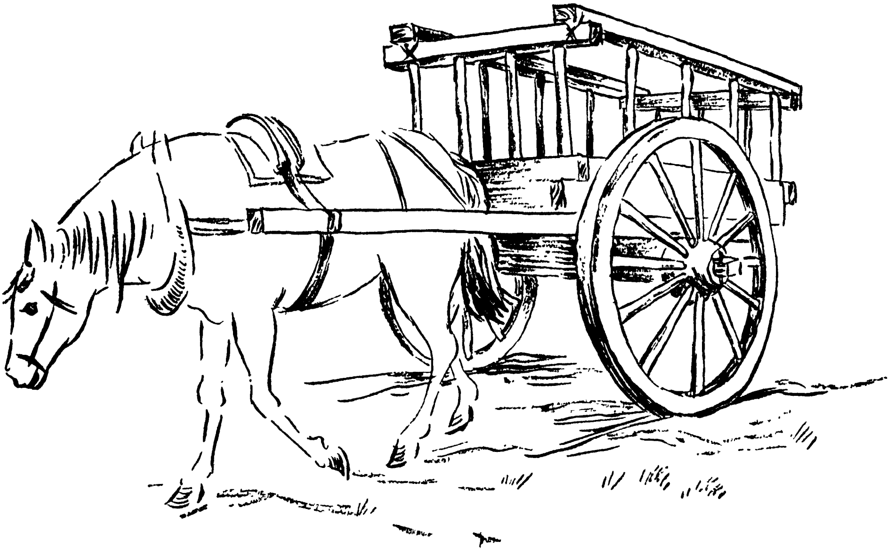

Fig. 1. Plains Ojibwa Cart

To designate half-breeds as Indians is to use cultural rather than biological criteria, with which to determine the classification. Over a century had passed from the time the Ojibwa first met the French traders and trappers until the establishment of the Ojibwa on the Plains. Historic records show that white men had been welcomed into the country and invited into the beds of the Ojibwa. Alexander Henry, literate trader at Pembina (now North Dakota) in 1800, was married to the daughter of the Ojibwa chief of the area.[3] In 1849, Captain John Pope explored the Red River of the North for the U.S. Government and reported 1,000 half-breeds in the United States and 7,000 in Canada. He explained that the large numbers of mixed bloods resulted from the interbreeding after the establishment of the Selkirk Colony along the Red River in 1812. Pope wrote:

> "Large numbers of Indians being soon attracted to the settlement by the presence of so many strange people and the display of so many tempting articles for traffic, and many of the colony being at once induced to take to themselves wives, in a few years the half-breeds who resulted from these connections amounted to several thousands."[4]

The Selkirk Colony was established almost entirely within the territory which had been shortly before conquered by the Ojibwa during their expansion onto the plains, and most of the half-breeds spoke the Ojibwa Indian language. At the birth of a child with white father and Indian mother it was impossible to say whether the child would become an Indian or a white man. If the white father kept the family together and educated his children they might be absorbed into the white community. A number of half-breeds were thus lost to the Indian community. However, the fact that there continue to be large Ojibwa Indian populations in North Dakota, Minnesota and Manitoba is proof that many half-breeds remained Indians culturally, politically and emotionally.

In 1915 Aleš Hrdlička made a study of the physical anthropology of the Ojibwa of the White Earth Reservation in Minnesota upon which part of the Plains Ojibwa had been settled by the U.S. Government. His anthropometric measurements and careful observations of 696 Ojibwa were the basis for his writing that

> "... a large proportion of the tribe was found to be mixed with whites in all possible grades, while relative few are mixed with Indians of other tribes ..."[5]

Hrdlička's research led him to conclude that less than 10% of the 25,000 Ojibwa of 1915 were full-bloods.

Oblate Father Laviolette of The Research Center for Amer-indian Anthropology at the University of Ottawa in a report this year on the "Aborigines of the Prairie Provinces" wrote that:

> "It is doubtful whether there are many full-blooded Indians in any of the three Prairie Provinces."[6]

Father Laviolette explained that the 1951 Canadian census recorded all persons living on Indian Reserves as "Native Indian." About two thousand Plains Ojibwa remain on small reserves in their ancient territory in Manitoba and about three thousand were in North Dakota in 1910.[7]

Many of the Plains Ojibwa found their way to other reservations when the buffalo were finally exterminated. Frank Desjarlais may be taken as an example. Frank's father was a half-breed trader who married an Ojibwa woman at Red Lake, Minnesota, but took his family to live for a while at Fort Gary. Frank returned to work for Mr. Kittson, a half-breed American trader at Pembina in 1844, and participated for about twenty years in the great Plains Ojibwa cart treks for buffalo. After a period as a U.S. Army mail carrier at Fort Totten, North Dakota, Mr. Desjarlais settled on the Red Lake Indian Reservation and became an Ojibwa medicine-man, active in 1910 at the age of 85.[8]

Since the traders often travelled beyond the limits of any single Indian group and might travel with wives acquired from any group, there could be a number of individuals from different areas and tribes connected with the trading posts within the Plains Ojibwa area. Lewis H. Morgan found informants, usually wives of traders and clerks at trading posts, from about a dozen widely scattered tribes during his trip down the Red River to Fort Gary in 1861. A single informant could usually adequately give the kinship terms Morgan was collecting. Notwithstanding the possibility for inter-tribal mixing, both the half-breeds and Indians of the Red River of the North maintained Ojibwa as the native speech, supplemented by French or English.

Many details of the spring and fall hunts on the plains during which carts were used to transport whole families as well as up to 500 pounds of dried meat, hides and tallow, come from the accounts written by the Oblate Fathers who accompanied the hunters in order to say Mass and to teach the catechism in the Ojibwa language. Father Belcourt, writer of an early Ojibwa Grammar and Dictionary, is the best known.

In 1855, Father Belcourt was missionary at Pembina and wrote at length regarding a peace-treaty between the Sioux and Plains Ojibwa at which the two peoples agreed to an inter-tribal boundary. This followed the defeat of the Sioux by the Ojibwa. The secret of the Ojibwa success over larger numbers of

Sioux was the two-wheeled carts arranged in a circle to form a fortified corral and strong point. Following the peace talks and agreement, Father Belcourt reported that Sioux and Ojibwa gathered before his portable chapel and heard Mass. He wrote:

> "Feathers, paint, head dresses, all arranged according to the various tastes, gave this new audience the most imposing and stately appearance for a man who knows enough to lay aside what is odd. . . . All clustered in a body, as far as the foot of the altar. The music and the singing began; the chapel was adorned as for a first class festival; everyone was enraptured; you might have thought they were all old Christians." (Translated from French.)[9]

One of the fullest accounts of the Plains Ojibwa hunting expeditions comes from Alexander Ross, a fur hunter of the far west, who settled in Winnipeg in 1825 after a trip to Oregon, and wrote a history of the Red River settlement which was published in London in 1856.[10] Ross reported (p. 13) that the Ojibwa had acquired their plains territory about 1780, but by 1817 when the Indians ceded some territory along the Red River to Lord Selkirk they were first mentioned and were acknowledged the most powerful tribe.

Ross (p. 246) gave statistics on the early hunts that assembled at Pembina for the years 1820 to 1840. The number of carts which assembled for the spring hunt increased from 540 in 1820 to 1,210 in 1840. He estimated that the 1840 hunt cost 24,000 English pounds for equipment and time employed. There were 403 buffalo-running horses, 655 cart horses and 586 draught oxen. 1,630 persons and 542 dogs also participated. Jean Baptiste Wilkie, an English half-breed according to Ross (p. 248) and a recognized Ojibwa chief according to Ojibwa at Turtle Mountain in 1900, had absolute control over the hunt. Under the influence of the Catholic priest no hunting was allowed on the Sabbath-day (p. 249).

From Pembina, close to the Canadian border, the hunters went 150 miles south to the Sheyenne River then northwest to the vicinity of Devil's Lake, a total distance of about 250 miles, before the herd of buffalo was sighted. Twenty-five hundred animals were killed but much was wasted, according to Ross, who also reports an encounter with some Sioux along the Sheyenne River. Some Ojibwa attacked and killed some Sioux ten miles from the main Ojibwa camp. The Ojibwa retreated to the safety of the main camp where the Sioux decided they were too greatly outnumbered to attack the entire camp. Ross (p. 270) criticized the half-breeds for protecting the raiders who had murdered their ancient enemies and thus added fuel to the flames of Sioux-Ojibwa warfare.

Although Ross and others often wrote as if the half-breeds were distinct from the Ojibwa Indians, Ross also provided the evidence that the Red River half-breeds were Ojibwa. He wrote (p. 270):

> "The Saulteaux [Ojibwa] and half-breeds, be it remembered, are mostly all related, either by marriage, or other kindred ties. . . . It was proved afterwards that a half-breed named Parisien was with the Saulteaux in the fight, and had actually fired the first shot."

The cart of the Plains Ojibwa was similar to the two-wheeled cart of Spain and Russia of the 19th century, with the exception that no iron whatsoever was used in the Red River cart. The wheels were large, up to five feet in diameter. The number of spokes varied from 4 to 12. The axle of tough wood, which tapered to receive the wheels, was never greased; consequently, loud squeaking accompanied the movement of the cart caravans. Upon the axle was mounted a light box with a pair of shafts to which the animal was harnessed with strips of rawhide. Ross (p. 244) estimated the cost of the cart in 1840 to be one pound ten shillings. Carts traveled upwards of a thousand miles per season and yet lasted several years because of the almost complete absence of rocks and stones in the deep alluvial soil of the Red River Valley.

In summary, it may be said simply that the Plains Ojibwa of North Dakota and Manitoba for about a half-century, 1815 to 1865, employed two-wheeled wooden carts for their spring and fall buffalo hunts. It seems proper to designate the Plains Ojibwa as the Cart-using Indians of the American Great Plains.

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Notes

1. A. L. Kroeber shows the territory on his Map 1a, "Native Tribes of North America," in his monograph *Cultural and Natural Areas of Native North America*, Vol. 38, *Univ. of Calif. Publ. Am. Arch. and Ethn.*, 1939.

2. Elliot Coues (Ed.), *New Light on the Early History of the Greater Northwest. The Manuscript Journals of Alexander Henry and David Thompson, 1799–1814.* New York, Francis P. Harper, 1897, Vol. 3, pp. 204–205. Under date of Sept. 20, 1802, Henry wrote at Pembina: "We have enough horses for all purposes, and a new sort of cart which facilitates transportation, hauling home meat, etc. They are about four feet high . . . and are drawn by one horse."

3. *Ibid.*, p. 262.

4. *Report of John Pope on explorations of Minnesota Territory*, 1850. U.S. Senate Executive Document No. 42, 31st Congress, 1st Session (Serial No. 558), in Report of Secretary of War, p. 30.

5. Aleš Hrdlička, "Anthropology of Chippewa," in *Holmes Anniversary Volume: Anthropological Essays*. Washington, D.C., 1916, p. 200.

6. Gontran Laviolette, O.M.I., "Notes on the Aborigenes of the Prairie Provinces (Manitoba, Saskatchewan, Alberta)." *Anthropologica*, 2 : 108, 1956.

7. U.S. Census.

8. Chas. De Noyer, "History of Fort Totten." *Collections of the State Historical Society of North Dakota*, 3 : 214–216, 1910. Bismarck, N.D.

9. Letter of Father Belcourt to a friend under the title "Pembina Mission." *Report on the Missions of the Diocese of Quebec*, 11 : 127, March, 1855.

10. Alexander Ross, *The Red River Settlement*. London, Smith, Elder & Co., 1856.

THE STUDY OF THE EARLY HISTORY OF AGRICULTURE IN THE TERRITORY OF THE U.S.S.R. IN 1945-1955

S. A. Tokarev

In post-war years among Soviet scientists interest has risen in the study of material production. This is connected with the increasing role of the masses of the people in the socio-political and cultural life of mankind. The Praesidium of the Academy of Sciences has intrusted the task of developing the history of agriculture in the U.S.S.R. to the Institute of History. The Institute has already published "Materials on the History of Agriculture of the U.S.S.R." (1952). A special commission has been organized, including the Institute of History, the Institute of Ethnology, and the Institute of the History of Material Culture. However, the cooperation in the work between historians and archaeologists is strongest in those areas (for example, Central Asia) where the chronological gap between archaeology and ethnology is smallest.

Important work is done on the history of agriculture among the Russian people and a "Russian Historical Ethnographic Atlas" is being prepared for 1956. One of the sections of the Atlas is titled "Agricultural Techniques of Russian Peasants in the 19th and the Beginning of the 20th Centuries." It is based on extensive research and expeditions and is concerned with the distribution of various types of agricultural implements and techniques. Eighty-two maps deal with agricultural implements. Fourteen summary maps are provided. These maps are divided into periods:

Mid-19th Century
Late 19th Century
1st Decade of the 20th Century

Typological tables are given, together with the following articles:

1. Types of agriculture and rural culture in the period of minimum capitalistic development.
2. Agricultural implements used in the period of maximum capitalistic development.
3. Types of harvesting.
4. Peasant buildings used for storing and processing grain.

Additional maps are provided for soils, vegetation, and ethnic distribution. The main work was done by D. V. Naydck. The Russian population of Siberia and Central Asia has so far been excluded. This work is projected for 1956–1960.

Considerable work is being done by archaeologists on the history of early agriculture among eastern Slavs. T. S. Passek has been working for many years on the problems of Tripolye and has shown the co-existence of hoe agriculture with animal husbandry. However, S. N. Bibikov considers plow agriculture to have been dominant at that time (3rd millennium, B.C.). Similar problems are being studied by V. D. Blavatsky (Hellenic Black Sea), P. D. Liberov

(Dnieper region, Early Iron Age), V. I. Dovzhenok (1st millennium, A.D., and Kiev period), A. V. Kiryanov (Volga Bulgars, combining archaeology with ethnology), P. N. Tretiakov (Slavs of the northern forest belt, where, until about A.D. 500, slash-and-burn agriculture was dominant), V. I. Ravdonikas (7th–10th centuries, A.D., Ladoga), and V. V. Sedov (8th–12th centuries, Smolensk region). Historians are also studying this problem by making use of written documents and, when possible, of archaeological materials. The late B. D. Grekov showed that agriculture formed the economical base of the Slavs by the 1st millennium A.D. Other workers include G. E. Kochin, L. V. Danilov, K. V. Sivkov, A. I. Baranovich, and E. I. Druzhinin.

In the last few years considerable work has been done in the history of agriculture in Siberia. Before the annexation of Siberia by Russia, agriculture was known only in some regions of the southern belt: along the Irtysh, upper Yenisei, western Baikalia and middle Amur. Russian peasants in the 16th and 17th centuries brought agriculture to new regions, and the local peoples (Altaians, Khakas, Buriats, Yakuts, and others) began to adopt it. On their part, Russian peasants took over some of the local techniques, for example, irrigation. A general work on this subject was written by V. I. Shunkov, "Sketches on the History of Agriculture in Siberia in the 17th Century."

The peoples of the Caucasus have been studied by ethnographers who also used archaeology and documentary evidence. Prof. G. S. Chitay has studied the development of the plow, and a Daghestan expedition was organized by the Institute of Ethnography.

The most important investigation in the history of agriculture has taken place in Central Asia, where it is oldest. Very extensive work was done for many years by the archaeological-ethnological Khorezm (lower Amu-darya) expedition under Prof. S. P. Tolstov. The development there is studied from the first establishment of agriculture with particular attention to irrigation. Aerial photographs have revealed many new features.

The picture that emerges is that of Neolithic hunting and fishing populations turning to primitive agriculture; by the early Bronze Age a primitive irrigation system was already in existence, utilizing dams.

The irrigation system later becomes more complicated and canals reach up to several kilometers in length. By the beginning of the Antique Period the irrigation system includes the whole delta of the Amudarya (6th century B.C.–3rd century A.D.). In the next period (4th–6th centuries, A.D.) the irrigation network shrinks, apparently due to important socio-political upheavals and the rise of feudal segmentation. After a short period of stabilization (7th–8th centuries), this irrigation system is further reduced by the 9th century. It is reestablished in the 11th–13th centuries during the centralization of the Khorezm state, but it suffers great destruction during the Mongol invasion. Irrigation revives again in the Uzbek period, beginning in the 16th century. Thus, the history of agriculture is closely related to sociopolitical events.

This expedition also discovered the use of fertilizers (for example, the remains of old buildings, sand, fertile soil, etc.). Bronze and iron scythes and grain-grinders were found. Proso, wheat, barley, grapes, melons, peaches, etc., were cultivated. Cotton appears at the beginning of our era.

Important results were also obtained on the Bokhara oasis (pre-Macedonian). General works have been written by V. F. Geydukevich, O. M. Dzhumaev, and M. R. Rakhimov.

Archaeological data have helped in the study of such problems as the socio-economic organization of early and medieval central Asian states, the relationship between nomads and settled populations, and the ethnogenesis of specific peoples. It has also helped to understand the formation of the present peoples—Uzbecs, Tadzhiks, Turkmen, Karakalpak, etc.

A collection of articles ("The History of Agricultural Cultures in the U.S.S.R.") has been produced as a result of the cooperation between archaeologists and historians and the Institute of Botany.

The history of agriculture in the U.S.S.R. is studied in connection with practical problems of reconstruction and further developments of the rural economy. The Institute of History is preparing a specific work on the "History of Collectivization of the Agricultural Economy of the U.S.S.R., 1928–1934." The ethnographers of the Ethnographic Institute, during recent years, have done important work in the study of particular collective farms in the various national areas of the Soviet Union. Along with the study of the changes in their lives, and the development of new culture, serious attention is also given to economic changes. Thus, in 1954, a collection of articles was published entitled "The Culture and Life of Tadzhik Collective Farm Peasantry."

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THE AUTOMOBILE IN CONTEMPORARY NAVAHO CULTURE[1]

Evon Z. Vogt

In the contact between mechanized and non-mechanized cultures, there is probably no area in the world today that surpasses the Southwestern United States in the extent to which still relatively unacculturated aboriginal peoples are coming into direct possession of very complex machinery produced in the industrial plants of distant urban centers. The decade since World War II has been characterized by a very marked increase in the number of automobiles that are owned and used by the Pueblo, Apache, Navaho, and most of the other American Indian tribes in the Southwest. This trend is especially notable in the case of the Navaho, where the number of automobiles has increased by at least four- or five-fold in the past ten years, and horse-and-wagon transportation in the majority of local communities is becoming a thing of the past. During the same decade it is clear that comparable changes have not taken place in other aspects of Navaho culture; the result is that one finds more and more "backwoods" Navahos, who know virtually no English and are fundamentally still Navaho in most of their behavioral patterns and value systems, owning and learning to drive automobiles.

There has been much discussion about the problem of how non-mechanized peoples adjust to complex machinery in culture-contact situations, but there have been few analyses of the process involving technological items as complex and as expensive as the automobile in situations where the machines are being purchased and used in large numbers by the aboriginal peoples themselves.

This preliminary paper will focus upon (1) Navaho cultural patterns which have encouraged such widespread adoption of the automobile; (2) the processes of adjustment of the Navaho to the automobile, especially the problems they face in purchasing the machine, learning to drive, maintain, and repair it, and in learning about such legal complications as license plates and drivers' licenses; and (3) some aspects of the effect of the automobile upon Navaho culture.

NAVAHO ADOPTION OF THE AUTOMOBILE

There are at least three persisting patterns in Navaho cultural history that bear importantly upon their widespread adoption of the automobile. The first, and most important, of these patterns is the well-known historical tendency for Navahos to learn new techniques and use foreign tools with no apparent emotional or cultural resistance to such borrowing. From the time that their Apachean ancestors first entered the Southwest with a simple hunting and gathering culture that had much to gain and almost nothing to lose by contact with other peoples, the Navaho record has been one of acquiring agriculture and probably weaving techniques from the Pueblos, sheep, horses, and silversmithing from the Spanish, and, later on, wagons, sewing machines, steel plows, radios, and countless other items from the Americans.[2] The adoption of the automobile is merely a contemporary chapter in this historical borrowing process.

But there are two other persisting patterns that add special importance to the automobile in the Navaho case: the scattered settlement pattern and the emphasis upon travel which is probably related to the emphasis upon "motion" in Navaho culture and language. The scattered settlement pattern has been one of the most significant continuities in Navaho culture. For a brief period in 1746–50 the Spanish attempted to persuade Navahos to settle down and build villages like their Pueblo Indian neighbors. The Navahos replied that they had been brought up "like deer in the mountains" and refused to congregate.[3] During the Bosque Redondo period of 1864–68 the American government also developed a program that called for their settling down in villages. But as soon as they were released from captivity, they scattered again into their customary extended-family hogan clusters and this pattern has not been fundamentally altered since. The result is that the automobile not only provides improved transportation into American towns, but is also of basic importance in trips between scattered hogan clusters and to the local trading posts in Navaho country.

It also seems evident that the automobile permits an even fuller expression than before of the high value that Navahos place upon travel. They have always struck observers as a restless, traveling group of people, and there is much to suggest in their culture, mythology, and language that they conceive themselves as living, as Harry Hoijer expresses it, in "a universe in motion" in which ". . . both gods and culture heroes move restlessly from one holy place to the next, seeking by their motion to perfect and repair the dynamic flux which is the universe."[4]

NAVAHO ADJUSTMENT TO THE AUTOMOBILE

Like most Americans these days, almost all Navahos purchase automobiles on time-payment plans. But inasmuch as Navahos are usually found at the bottom of the economic hierarchy in the Southwest, and have difficulty meeting their time-payments, they tend to end up with the oldest models of automobiles that are handled in the used-car markets. The result is that expensive repair bills are almost immediately added to the costs of the time-payments if the cars are to be kept in working order. On the other hand, this economic burden has recently led in some Navaho communities to the emergence of a new pattern of sharing these costs within the extended family that uses the car. One male member of the extended family, usually a son-in-law, will purchase an automobile and then take a wage-work job on the railroad and send back the money for the time-payments while the other members of the extended family proceed to drive and wear out the vehicle. When he returns after a few months, the car will be turned in to a dealer on a newer model, and another of the sons-in-law, or perhaps his father-in-law, will go off to work and send back the money for the payments while the others proceed to wear this vehicle out—and so on.

Navaho automobiles are still driven predominantly by the men, but in recent years an increasing number of Navaho women are learning to drive. When Navaho women do learn to drive, it is significant that they often make trips to trading posts and into town without necessarily being accompanied by their husbands, brothers, or fathers, as is the case with conservative Pueblo or Spanish-American women who seldom travel away from home without one or more of the male members of the family along.

Learning to drive is mainly a process of observing another member of the family closely and then getting into the car and trying it out around the hogans and through the timber (where there is no traffic to worry about) until one acquires the skill by experience. As far as I can determine, there is little or no formal instruction of the young or uninitiated as there is in our culture.

The legal complications are formidable adjustments for the Navahos, and would be more so if it were not for the fact that much Navaho automobile travel takes place on isolated roads where little attention is paid to such matters as drivers' licenses, license plates, and brake and light stickers. It is clear that hundreds of Navahos driving on Southwestern roads today are doing so without benefit of drivers' licenses and that additional hundreds holding licenses would not have them if the states of Arizona and New Mexico took the requirements as seriously as does, for example, the state of Massachusetts.

License plates are another complication when it comes to travel on the main highways, but one effective and fairly common means of dealing with this problem is to pass one set of plates around among an extended family or "outfit"; the man who is going to town on a given day gets the license plates to use on his car! This system works well unless the automobile is involved in an accident or in the infringement of a traffic regulation, and then the driver is of course arrested and fined.

In the long run, however, perhaps the most difficult problem of adjustment to the automobile has been the matter of maintenance and repair. As Kluckhohn has commented, ". . . the Navahos are good natured, patient, and persistent in tinkering with broken-down machines; by sheer trial and error methods some are able to keep very ancient models in circulation."[5] Indeed, there are occasional cases of sheer genius in dealing with problems that arise. For example, one of our Ramah Navaho informants once had his fuel pump fail about 15 miles from town. He took the top off his gasoline tank and tightly wired an old piece of inner tube with valve stem attached around the opening to the tank. He then screwed on the tire pump and pumped enough air pressure into the gasoline tank to force the gasoline past the damaged fuel pump and into the carburetor, thereby permitting him to drive a mile or so until the air pressure went down in the tank. Then he pumped more air into the gasoline tank, drove another mile, and continued this procedure until he reached a garage in town. According to competent mechanics I have talked to, this "invention" would probably not have occurred to one out of a thousand men in our culture.[6] There is also the case of the Navaho who worked out a novel system for keeping his automobile engine warm for quick-starting on cold winter mornings. The observer of this case reports that before the Navaho started up his car, he opened up the hood and out flew ten chickens he customarily roosted on top the engine at night to keep it warm![7]

But the predominant pattern continues to be one of supreme neglect of such necessities as grease, oil, battery water, and anti-freeze, and they sometimes go to the extreme of draining the radiator to drink the water and then driving without water until the car is ruined. This pattern is also related to persisting continuities in Navaho culture. Navahos tend to treat automobiles as they used to treat horses—as instruments of travel that were ridden hard, then abandoned for another horse when they were exhausted. There is little or none of the loving care or concern accorded horses or automobiles in our culture. Navahos do not generally pet and curry-comb horses; they do not generally wash, polish, or grease cars. It is also evident that Navahos have failed, by and large, to acquire and internalize a future-time and planning orientation from us along with the automobile—with the result, for example, that anti-freeze may not get put into the radiators until it's too late in the autumn and the automobiles have to be abandoned with cracked engine blocks after the first frosty night. In 1951 one-third of the automobiles owned by the Ramah Navaho had cracked engine blocks through this failure to put in anti-freeze in time.

SOME EFFECTS OF THE AUTOMOBILE UPON NAVAHO CULTURE

The adoption of the automobile has undoubtedly led to important changes and has had some disruptive effects upon traditional Navaho culture. It has added an important piece of technology and an improved mode of transportation to the cultural inventory; the language has been enriched by a whole complex of terms the Navahos have coined to communicate with each other about this machinery; the desire for automobiles has led many a Navaho deeper into the whole money economy of our society than he might otherwise have gone. The disruptive effects are also clearly apparent: excessive economic strain upon low family incomes; increased travel away from Navaho country and into white American towns and cities; decrease in the strength of reciprocal hospitality patterns within Navaho country as people travel to and from their destinations in one day and no longer need to stay overnight with relatives as frequently as they did in the horse-and-wagon era.

At the same time, the automobile has had a profound integrative and conservative influence upon traditional Navaho culture, and an influence that needs to be emphasized for the benefit of those who tend to think that improved modes of transportation lead mainly to changes in traditional cultures. In the Navaho case, the new mode of transportation has meant an important *increase* in the numbers of Navahos who attend large ceremonials, such as the Night Chant and Mountain-top Way in the winter season, and the Enemy Way in the summer season. They gather from more distant points in Navaho country, and, more important still, Navaho families working and living in towns off the Reservation can travel to these ceremonials and return to their jobs the next day or on Monday morning after a week-end. Perhaps even more significant in the long run is the fact that the automobile extends the geographical range of the Navaho singer's practice. If a given local Navaho community no longer has in residence a singer who knows a particular traditional ceremonial, a Navaho family may now readily import a singer from another community a hundred or two hundred miles away and return him to his native community in time to perform all the ceremonies he would have performed as a matter of course in the horse-and-wagon era. In the present scene where the supply of qualified singers of the longer and more complex ceremonials is steadily diminishing (as the older singers die and younger men do not learn all of the ceremonials), this travel by automobile is crucial in preserving many of the patterns of traditional Navaho ceremonialism which in turn express and emphasize many of the central values of Navaho life.

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Notes

1. The research on which this paper is based is part of the Comparative Study of Values in Five Cultures Project of the Laboratory of Social Relations, Harvard University, supported by the Social Science Division of the Rockefeller Foundation.

2. See especially Hill, 1940; Kluckhohn, 1942; Adair and Vogt, 1949; Vogt, 1951; Underhill, 1956.

3. See Kelley, 1941, pp. 41–70.

4. Hoijer, 1951, p. 117; see also Astrov, 1950, and Albert, 1956.

5. Kluckhohn, 1942, p. 5.

6. Personal communication from Paul Davis, Davis Repair Shop, Ramah, New Mexico.

7. Personal communication from Richard Van Valkenburgh, Window Rock, Arizona.

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HISTORY AND ETHNOHISTORY, AND A CASE IN POINT

Erminie Wheeler-Voegelin

The localization of native groups has always been of basic concern to American anthropologists, ever since scientific field study of such groups started during the last quarter of the 19th century. In the case of peoples who had not been displaced, such as many of the western North American Indian groups, the anthropologist went into the field, interviewed and took short trips through his area with as many elderly Indians as he could find and persuade to talk with him. From these informants he learned where their people had been accustomed to range seasonally for many different kinds of food, for raw materials used for manufactures in the native economy, and so forth. During field work the anthropologist also attempted to ascertain, both from informants and by inspection, where the group he was studying had had seasonally used camps, where its more or less permanent villages had been located, where crops were planted, the dead buried or cremated, and other uses to which land was put. Boundaries of the native group's "own country" were also inquired about directly, as well as native names for settlement sites and for neighboring groups, and traditions concerning previous movements or migrations.

With all this and a great deal more field data recorded on U.S. Geological Survey topographical sheets and in field notebooks, the anthropologist, when either time or field funds, or both, ran out, returned to his university or museum and proceeded, perhaps within the next six months, or perhaps within the next sixty years, to write up the results of his field study. Usually, among American anthropologists, studies of exotic cultures were (and still are) written in monographic, not in book form. If the study had been of a group which had not been displaced or removed during the White contact period, the frontispiece for the monograph was nearly always a map delineating in no uncertain terms—usually by heavy black lines—the so-called territory of the group, as well as the territories of neighboring groups. If the map was large enough, village and camp sites might be included on it, or these might be indicated on a second map drawn on a larger scale and embracing only a fractional part—say a fifth, or a quarter, or a third—of the first, "total territory" map. The fact that the village- and camp-site map always covered a much smaller area than the territorial map seemed of little concern to anthropologists. Nor were they, apparently, disturbed by the fact that, with little or no checking of documentary material, they were assigning relatively small groups an occupancy of often amazingly large, definitely bounded areas, without offering, in most cases, much data in their text as to what use was made of the total area delineated.

For the many eastern and midwestern North American Indian groups who had either removed or been removed from their early habitats to locations west of the Mississippi River during the late 18th and the first half of the 19th centuries, the anthropologist necessarily tackled the problem of native occupancy in quite a different fashion. For several of these groups which had been fortunate enough to survive three centuries of White contact, field studies have been made, but maps showing the "aboriginal" locations of such groups are nearly always lacking.

Determination of so-called aboriginal occupancy for groups in the eastern and midwestern sections of the United States has been, necessarily, one which has rested upon documentary evidence. Comparatively speaking, the number of anthropologists who have devoted themselves to documentary research, compared to the number who have based their research on field work, is relatively small. As anyone who works with documents so well knows, such research is time-consuming. The total number of professionally trained anthropologists during the first half of the 20th century when the discipline was becoming academically established was small. There was a great amount of field work to be done in the west and southwest while Indians who still remembered something of their native cultures were still alive. Published and archival material would keep; field work, from which much fuller delineations of native cultures could be drawn, had to be done immediately if it was to be done at all.

But despite the fact that relatively few anthropologists spent a major part of their research time in documentary research on the eastern and midwestern groups, several continental maps assigning locations and even bounded territories to these groups, as well as to the western ones which had been mapped, have been published by anthropologists during the past half century. One of the earliest of these to appear was Clark Wissler's 1917 continental map of North American Indian tribes, on which tribes are located (regrettably, in several instances erroneously), but tribal boundaries are not assigned. The latest tribal map is a 1953 one by Harold Driver and five other anthropologists, which locates all the North American Indian tribes and also assigns boundaries to their territories. All such tribal maps, either explicitly or more often implicitly, purport to show aboriginal locations and to indicate, usually by boundary lines, the territory occupied by Indian groups in so-called aboriginal or earliest White contact times.

Such maps are unsatisfactory for anything except the most general purposes. Their use for such purposes has recently been defended by the chief compiler of the most recent continental map, at the same time that he specifically disclaims the applicability of his map for any detailed problems touching on actual occupancy, such as those posed in current Indian land-claims cases.

From *ca.* 1950, when anthropologists began to be called upon in Indian land-claims cases as expert consultants, occupancy and use of specific areas, by specific groups, has become a problem engaging the attention of many members of the profession. Whether they are called upon to deal with western, or with eastern claims cases, anthropologists engaged in this research have all had to enquire into the history of occupancy of specific areas, from a reasonable period prior to date of sovereignty by the United States, to time of treaty cession, or time of taking. For many anthropologists who heretofore have relied exclusively upon field data, this broadening of time perspective has been of utmost value in furnishing greater insight into present-day problems concerning native peoples and their cultures. The fact, also, that in claims cases the anthropologist is concerned with a specific group's claim to a certain specific area implies something of a new approach. While it is true that in a claims case a tribe (or tribes) is suing, and that therefore a specific tribe is involved, the suit hinges upon occupancy of a specific area throughout a period of time—and the area involved may at different periods have been used and occupied not by one, but by several different Indian tribes or parts of tribes. It is the area, then, that remains constant, whereas tribal occupancy of the area or parts of it is likely in many instances to have changed during contact times. Where this is the case (as it is for many of the Great Lakes–Ohio Valley areas), who occupied an area and when are questions of primary importance. If the Sac and Fox, for example, moved into western Illinois only two or three decades before ceding title to the area known as Area 50 in Charles C. Royce's compilation of Indian Land cessions, where the Sac and Fox were for ten decades before ceding title to Area 50 is of secondary importance, since it concerns a negative rather than a positive point.

How the anthropologist goes about investigating native occupancy of a specific area over a stated length of time parallels methods familiar to historians, since for this work the anthropologist uses much the same corpus of material that the American history-research scholar uses. It is not the corpus which marks a difference between the historian and the anthropologist, but the different interest which the same document holds for each of them. The historian is interested in documents containing references to American Indians, for whatever information they contain as to how the Indians affected French Colonial, or British Colonial, or American cultural and political history. The anthropologist, on the other hand, is interested in the same documents for what they tell him about inter-tribal contacts, locations, numbers, movements, and population of the Indians mentioned therein, for what information they give him about the culture of the various Indian groups, and whatever data they supply as to how Indian groups were being moved and manipulated by the Whites in their midst, and how their culture was being affected.

The remark of Major Arent De Peyster, commandant at the British post of Michilimackinac in 1777, in a letter to General Frederick Haldimand, Governor-General of Canada, that fifteen St. Joseph, Michigan, Potawatomi chiefs and chief's sons who had visited him at his post were all "totally ignorant of Bark Canoes," and his further remark that, as a consequence of this, he was "therefore oblig'd to send them [home to St. Joseph, at the southern end of Lake Michigan] in a Return Canoe I hired for that purpose," may not be particularly meaningful to a historian, but has a good deal of significance to an anthropologist. Among other things, it indicates the direction St. Joseph Potawatomi movements and trade took at that time; it also bears on differences of modes of travel and transport within the Eastern Woodlands, and on the more theoretical question of a possible cultural loss.

I could cite many examples of historical documents which hold one sort of interest for the historian and another for the anthropologist, but I will mention only one more. In June, 1778, Henry Hamilton, British Lieutenant Governor stationed at Detroit, assembled 1,683 Indians from several groups in council, and persuaded them to join the English in raising the hatchet against the Americans. The record of this council is, I am sure, significant to the historian in terms of Anglo-American conflict. The anthropologist, who necessarily must be aware of the reason for the council having been held, is not particularly interested or concerned with this reason; what he is interested in are points such as the following (all mentioned in the official record of the council): which Indian groups attended, and, of these, which groups are localized (as the St. Joseph Potawatomi are); the fact that dual chieftaincy (war chief and village chief) is noted for various tribes; which of the two types of chief spoke at the council; the native names given for various Indian leaders present; the fact that an Ottawa chief, Chaminitawaa, spoke in council on behalf not only of the Ottawa but of the Potawatomi and Chippewa (Ojibwa) as well. Each of these details has significance for the historically minded anthropologist attempting to treat the history of various Great Lakes–Ohio Valley Indians, or to reconstruct their cultures, but I doubt whether many of the same details would be especially meaningful to the historian.

In a sense, then, perhaps the historian and the ethnologist may never quite meet, because of their difference in interests. I feel hopeful, however, that more familiarity with each other's fields will enrich the viewpoints of both and contribute toward lessening the naivetes both are likely to fall into when essaying historical treatment of Indian–White or White–Indian relations. It may also not be overly optimistic to envisage, in view of the increased interest that historians are showing in the American Indians, a new field developing in American history, namely, the history of the American Indians, in which much that the anthropologist has learned about American Indians, and much that the historian knows about their background, will be combined.

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SECTION IV

ETHNOGRAPHY

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LES ORIGINES ETHNIQUES DE LA POPULATION MAROCAINE MUSULMANE DE CASABLANCA

André Adam

La population musulmane du Maroc augmente, comme on le sait, à un rythme rapide. Entre le recensement de 1936 et celui de 1952, elle est passée — pour l'ex-zone française — de 5.885.000 à 7.443.000, ce qui représente, dans ces onze années une augmentation de 26,5% et un taux annuel d'accroissement de 1,48%. La population urbaine a cru dans des proportions bien supérieures: 91% en seize ans. Il y a donc en exode rural, déplacement de populations des campagnes vers les villes. Le fait est particulièrement sensible à Casablanca, petite ville de 20.000 habitants en 1900, qui en comptait en 1952, 680.000 dont 472.000 Marocains musulmans. Entre 1936 et 1952, l'accroissement a atteint 200% et Casablanca représente à elle seule 34% de la population urbaine du Maroc et 60% de l'accroissement urbain.

La majorité des Casablancais viennent d'ailleurs. D'où viennent-ils? Il m'a paru intéressant de le savoir et j'ai pu, grâce à l'obligeance et à la collaboration des autorités municipales, profiter des opérations du recensement de 1952 pour pratiquer un sondage qui a touché environ 40.000 personnes.

La question posée était le lieu de naissance, ville ou tribu, du chef de foyer. Les réponses n'ont pas toutes été précises, tant par la faute des enquêteurs que par celle des enquêtés. Cependant, en maniant les chiffres obtenus avec les précautions d'usage, on peut en tirer quelques conclusions qui ne sont pas dénuées d'intérêt. Je ne saurais les exposer toutes ici. Je laisserai de côté, par exemple, la répartition ethnique entre les différents quartiers de la ville, pour m'en tenir à la comparaison des apports (trés inégaux, comme on va le voir) fournis par les diverses populations du Maroc.

Peu de Casablancais sont nés à Casablanca: 8% seulement. Il ne faut pas s'en étonner. L'accroissement de la population date surtout des années qui ont suivi la seconde guerre mondiale, puisqu'elle a doublé de 36 à 52. Il n'y a pas beaucoup plus de citadins, je veux dire d'émigrés originaires d'une autre ville: 17,7%. Les villes de l'intérieur donnent peu (à une exception près sur laquelle nous reviendrons), celles du Nord également, mais ce dernier trait tenait peut-être à une cause politique, la division administrative en deux zones de protectorat, et la récente suppression de ces zones peut modifier l'aspect des mouvements migratoires de la population marocaine. Le gros apport vient des villes situées au sud de Casablanca, sur la côte, comme Mazagan, Safi, Mogador, ou à l'intérieur comme Marrakech. Cette dernière ville vient nettement en tête avec 16,2% des casablancais d'origine citadine.

Elle est suivie de peu par Fès (11,6%). C'est l'exception dont je parlais tout à l'heure. Fès, comme Marrakech, est une ville dont la population reste stationnaire: leur accroissement moyen annuel est inférieur à celui du Maroc tout entier. Non que leurs habitants soient moins prolifiques, mais ils émigrent. Les deux villes ont été déchues de leur rôle, Marrakech de capitale politique, Fès de capitale politique et économique. Ce ne sont plus, sur l'un et l'autre plan, que des capitales régionales. Mais leurs populations ont réagi différemment: Marrakech exporte vers Casablanca des petites gens, Fès des bourgeois. Dès le début du siècle, les Fassis, en commerçants avisés, sentant l'importance croissante de Casablanca, au fur et à mesure que le Maroc s'ouvrait au monde extérieur, y créèrent des succursales qui sont devenues depuis la maison principale. Aujourd'hui, ils y détiennent tout ce qui, du gros commerce d'importation, est entre les mains des Musulmans. Ainsi s'est créé entre Fès et Casablanca un courant d'émigration qui n'a pas cessé mais s'est encore accru avec le développement récent de Casablanca. Emigration importante par le nombre, sans doute, mais surtout par la qualité: Fès a fourni le noyau de la nouvelle bourgeoisie casablancaise et les Fassis n'ont ainsi rien perdu du rôle dominant qu'ils jouaient naguère dans la vie publique du pays. Voilà un phénomène humain qui ne doit rien à des impératifs géographiques, mais tout à l'esprit d'initiative et d'entreprise d'une population.

Les Casablancais sont donc en majorité des ruraux: 82,3%. Je ne vous en donnerai pas le détail par tribu. Ce serait trop long et fastidieux. Si l'on s'en tient aux grands ensembles, on s'aperçoit, une fois encore, que certaines régions donnent beaucoup et d'autres pas du tout. Toute ville recrute dans ses environs immédiats et Casablanca n'échappe pas à la règle. Mais au delà, l'émigration se limite au sud de la ville, un sud qui commence aux Chaouïa pour finir aux confins du Sahara. On peut tracer schématiquement une ligne qui joindrait Casablanca au coude de l'oued Dra, en englobant le Tadla: tout ce qui émigre vers Casablanca est situé entre cette ligne et la côte. En dehors, il n'y a que des quantités négligeables, à une exception près, une fois encore, c'est le Tafilalet. Que cette oasis à population très dense fournisse des émigrants, ce n'est pas pour étonner. Ce qui étonne c'est qu'ils n'aillent pas vers les villes qui sont en rapports directs et anciens avec le Tafilalet, c'est-à-dire Fès et Meknès, comme les les Ksouriens de Figuig vont vers Oujda et Oran. En réalité, c'est bien, me semble-t-il, le vieux lien séculaire avec Fès qui continue à jouer: Fès, en émigrant vers Casablanca, y entraîne sa clientèle traditionnelle.

Pourquoi les régions situées en dehors du triangle indiqué n'émigrent-elles pas vers Casablanca? Pour le Maroc oriental, il faut tenir compte de l'éloignement et du fait que les industries minières qui se développent autour d'Oujda suffisent à absorber l'excédent d'une population de faible densité. Les Rifains de l'ex-zone française trouvaient jusqu'ici dans l'émigration saisonnière vers l'Algérie ou vers les riches exploitations agricoles de la plaine de Meknès, un débouché suffisant. Ceux de l'ex-zone espagnole étaient gênés par la présence d'une frontière administrative. L'unification du Maroc, bien qu'elle ne soit encore qu'imparfaitement réalisée, a eu pour résultat de lever cette barrière, et l'on note déjà un afflux sensible d'émigrants du Nord vers Casablanca. Rabat et Port-Lyautey suffisent à absorber l'excédent des populations du Gharb. Les Berbères de langue *tamazight*, tribus pastorales qui occupent le Moyen-Atlas et le Haut-Atlas central, émigrent peu. Ils ont, semble-t-il, réussi à conserver jusqu'à présent un équilibre démographique et économique satisfaisant.

Les populations comprises à l'intérieur du triangle sont de deux sortes. Le plateau du Tadla et les plaines atlantiques depuis les Chaouïa jusqu'au pied de l'Atlas, sont habités par des Arabes; le reste, jusqu'à la bordure du Sahara, est occupé par des Berbères. On ne devrait pas parler, d'ailleurs, d'Arabes et de Berbères, mais seulement d'arabophones et de berbérophones. Car la plupart des populations dites arabes du Maroc sont en réalité des Berbères arabisés, et l'on trouve en revanche, aux confins du Sahara, des restes d'anciennes tribus arabes battues, dispersées et assimilées par les Berbères. La question des races est aujourd'hui inextricable au Maroc et le seul contenu scientifique que l'on puisse donner à la distinction Arabes-Berbères est un contenu linguistique. Les Berbères de la zone d'émigration vers Casablanca appartiennent tous au groupe dialectal de la *tachelhit*, ce sont ceux qu'on appelle vulgairement les Chleuhs. Ils ne forment d'ailleurs pas un groupe homogène. Les uns sont blancs: ce sont les montagnards du Haut-Atlas occidental et de l'Anti-Atlas; les autres noirs: ce sont les *Haratin* des palmeraies du Dra et du Bani.

Sur l'ensemble des Musulmans casablancais d'origine rurale, les arabophones représentent 66% et les berbérophones 34%. Les noirs figurent pour 40% parmi les berbérophones, soit 13% de l'ensemble. Il y a lieu de noter que la proportion des berbérophones est à peu près la même que dans la population globale du Maroc.

Ces populations ne se différencient pas seulement par la langue ou la couleur de la peau, mais par le genre de vie et le régime juridique. Les uns et les autres sont des sédentaires agriculteurs, sans doute. Mais les uns le sont depuis l'aube de l'histoire et les autres de fraîche date. Les Berbérophones du Haut et de l'Anti-Atlas sont fixés au sol depuis longtemps et représentent sans doute la plus ancienne civilisation sédentaire du Maroc. Les Heratin des oasis pratiquent l'agriculture à la houe et sont passés maîtres dans l'art délicat de l'irrigation. Les Arabophones des plaines atlantiques au contraire, sont de petits nomades, éleveurs de moutons, dont la fixation au sol est précaire et récente, soit qu'ils descendent des tribus arabes de grands nomades *Beni Hilal* et *Maaqil* qui envahirent le Maroc aux XIème et XIIIème siècle, soit que — c'est le cas du plus grand nombre — il s'agisse d'anciens Berbères sédentaires, dont la civilisation agriocole fut détruite par les invasions des Bédouins et qui se trouvèrent à la fois arabisés et entraînés dans le nomadisme des envahisseurs. Le cas des Chaouïa est typique à cet égard. Leur pays, le Tamesna, était jadis habité par des Berbères sédentaires, les Berghwata, et les vieux chroniqueurs nous le présentent comme le grenier à blé du Maghreb. Après les invasions bédouines, les habitants ne sont plus désignés que par le nom de Chaouïa, qui signifie éleveurs de moutons. Aujourd'hui, c'est de nouveau une riche terre à céréales.

Quant au régime juridique, les arabophones de la plaine, soumis depuis toujours au pouvoir à la fois religieux et temporel du Sultan, sont régis par le *Chraa* ou droit coranique orthodoxe, de rite malékite. Les Chleuhs, au contraire, sont restés fidèles au vieux droit coutumier berbère, que des siècles d'islamisation ont, il est vrai, rapproché du Chraa, mais qui n'en garde pas moins son originalité sur plusieurs points importants.

Je n'ai pas l'intention d'analyser les causes de l'émigration. L'étude a déjà été faite. Je voudrais seulement m'attacher aux formes de l'émigration vers les villes et en particulier vers Casablanca. On en peut observer deux principales: une émigration familiale et définitive—l'homme part pour la ville avec femme et enfants et ne revient pas — et une émigration masculine et temporaire — l'homme part seul, laissant les siens au pays natal, et y revient, périodiquement d'abord, puis définitivement pour y finir ses jours. Or, il se trouve que deux types d'émigration correspondant aux deux catégories de population que nous venons de distinguer: l'émigration définitive est le fait des arabophones de la plaine, l'émigration temporaire celui des berbérophones. On ne saurait évidemment en chercher la cause dans une sorte de détermination racial qui serait dépourvue encore une fois de toute valeur scientifique, et même de toute espèce de signification. Il n'en est pas de même du genre de vie et du régime juridique. Les petits nomades fraîchement sédentarisés de la plaine atlantique éprouvent un attachement médiocre pour un sol qui n'était pour eux qu'une terrain de parcours, qui appartenait souvent à la tribu non à l'individu, et qu'ils cultivaient çà et là au gré du partage annuel des terres collectives. Les Berbères du Sous, au contraire, sont profondément attachés à leur terre bien familial et non tribal, enrichi au cours des siècles par de patientes techniques agricoles. La meilleure preuve de cet attachement, c'est que les gains réalisés dans l'émigration sont employés en premier lieu à racheter les parcelles que le malheur des temps a contraint d'hypothéquer ou, pour les plus heureux, à arrondir le patrimoine. Les ventes définitives sont rares dans le Sous. Celui que l'infortune oblige à vendre ne désespère jamais de recouvrer son lopin de terre et il ne consent qu'une *rahnia* c'est-à-dire une vente à réméré, qui lui permettra de racheter quand il en aura retrouvé les moyens.

Le Berbère qui pratique l'émigration temporaire n'abandonne pas femme et enfants. Il les laisse à la garde de la famille patriarcale. (Je prends ici "patriarcale" non au sens qui l'oppose à "matriarcale" mais à celui de famille large, où les fils restent sous l'autorité du père, même après leur mariage). Il y a toujours des hommes à la maison pour protéger les siens pendant son absence et, quand il regagnera le village, un frère ou un cousin partira aussitôt pour la ville à son tour. Mais la famille patriarcale n'est pas au Maroc, une structure propre aux Berbères. Elle existe aussi chez les Arabes, même eux des villes. En fait, elle est entrée en décadence chez ces derniers, elle se désagrège, tandis qu'elle a gardé toute sa cohésion chez les Berbères. Pourquoi? C'est le droit, semble-t-il, qui donne la clé de cette différence. Le droit coranique, appliqué chez les Arabes, reconnaît à la femme le droit d'hériter. Sa part n'est, à vrai dire, que la moitié de celle de l'héritier mâle, mais enfin elle hérite. Quand le père meurt, elle manque rarement, poussée par son mari, de réclamer sa part. Il faut partager et le patrimoine familial se trouve divisé. Le droit coutumier berbère, au contraire, exclut la femme de l'héritage. Chez les Imazighen du Maroc central, la règle est formelle et ne souffre aucune atténuation. Les Chleuhs, plus influencés par l'Islam, ont introduit dans leur coutume le principe — théorique — de la vocation des femmes à hériter. Mais comme il répugne en fait à leur conception profonde de la famille, ils se sont appliqués à le tourner par divers subterfuges juridiques, dont le plus fréquent est le *habous* familial qui permet de déshériter presque intégralement les filles au profit des fils. Le résultat, c'est que le patrimoine familial reste dans l'indivision entre les fils et que la famille patriarcale garde ainsi le substrat économique qui lui permet de maintenir sa cohésion. Le caractère familial — et non individuel — de la propriété est si fortement marqué qu'il s'étend la plupart du temps aux entreprises de l'émigration. C'est ainsi que chez les tribus de l'Anti-Atlas qui pratiquent avec succès le commerce de détail (généralement l'épicerie) dans les villes comme Casablanca et Rabat, la boutique est la propriété du groupe familial dont les hommes se relaient pour en assurer la gestion.

Ainsi s'expliquent, croyons-nous, les deux types d'émigration qui alimentent le peuplement musulman de Casablanca.

La structure ethnique de la ville peut donc se dessiner de la façon suivante. Au sommet, une riche bourgeoisie d'origine fassie, dont Casablanca a décuplé la fortune et la puissance, mais qui reste fière de son origine et de ses traditions. Il y a bien à côté une bourgeoisie purement casablancaise, celle des propriétaires enrichis depuis 1907 par la monstrueuse plus-value des terrains. Mais quelle que soit sa richesse elle n'a pas l'esprit d'enterprise des Fassis et surtout elle n'a pas leurs traditions de civilisation raffinée. Les Fassis sont de vieux citadins, héritiers de la culture andalouse, et c'est eux qui donnent le ton à la société casablancaise, qui les imite en les jalousant, parfois même en les détestant. Cette bourgeoisie fassie s'est maintenue jusqu'ici assez pure, car on s'y marie non seulement entre compatriotes, mais entre cousins, au sein de grandes familles trop fières de leur nom pour se mésallier.

Parmi la masse du peuple, la structure tribale ne survit guère à l'émigration. Il serait vain de rechercher dans les quartiers de Casablanca, la prédominance de telle tribu ni même de véritables groupements tribaux. Mais cela ne signifie pas que toute frontière ethnique disparaisse. S'il y a des classes sociales nouvelles qui se créent, opérant un brassage qui efface les vieilles différenciations, — et c'est le processus même d'urbanisation des masses rurales, — on observe encore aujourd'hui un particularisme (je ne donne bien entendu à ce mot aucun contenu politique) qui isole et garde dans une relative cohésion les deux groupes berbères de Casablanca: les Chleuhs du Sous et les Haratin.

Les Haratin—qu'on appelle à Casablanca Drawa ou Sahrawa—sont d'excellents agriculteurs mais la ville n'offre guère d'emploi à leurs talents. Aussi exercent-ils les métiers les plus humbles, et occupent-ils le bas de l'échelle sociale. Ils ont des coutumes particulières auxquelles ils restent très attachés. Ils sont relativement isolés par la couleur de leur peau (le Marocain n'est pas raciste, mais celui qui a le teint parfaitement blanc en éprouve cependant une certaine fierté), et se marient surtout entre eux. Leur nombre s'accroît rapidement tant par une forte émigration que par leur prolificité et il n'est pas interdit de penser que, dans un avenir plus ou moins lointain, Casablanca sera une ville à forte proportion de population noire, d'autant qu'ils pratiquent de plus en plus, à leur tour, l'émigration familiale et définitive.

Les Chleuhs ou berbérophones blancs se répartissent en deux catégories: les commerçants et les ouvriers. Distinction à base purement économique, semble-t-il, et où s'effacent les différences ethniques? Il n'en est rien. Commerçants et ouvriers ne viennent pas des mêmes tribus. Deux ou trois tribus de l'Anti-Atlas fournissent la quasi-totalité de ces épiciers qu'on rencontre dans tous les quartiers de Casablanca, musulmans ou européens, et que leur labeur, leur âpreté au gain, leur astuce commerciale, leur amabilité ont rendus célèbres, au même titre que les Mozabites en Algérie. Ceux des tribus voisines n'ont jamais réussi dans le commerce et doivent se contenter de gagner leur vie comme manœuvres sur le port ou dans les usines. Pourquoi cette différence? C'est une question à laquelle personne n'a pu encore répondre.

Le particularisme de ces Chleuhs s'explique suffisamment par leur type spécial d'émigration. Ils ne sont pas intégrés à la cité, où ils ne séjournent que provisoirement. Ils gardent les yeux tournés vers le pays natal, où se trouvent leur famille et leurs véritables raisons de vivre. Mais la ville a ses attraits et ses exigences. Certains — et ils sont de plus en plus nombreux — se lassent de vivre seuls et font venir un enfant, ou même leur femme. Ce sont les plus aisés qui commencent, ceux qui peuvent se payer un logement, assez vaste, donc les commerçants. Mais leur exemple est suivi par un nombre croissant d'ouvriers.

Certains commerçants Chleuhs ont réussi à se faire une place dans le commerce de gros, l'alimentation surtout. Mais ils commencent à toucher aussi à l'importation de tissus, fief traditionnel des bourgeois de Fès. Il y mettent une ardeur et une ténacité qu'alimente la rancœur du long mépris où les Fassis les ont tenus. Ceux-ci devront bientôt compter avec cette force qui monte, d'autant plus redoutable qu'elle est soutenue par l'esprit de clan: les Soussis se tiennent si bien les coudes qu'on n'a jamais enregistré aucune faillite d'un commerçant soussi.

On peut se demander si ce particularisme durera. Les enfants élevés à Casablanca parleront de plus en plus l'arabe et de moins en moins le berbère. Ils oublieront le pays natal et se sentiront citadins. Les mariages les mêleront à des familles d'origines diverses. Ces cas existent déjà. Ils ne peuvent que se multiplier. C'est l'essence même du phénomène sociologique qu'est la ville que d'effacer les différences tribales et de leur substituer d'autres classifications, fondées sur d'autres valeurs. Mais Casablanca est une cité en gestation, où le processus d'urbanisation est loin d'être achevé. Et pendant quelque temps encore les conditions sociologiques qui régissent la vie des petites sociétés tribales où s'aliment la grande ville continueront d'y faire sentir leur influence et d'entretenir un particularisme qui sera comme l'écho des composantes ethniques de la nation.

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INTERTRIBAL RELATIONS IN THE PUEBLO KACHINA CULT

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Perhaps the most flourishing of the native religious cults among the Pueblo Indians to-day is that of the rain- and fertility-bringing spirits known as the kachinas, who are represented by masked impersonators in a large number of complex and colorful ceremonials. It has long been assumed that this cult originated very largely among the Zuni of western New Mexico, and thence spread to other towns. However, an examination of all pertinent sources of information shows that the Western Keresan towns in particular have also contributed heavily to the formation of the cult, and that almost all Pueblo groups have participated in its development to some extent (see Anderson 1955, for a general statement).

Zuni remains, nevertheless, the most important single source for much of the cult, originating many basic features as well as details. This widely held opinion is based on the obvious complexity of all aspects of the cult here, and its thorough integration with the general socio-ceremonial organization. However, Zuni has borrowed as well, particularly, of course, from its nearest neighbors, the Western Keres and Hopi. I need only mention the Kyanakwe and the Shulawitsi ritual from the Western Keres, and the ceremony of Ololowishkya from the Hopi, to bring out the importance of this borrowing. Probably no town, however, has so well integrated the items borrowed as has Zuni, giving many traits and complexes an appearance which has in some cases probably misled investigators into assuming for them a native origin.

It has long been recognized that the great bulk of the Hopi kachina cult has been derived from the pueblos farther east. Specifically, Zuni has been mentioned as the principal source of the Hopi cult. The number of kachina songs with Zuni words (Curtis 1922: 176) may be noted, and Parsons has mentioned Shalako and the Koyimsi as recent acquisitions by the Hopi, and has made the rather dubious suggestions that Powamu is an earlier borrowing from the Zuni initiation, and that Niman derives from the last day of Shalako (Parsons 1928b: 594). She has emphasized also the lack of integration of the Hopi cult as suggesting that it is for the most part a borrowed complex: ". . . sporadic or optional celebration, omission of the esoteric, together with lack of integration into the rest of the ceremonial system—all these are the earmarks of a borrowed ceremony. It is because kachina celebrations among Hopi . . . have this character when compared with Zuni celebrations that I infer Zuni provenience for the kachina dance cult" (Parsons 1939: 973). With regard to the omission of esoteric features, it may be noted that the Hopi did not learn that Zuni dancers carry seeds in their belts, thus making the dance efficacious. They did learn, however, many things which could not have been discovered from mere casual observation—for example, control (or partial control) by the Badger clan, found at Oraibi and for Shalako on First Mesa (Parsons 1939: 971–2).

Important influences have also been received by Hopi from the Keres, including the word *kachina* itself, a number of impersonations, and perhaps the

Niman ceremony or at least its fundamental features. Parsons has mentioned "the preeminence of Eototo on First Mesa. Eototo is the father of the kachina, and Eototo is to be identified with Heluta or Heruta of the Keres" (Parsons in Stephen 1936: xlvii fn.). He has no prominent counterpart at Zuni, so that this seems a case of direct transmission from Keres to Hopi.

It is also probable that parts of the modern kachina cult were already present among the Hopi when Zuni (and other eastern) influence began to make itself felt. Parsons has pointed out that the ancient clan kachinas may well have been independent of this influence (Parsons 1939: 973; 1928b: 594), and that impersonation of the spirits, often without mask, is apparently an old Hopi trait (Parsons 1933: 76). For the Hopi clowns and bogeys she suggests Aztec parallels (Parsons 1939:1091–2), but she need not have gone so far afield. Many of the so-called *abuelos* in the eastern pueblos behave similarly. As for the interesting group of animal, bird, and insect kachinas, it can only be suggested that they have become a native Hopi specialty, stemming from the recent efflorescence of the cult.

The view which interprets the Keresan kachina cult as derived principally from Zuni seems rather unfair to the Keres, who have not been merely passive recipients of the cult, but have contributed very significantly to its development. Eggan has suggested that the Keresan peoples had originally an organization of western pueblo type, including a kachina cult, which in the east has become somewhat changed (Eggan 1950: 313), and if this interpretation is correct, we probably can see in Acoma (formerly also in Laguna) something of this earlier type of organization. Probably, between Zuni and Acoma, there has been a long period of interinfluence, with adjustments on both sides. Parsons, however, saw the relationship as one of almost complete dependence on the part of Acoma, though admitting that here the kachina cult was more thoroughly integrated with the rest of the ceremonial system than is true among the Hopi (Parsons 1939: 979). But it is hard to see that this integration is any less thorough than at Zuni, and if it is not, the alternative hypothesis that the kachina cults of the Zuni and Western Keres were coordinate developments is suggested. Seeing Acoma as the borrower, Parsons finds that the association between Town chief and Antelope clan may be a result: "Instead of developing such an independent kachina organization as the Zuni, the Acomans clustered kachina traits or functions about the Town chief; and because the chief of the Zuni kachina-kiva organization was an Antelope clansman, the Acoma Town chief had also to be an Antelope" (Parsons 1939: 979). But it seems to me very hard to believe that the advent of a new cult, presumably without much prestige, could result in a change in the method of selection of the Acoma Town chief. If, then, he has always been a member of Antelope clan, this requirement for the Zuni kachina society chief could be interpreted as a borrowing from Acoma.

Other Zuni-Acoma parallels are some items of kachina dramatization, such as the Corn clan ceremony of Shuracha and the Fight with the Kachinas; the Gomaiowish kachinas; the practice of impersonators carrying a seed pouch; the spacing of ceremonies every "four" years; the number of kivas and their close association with the kachina cult; the position of kiva chiefs as dance chiefs; the association of specific dances with each kiva; and the parallelism between the Zuni summer series of dances and the Acoma Natyati (as a much simplified version) (Parsons 1939: 979–980). Indubitably these are parallels, but the problem remains of discovering which way in each case the influence has gone.

The principal reason for the statement that they were originally Zuni traits is merely the assumption that Zuni has been *the* center of the kachina cult.

White, among others, has made this assumption, and he remarks, "The kachina cult [of Acoma] shows more affinities to Zuni than to the eastern pueblos" (White 1930: 141), but this does not prove Zuni influence unless it can be shown that the eastern pueblos have retained a more archaic type of the cult.

There are also Hopi traits at Acoma, including the tale of the acquisition of Shuracha by the Corn clan, the whipping of girls at initiation, the presence of a kachina altar (found nowhere else), the single summer rain ceremony resembling Niman, the keeping of masks in a side room or recess of the kiva (Parsons 1939: 980–981; 1936a: 559). Though the parallels are undeniable, the direction of influence remains obscure, but these traits are of great interest as proving the direct interaction of Western Keres and Hopi without Zuni mediation.

Though the kachina cult of the Eastern Keres is fairly complex, and it is clear that they share very fully in that of their western relatives, certain aspects of it show less development. One of the most significant of these features is the lack of kachina lore and mythology among the Eastern Keres as compared with the peoples farther west. As White says, this condition "might be interpreted as indicating that the katsina cult had been introduced among the Keres from some other people, but without all the lore that would make the characters and organization intelligible" (White 1942: 223 fn.).

On the whole, it may be assumed that those elements in this cult which are not of native origin among the Eastern Keres were borrowed by them from the west—from the Western Keres and in some cases directly from Zuni. Sia in particular seems to show such borrowing, as is evidenced by such (former) Sia traits as the close association of the cult with the Kurena, and the initiation of girls (Parsons 1939: 983).

In one important respect, however, the eastern Keresans have probably been influenced by the Tewa. Both Parsons and Eggan have seen the two-kiva system so characteristic of these towns as the result of Tewa inspiration, developing after the Keresan movement into the Rio Grande area (Parsons 1929: 280; 1939: 986; Eggan 1950: 313). This borrowing implies, of course, that Western Keresan organization is closer to the "original" Keresan type than is that of the east. Eggan accepts this implication (Eggan 1951: 313), and I think with good reason. But if so, though Zuni influence on Acoma has no doubt made itself felt, at least equally effective in causing the differentiation of Western and Eastern Keres has been the Tewa influence on the latter.

Special relations between the Eastern Keres and the Hopi have been noted, but these have resulted in no very profound effects, other than the exchange of some mask types (Parsons 1939: 1072).

The history of the kachina cult among the Tanoans has been almost entirely one of borrowing. Jemez has clearly taken the most—the Jemez cult is very nearly as full as that of the Eastern Keres, on which, for the most part, it is based (Parsons 1939: 985). Moiety here may be original or may have been derived from the Tewa. Their principal contribution—one might say their only one—is the so-called "Jemez" kachina, now very widespread and popular.

The Tewa have borrowed less fully, again almost entirely from Eastern Keres (Parsons 1939: 985; 1929: 280). Their contribution has been a plan of organization, based on the moiety, which has spread, I believe, to Jemez and Eastern

Keres. Hawley sees both the cult and the big kiva as prehistoric borrowings from Keres (Hawley 1950: 297). How much esoteric ritual the Tewa borrowed is hard to tell, due to their secrecy, but the externals such as mask and prayer-stick were acceptable (Parsons 1939: 940; 1929: 279).

The Tiwa, except perhaps Sandia, have borrowed less of the kachina cult than any of the other tribes. They hold a fairly typical series of beliefs concerning the kachinas, but the objective features are very meager. Such as they are, the latter seem to be due to Keresan influence, either direct or through the Tewa (Parsons 1939: 984–5). It is clear, for example, that the maskless dances of Isleta and the Turtle Dance of Taos are connected historically with the kachina cult (Parsons 1930: 347; 1936b: 91, 115), but they may not be so connected, conceptually by the performers.

It seems that the principal factors operating in the diffusion of the Pueblo kachina cult have been the following:

(1) The existence everywhere, prior to the diffusion of specific cult elements, of the underlying ideology concerning the kachinas and their general nature;

(2) The hospitality of the cult itself, both with regard to the breadth of participation in it and its consequent relative lack of the esoteric, and the ease with which innovations may be introduced;

(3) The prevalence of interpueblo visiting, by individuals, by dance groups, by large parties staying for indefinite periods (sometimes as permanent immigrants);

(4) The conscious and purposeful exchange or borrowing of ceremonies or details of ceremonialism between towns.

Even at Isleta and Taos there exists a typical series of beliefs concerning the kachinas, and it is at least possible that this ideological background has not been borrowed, but is the local expression of a very general set of concepts found over a wide area in the Southwest. Elsewhere certain more complex beliefs have probably combined with these to make the introduction of a developed kachina cult relatively easy. For example, the concepts of the Cloud spirits and the Chiefs of the Directions are in many ways similar to recent ideas concerning the kachinas, and they are probably older. As Parsons has mentioned, impersonation of supernaturals is an old trait, at least among the Hopi, though it may have been accomplished originally without masks (Parsons 1933: 76).

Participation in the kachina cult is everywhere broader than in any other aspect of Pueblo ceremonialism, which of course implies that a far greater number of individuals have knowledge of at least some of its details. As a result, kachina observances are relatively open (Parsons 1939: 944), and it seems possible that this openness results in ease of borrowing not only because observation is easy, but also because it produces a consciousness of the process of borrowing which facilitates integration, since items borrowed may be changed readily because they lack the support of tradition (Parsons 1939: 1161). On the other hand, a new trait may be interpreted as a variant of something already present (Parsons 1939: 875 fn.), thus both minimizing its significance and establishing a ground for its acceptance.

Very significant for the general history of the cult have been the efforts to integrate kachina organization with that prevailing before its introduction. This has taken the form of fitting the cult into prevailing patterns, rather than the creation of wholly new types of associations. Parsons has remarked that among the Hopi, masking "was established by being associated with clanship" (Parsons 1933: 76), and among the Keres also, "the cult was accepted and then fitted into the prevailing social pattern" (Parsons 1939: 1092). This type of acculturation seems to have occurred wherever the cult went.

The early observer Fewkes recognized as important the frequent visits made by Hopi men to other towns, during which they would observe kachinas other than their own and would often attempt to establish them at home on their return (Fewkes 1921: 496; 1903: 17). These efforts have frequently been successful, and not only among the Hopi, since interpueblo visiting, particularly in connection with ceremonies, is common all over the area. Fewkes was especially interested in the ceremonial couriers of the Hopi, young men who volunteer to visit the dances of neighboring towns (Fewkes 1892: 46). These formal emissaries do not seem to be found elsewhere.

In the west, also, a powerful factor in standardizing kachina dances has undoubtedly been the practice of exchanging visits by dance groups, between the Hopi towns and between Hopi and Zuni. Such visits have apparently been made for a long time (Parsons 1933: 5). They are not known to have occurred in the east, but borrowing of ceremonial groups and dances as such is not uncommon among the Rio Grande towns.

Quite as effective are more extended visits by groups of people who dance or otherwise impart ceremonial traits during their stay. Sometimes this involves a permanent resettlement—the visitors become immigrants. Fewkes long ago pointed this out as a factor in the growth of the Hopi cult, Tusayan having been for a long time an asylum for refugees from the Spanish-controlled east, who of course brought their own kachinas and kachina observances with them (Fewkes 1898: 193). Parsons has suggested that the so-called "Hopi" and "Zuni" dances of Jemez may have been introduced there by visitors in famine times (Parsons 1939: 985 fn.), when the western people have frequently come to the eastern pueblos in search of aid. Loans made under these conditions of immigration or extended visiting can be fuller and more exact than those made merely through observation, since there is in these cases opportunity for actual teaching. An example is the introduction of the typical Zuni kachina prayerstick into Laguna, brought by a Zuni immigrant family (Parsons 1939: 969).

Conscious, purposeful, exchange of dances, ceremonies, songs, and masks has apparently long been a common practice among the pueblos (Stephen 1936: 153; Stevenson 1904: 531; and see also Bunzel 1930: 901). Parsons remarks on this (Parsons 1939: 969), and recalls the instance described by Stevenson, when the Sia Shumaikoli masks were handed over to the Zuni Shumaakwe society, the latter offering in return to initiate a Sia youth into the Zuni kachina society and to teach him the secrets of the cult and impersonation of the Koyemshi, so that he might initiate others at home (Stevenson 1904: 531). We do not know how prevalent this kind of formal exchange may have been, but as an instrument of diffusion it must be most effective, involving as it does the conscious training of the borrower and the imparting of much, if not quite all, of the esoteric ritual.

Obviously there has been little resistance to the diffusion of the kachina cult among the Pueblos, but some evidence of it has been suggested. Parsons points out that the cult met with some opposition among the Keres, opposition from the medicine societies, which have imposed their control upon it to some extent, and have appropriated certain impersonations. She notes that at Acoma, the appointment of kiva chiefs by the Town chief must be ratified by the medicine men, Kabina society chief erects the kachina altar, and a Flint society member impersonates the whipper at kachina initiations. "In the

East, Town chief and societies achieved complete control of the cult". This statement is a little extreme, and Parsons herself goes on to say, "It is only when we reach Tewa and Tiwa that resistance or indifference is unmistakable; mask cult or usage is slight among Tewa, and among Tiwa, excepting Sandia, nonexistent" (Parsons 1939: 1092).

For Tewa, it seems not so slight as Parsons thought, but the Tiwa are another matter. At Isleta, for example, kachina ideology is present and the Laguna colonists perform masked kachina dances in the town itself, but Isletans have not taken over the mask cult (Parsons 1928a: 609; 1939: 1093). Here, according to Parsons, the people say "That they did not have mask dances when they came up [from the Underworld] and so nowadays they are not allowed to have them" (Parsons 1930: 344). Or they may belittle the kachinas as having no power (Parsons 1930: 285). Parsons has also suggested that Church control might partly account for the resistance (Parsons 1930: 347 fn.), but that another factor, perhaps more potent, has probably been the lack of craftsmanship among the Tiwa, Southern and Northern: "Mask, prayer-stick-making, and ceremonial array in the modern Kachina cult require techniques that preclude the spread of the cult to those like Tiwa who lack the techniques" (Parsons 1939: 1157, also 1093–4). The documentary material from the mid-seventeenth century, however, ascribes great numbers of masks to Isleta and other Tiwa and Piro towns (Anderson 1956), which leads one to wonder whether the modern condition is a case of resistance at all. Perhaps, instead, the present-day "Grandfather" masks are all that remains of a once more exuberant development—in which case the fact that they seem European in type may not indicate a European derivation.

It is difficult, then, to document clear-cut cases of resistance to the spread of the kachina cult. Its history, on the contrary, seems to show that it has been greeted with considerable enthusiasm almost everywhere, and its position to-day, following a period of increasing efflorescence in the last fifty or seventy-five years, would tend to confirm this impression.

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UN COMPLEXE CULTUREL: LA COURSE DE PIROGUES AU LAOS

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Les courses de pirogues, en tant que complexe rituel, couvrent une aire géographique qui s'étend de la Chine et de la Birmanie jusqu'au Cambodge, au Siam et au Laos. Certains ethnologues, tels Przyluski et son école, convertissant cette aire géographique en aire culturelle, ont réduit ce rite plurifonctionnel à une seule fonction: la décrue et la succession des saisons. La course de pirogues n'étant selon ces auteurs qu'un combat mimé entre les génies de la sècheresse et ceux de l'humidité. Or loin de pouvoir être ramenées à cette unité fonctionnelle, les courses de pirogues, semble-t-il, comprennent, selon les aires culturelles, des éléments différents qui leur donnent, une forme particulière et, dans une même aire culturelle, il est possible d'observer des manifestations différentes de ce complexe rituel, selon les sous-cultures locales; c'est ainsi qu'au Laos, cette cérémonie présente du Nord au Sud des variations morphologiques et fonctionnelles importantes.

A Luang Prabang, capitale royale du Nord Laos, cette cérémonie est accomplie deux fois par an, au mois d'août, c'est-à-dire au neuvième mois laotien, époque où les eaux atteignent leur hauteur maxima et au mois d'octobre, c'est-à-dire au douzième mois laotien, au moment de la décrue.

Le douzième jour de la lune décroissante du neuvième mois, les piroguiers du palais montés dans deux pirogues royales vont déposer un bouquet de fleurs remis par le Roi, au confluent du Mékhong et de son affluent la Nam Dong. Cette offrande est dédiée à Thao Bun Song, l'un des quinze génies ophidiens protecteurs de la capitale. Le Roi entouré de ses dignitaires, de ses pages et de l'orchestre royal assiste alors aux compétitions mettant aux prises les pirogues des villages riverains. Ces pirogues dont on ne peut se servir pour un usage profane ont près de dix mètres de long et peuvent contenir plus de trente rameurs. Partant du Mékhong, elles s'arrêtent au confluent du fleuve et de la Nam Dong.

Deux jours plus tard, les piroguiers royaux vont déposer un autre bouquet sur un rocher situé au confluent du Mékhong et de son affluent la Nam K'an, ce rocher étant considéré comme une des demeures do Thao Tong Khuang, autre Naga protecteur du royaume. Les pirogues des villages riverains font alors assaut de vitesse sous les yeux du Roi. Les courses, cette fois-ci, se déroulent dans la Nam K'an et non plus dans le Mékhong et le point d'arrivée est marqué par le confluent de ce cours d'eau et de son affluent le Houei Kang. Quelle fonction attribuer à cette cérémonie? Si nous nous reférons aux textes laotiens—aux Annales de Vientiane entre autres—les Nagas et les génies ophidiens qui sont sous leurs ordres sont censés résider, durant la saison des pluies, dans les rizières, les mares, les points d'eau et circuler dans le fleuve durant la saison sèche. Aussi selon la croyance populaire, les noyades dans le Mékhong imputables au ressentiment des Nagas sont-elles plus fréquentes durant la saison sèche que durant la saison des pluies.

Si nous nous basons sur ces croyances attestées par les textes nous pouvons induire que cette cérémonie du neuvième mois a pour but d'assurer le passage des génies ophidiens du fleuve jusqu'aux points d'eau intérieurs qui serviront de déversoirs au fleuve et assureront la fertilité des rizières. Or si nous examinons le lieu où se déroulent les courses, le point de départ et le point d'arrivée des pirogues, cette hypothèse est confirmée. En effet les courses du douzième jour de la lune décroissante se déroulant dans le Mékhong avec pour point d'arrivée l'embouchure de la Nam Dong ont pour but d'assurer le passage des Naga du fleuve dans un affluent principal. Les courses du quatorzième jour se déroulant dans un autre affluent du Mékhong, la Nam K'an avec pour point d'arrivée l'embouchure du Houei Kang ont pour but d'opérer le passage des Naga des affluents principaux aux affluents secondaires. Ainsi par l'intermédiaire des principaux cours d'eau tributaires du Mékhong, le passage des Naga est effectué progressivement jusqu'à l'intérieur des terres.

La course de pirogues du douzième mois ne constitue plus une cérémonie particulière mais un des nombreux rites de la fête du "That Luang", le reliquaire du royaume. Le treizième jour de la lune croissante, le chef des piroguiers royaux place, à l'avant d'une des pirogues royales, deux plateaux contenant des poulets bouillis, des fruits, des chiques, de l'alcool. Il répartit entre la proue et la poupe ces offrandes et mets qui sustanteront la pirogue et ses cent vingt âmes. A l'intérieur de la pirogue, il dispose deux autres plateaux contenant des mets, des fleurs et des cierges. Les piroguiers montent ensuite dans l'embarcation et vont répartir ces mets entre l'autel dédié aux mânes des piroguiers royaux décédés et les différents rochers demeures des Naga protecteurs du royaume. Il y a une dizaine d'années, ces rites propitiatoires étaient suivis de courses qui mettaient successivement aux prises les pirogues des reines (Reine de droite et Reine de gauche) ainsi que celles des principaux dignitaires. Ces courses étaient réglées de telle sorte que la victoire respectait les préséances hiérarchiques, ainsi la pirogue de la Reine de droite l'emportait toujours sur celle de la Reine de gauche, la pirogue du Vice-roi l'emportait sur celle du "Rajavong".

La signification de ce rite ne peut être comprise qu'après un examen de l'arrière fond historico-légendaire de cette cérémonie du "That Luang". A Luang Prabang en effet cette cérémonie a pour but de retracer au moyen de rites complexes la fondation légendaire du royaume créé au dépens des eaux. Selon une version que nous avons recueillie, un couple de vieillards considérés comme les ancêtres mythiques des Laotiens furent chargés par le Roi des Dieux de créer le monde en piétinant les flots. Par la suite ils devinrent les génies tutélaires des Laotiens et, chaque année, la danse des ancêtres exécutée par ceux personnages masqués constitue un des rites principaux de la cérémonie du That. Cette danse ayant pour but de recréer le territoire, de faire surgir magiquement la terre des eaux nous comprenons maintenant le rôle dévolu à la course de pirogues. Se déroulant dans le Mékhong à partir de son confluent avec la Nam K'an elle avait pour fonction de contribuer à l'évacuation des eaux en faisant passer à nouveau dans le fleuve les Naga qui séjournaient dans les rizières, mais un rite étant toujours plurifonctionnel, ce drainage magique n'était qu'une des fonctions de la course de pirogues.

Le territoire recréé, il importait ensuite d'instaurer la royauté et les cadres administratifs, c'était là l'autre fonction de la course de pirogues. Mettant aux prises les membres du palais et les représentants de l'administration royale, elle permettait en respectant l'ordre des dignités de fixer l'ordonnancement du royaume. De la recréation mimée du territoire on passait ainsi à l'instauration administrative. Si l'on ajoute que durant la cérémonie du That, le Roi s'abimait en méditation durant onze jours près du reliquaire boudhique, réservoir de cette loi suprême, essence secrète de la royauté qu'il entendait implanter sur terre, nous voyons que par sa seconde fonction, la course de pirogues contribuait à assurer le succès de la magie royale.

A Vientiane, dans le Moyen Laos, capitale d'un ancien royaume, la course de pirogues a lieu au onzième mois, c'est-à-dire en octobre, à la sortie du Carême Boudhique, au commencement de la décrue. Le quinzième jour de la lune croissante les piroguiers de deux villages riverains costumés en pages royaux conduisent sur le banc de sable qui s'étend au milieu du Mékhong, les notables chargés par le Préfet de la ville de déposer une coupe d'offrande à l'autel du Naga Soukanta. Selon la version laotienne du Ramayana, ce Naga serait le petit-fils du génie ophidien Sissatanaga qui indiqua au Roi Tattalata venu du Cambodge l'emplacement où il devait élever sa capitale, c'est-à-dire Vientiane. Au retour, les notables présentent des offrandes aux autres génies protecteurs—pour la plupart rois divinisés—dont les autels se dressent çà et là sur la berge du fleuve. Le lendemain, les villages riverains procèdent à la mise à l'eau des pirogues qui participeront à la cérémonie.

Dans chaque localité avant la mise à l'eau, le maître du rituel à qui incombe la garde de l'autel du village va avertir la "Médium" qui incarne les esprits protecteurs de la communauté. Cette "Médium" présente alors des offrandes aux génies du village, les invite à descendre dans la pirogue pour participer aux courses qui se dérouleront dans la capitale et assurer la victoire des villageois. Tombant en transe, elle incarne tour à tour les différents génies puis danse autour de la pirogue pour l'exorciser. L'embarcation mise à l'eau, la Médium et le maître du rituel assis à la proue surveillent la coupe d'offrande et l'oreiller —couche des génies—qui sont fixés à l'avant. Quand les pirogues arrivent à Vientiane, le personnel sacré de chaque pirogue doit offrir de l'alcool et des chiques au génie de l'embarcadère. Après les compétitions, une course spéciale est disputée uniquement par les femmes. Il semble que ce dernier rite soit la survivance très altérée d'un état plus ancien de la cérémonie qui attestait indiscutablement une acculturation siamoise. Autrefois en effet, les courses opposaient la pirogue du roi à celle de la reine et cette dernière devait gagner pour assurer la prospérité du pays.

Présence du personnel sacré surveillant la couche des génies protecteurs à l'avant des pirogues, compétitions féminines attestant un état plus ancien du rite, ces éléments donnent au complexe rituel une structure particulière. Si la course de pirogues a également ici pour fonction d'opérer le drainage magique des Naga, les génies des villages, par l'intermédiaire des médiums et des maîtres du rituel, contribuent activement à cette opération.

Dans le Sud Laos, la région de Bassac qui fut le berceau du Royaume des Kambujas devint au XIVe siècle le centre d'un royaume laotien, le Royaume de Champassac. Réorganisé au début du XVIIIe siècle par un descendant des rois de Vientiane, ce royaume tomba par la suite sous le joug Thailandais avant d'être démembré par les traités franco-siamois. Actuellement il ne forme plus qu'une circonscription de territoire laotien, mais Bassac est demeuré en fait le fief des Princes du Sud dont le titre, jusqu'à ces dernières années, était héréditaire.

A Bassac, la course de pirogues a lieu de même qu'à Vientiane à la sortie du Carême Boudhique, au onzième mois laotien. Le premier jour de la cérémonie, le personnel sacré avertit les génies protecteurs de la principauté et leur présente des coupes d'offrandes provenant de la famille royale. Des Kha, aborigènes indonésiens descendants des anciens esclaves des rois de Champassac participent à ce rite d'avertissement et montent dans la pirogue consacrée aux génies protecteurs pour délimiter l'aire où se dérouleront les courses. Chaque jour, durant les quatre jours que dure la cérémonie, la pirogue des génies protecteurs montée par des Kha doit accomplir ce même circuit sinon de graves accidents risqueraient de surgir durant les compétitions. Toutefois cette pirogue ne participe pas aux courses, seules y participent les pirogues des villages riverains. Quand ces pirogues arrivent à Bassac, les dignitaires de la principauté présentent des coupes d'offrandes au personnel sacré de chaque embarcation, car de même qu'à Vientiane, à l'avant des pirogues, maître du rituel et médium surveillent l'oreiller qui sert de couche aux génies des différentes localités. Ces oreillers ainsi que les objets de dignité des génies: sabres, coupes, sont placés ensuite sur de petits autels élevés le long du Mékhong, de chaque côté du "Pavillon de la Victoire" où le Prince de Champassac assiste aux compétitions. Sur les autels de la principauté et sur ceux dressés par les chefs du rituel, en l'honneur des génies de leur village, des coupes d'offrandes sont déposées le dernier jour de la cérémonie. Ces coupes contiennent des mets préparés avec la chair d'un buffle qui a été sacrifié la veille dans la nuit. Après les compétitions les embarcations, suivies cette fois-ci de la pirogue des génies protecteurs montée par les aborigènes, accomplissent un tour d'honneur, pendant que le personnel sacré des autels lâche au fil de l'eau un radeau chargé d'offrandes destinées aux génies ophidiens.

Selon les Annales historiques du Royaume de Champassac, ce serait le Roi Soi Sisamut qui aurait organisé cette cérémonie en 1725 lui donnant le caractère qu'elle a conservé jusqu'à nos jours. Son rôle consista cependant moins à créer qu'à grouper un certain nombre de rites autour de ce qui constituait jusqu'alors la cérémonie principale de Bassac: le sacrifice du buffle. Ce rite est en effet bien antérieur à Soi Sisamut. Selon les Annales, au début du XVII^{e} siècle, la jeune reine de Champassac, Nang Pao eut des relations amoureuses avec le Prince d'un état voisin qui était venu chasser l'éléphant dans la région. Le Prince repartit bientôt laissant Nang Pao enceinte de ses œuvres. Au moment d'accoucher, éprouvant de vives douleurs, elle s'écria: "Due désormais toute jeune fille qui se laissera séduire, offre un buffle en sacrifice aux génies protecteurs du royaume et qu'elle se marie avec son séducteur. Si ce rite n'est pas observé que le riz dans les rizières se dessèche et périsse." Elle fit enregistrer cette déclaration, y apposa son sceau et ordonna que cet édit fût mis en vigueur dans tout le royaume. Deux fois l'an, au sixième mois avant les labours et au onzième mois lors de la course de pirogues, des buffles sont offerts aux génies par les filles mères de la principauté. Le sacrifice du buffle ayant au Laos pour fonction principale de provoquer la pluie—c'est pourquoi il a lieu généralement à l'époque des labours—il peut sembler surprenant qu'il ait été accouplé à la course de pirogues du onzième mois dont une des fonctions à Bassac comme à Vientiane et à Luang Prabang est de provoquer, avec le passage des Naga dans le fleuve, l'assèchement des points d'eau intérieurs. Certains rites de pêche du Sud Laos permettent d'expliquer cette contradiction apparente. En effet selon le droit coutumier, les pêcheurs ne peuvent établir des barrages dans les affluents du Mékhong avant la fin de la course de pirogues de Bassac car ces barrages pourraient retenir les Naga prisonniers. Ces Naga commencent à quitter les affluents lors des premières courses et ceux qui pourraient demeurer dans les points d'eau intérieurs coupés des cours d'eau sont pour ainsi dire remis à flots par la violente pluie qui accompagne toujours le sacrifice du buffle, la veille de la clôture des courses. Le sacrifice du buffle contribue ainsi, en dégorgeant par une brève crue les points d'eau obstrués à assurer le circuit des Naga. En plus de cette fonction, qu'il acquiert en étant accouplé à la course de pirogues le sacrifice du buffle à Bassac conserve son sens propre qui est de faire rentrer dans l'ordre ceux qui se sont fourvoyés. Les fautes génératrices de pestilence étant sanctionnées, l'ordre naturel compromis sera rétabli, les saisons se succèderont régulièrement sans qu'aucune perturbation ne se produise. Mais cette course de pirogues possède encore d'autres fonctions: comme à Luang Prabang elle instaure, recrée magiquement le royaume à partir d'un temps originel qui précéda l'arrivée des envahisseurs laotiens. Montés dans la pirogue du génie protecteur, les Kha aborigènes représentants des génies locaux des montagnes et des eaux—génies qui existaient bien avant l'arrivée des Laotiens et de leurs dieux conquérants—délimitent et propitient l'aire sacrée dans laquelle se dérouleront les compétitions. Dépositaires du bonheur et de la prospérité de la principauté, ils font valoir durant la cérémonie leur droit d'aînesse, leurs moindres désirs devant être réalisés. Ce renversement temporaire de la société annule moins l'histoire qu'il ne préside à sa recréation, car le dernier jour de la cérémonie les aborigènes, par l'offrande de jarres d'alcool au Prince de Champassac renouvellent l'antique contrat avec le descendant de ceux qui les dépossèdèrent et reconnaissent ainsi le droit nouveau qui leur fut imposé par les envahisseurs Laotiens.

A Luang Prabang, le royaume une fois recréé, surgi des eaux grâce à la danse des personnages masqués et à la course de pirogues, l'instauration administrative était réalisée par des compétitions respectant l'ordre hiérarchiques. A Bassac aucune compétition stéréotypée n'assure la victoire aux dignitaires les plus élevés en grade, nul doute toutefois que le Roi Soi Sisamut n'ait organisé cette cérémonie à des fins administratives et politiques. Quand il monta sur le trône en effet, le pays était en pleine anarchie et son premier soin fut de diviser le territoire en fiefs dont il confia la garde à des feudataires qui, par la suite, essayèrent maintes fois de se libérer avec l'appui du Siam. La course de pirogues fut ainsi un moyen pour lui de renforcer l'autorité de royaume en resserrant les liens existants entre les vassaux et leur suzerain. De même que dans la Chine ancienne, chaque année, les vassaux venaient du plus loin de leur province se grouper avec leur petite motte de terre autour de leur souverain pour renouveler les énergies du dieu du sol dont une parcelle avait servi à établir leur fief, de même les feudataires du Royaume de Champassac venaient chaque année, à l'occasion de la course de pirogues, recomposer autour du Roi toutes les énergies dispersées. Leur pirogue à l'avant de laquelle reposait sur un petit matelas le génie protecteur de leur fief remplaçait la motte de terre des princes chinois. Tout en roborant le pôle religieux du pays, ils rechargeaient les énergies de leurs génies locaux et de retour dans leur village, ils diffusaient à leur tour, lors des courses locales—la cérémonie de Bassac ouvrant la période des courses de pirogues dans tout le royaume—l'influx reçu là-bas au centre spirituel du royaume. Cette fonction politique soigneusement entretenue par tous les descendants de Soi Sisamut permit longtemps au Royaume du Sud de lutter contre le démembrement. La puissance territoriale de Champassac une fois perdue, l'unité du Laos réalisée sous le Protectorat Français au profit des

Princes de Luang Prabang, la course de pirogues de Bassac répétant indéfiniment une phase determinée de l'Histoire—celle de l'apogée du Royaume du Sud sous Soi Sisamut—demeura l'instrument d'une politique séparatiste.

Selon Przyluski et son école, la course de pirogues aurait uniquement pour fonction d'assurer la décrue en saison sèche par la défaite des génies de l'humidité, or l'examen de cette cérémonie sur une seule aire culturelle nous permet de préciser qu'il s'agit—du moins au Laos—d'un complexe rituel plurifonctionnel dont la structure et les fonctions varient selon les sous-cultures locales représentées par les royaumes de Luang Prabang, de Vientiane et de Champassac. Sous les variations il est possible certes de déceler une fonction commune, mais cette fonction est d'assurer le remplissage et le drainage des points d'eau intérieurs en opérant le circuit des Naga et non de provoquer la décrue par l'expulsion des génies ophidiens. S'il s'agissait uniquement de provoquer la décrue à la fin de la saison des pluies, comment en effet expliquer l'existence de la cérémonie au neuvième mois à Luang Prabang, et s'il s'agissait d'expulser les Naga comment rendre compte de la croyance selon laquelle en saison sèche ils circulent dans le fleuve?—Outre cette fonction commune, principale ou secondaire selon les sous-cultures envisagées et relevant probablement d'un substrat ancien, la course de pirogues peut retracer une phase d'un scénario historico-légendaire ou se charger d'intentions éthiques et politiques.

Ces variations ne doivent pas être négligées car c'est grâce à elles que les cultures acquièrent des formes distinctives.

L'Ecole Française d'Extrême Orient,
Laos.

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THE IDOMA COURT-OF-LINEAGES IN LAW AND POLITICAL STRUCTURE[1]

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In our generation, at least, the quest for a completely satisfactory definition of law is probably a hopeless one. It is not for that reason a useless quest. The progress of ethnological science brings us every year fresh examples of legal process in a great variety of societies. These materials shed new light on European law and by the same token force us to a constant re-study of the body of concepts with which we attempt to bring ethnographic data to bear on the legal and social problems of modern civilization. I mention these considerations partly to show the larger relevance of the materials which I have collected in the Idoma country of Nigeria and partly in order to avoid, in this short paper, the question of whether and to what extent the Idoma have law. For the immediate purpose, Hugh Goitein's definition will serve very well: "It is proposed to define law for the jurist as the sum of the influences that determine decisions in courts of justice" (Goitein, p. 198). Under such a definition, the body of legal statements that make up the code or the common law would figure as one important influence on the courts' decisions, but only one. Another effect of this definition is to throw the burden of the problem, for the ethnographer of law, onto the concepts "decision", "courts" and "justice". The process of decision-making in primitive society has not yet been the subject of much study, although some of the materials which I have collected bear on it. I shall, in this paper, avoid discussion of the thorny problem of the nature of justice with the comment that for our present purpose the Idoma notion of justice is reasonably similar to our own. This paper, then, will deal with Idoma law by considering the structure and activity of a particular set of Idoma courts.

The Idoma are, like most Africans, an agricultural people. They live, for the most part, south of the Benue River and about a hundred miles east of the Niger river in Nigeria. The tse-tse fly makes cattle-keeping a near impossibility in this region, but a large variety of crops is grown, including those proper to the rain-forest of Southern Nigeria and to the dry savannahs of the north. Idoma today exports considerable food to the rest of Nigeria. The Idoma live politically in about twenty districts, or "lands", to use their own name for this unit. Most of these districts are in Idoma Division and are today, together with six ethnically different districts, under a paramount chief, the *ɔcídɔ̌ma*. A more or less typical land is made up of from two to four related patrilineal lineages, which select a Land Chief, according to the widespread African pattern of rotation: each lineage in turn has the honor. The effect of this is that the King, although a sacred personage, is not very strong politically. He is in fact a creature of the lineage corporations; and when they cannot agree on the succession for some reason, there may be a protracted interregnum. The lineages hold large, unified tracts of land, but the sub-lineages, despite their corporate nature, do not. Instead the lands of a lineage tend to be divided into sub-lands, or villages, in each of which there will be compounds from several sub-lineages. Despite its dispersed relation to the land, the sub-lineage is a corporate group; despite the fact that it forms a specially unified, named neighborhood, the village is not a corporate group. I have explained this whole situation in other places at greater length.[2] The summary statement given here applies mainly to the central Idoma districts, including especially the Oturkpo District.

I only discovered the existence of the institution which I call the Court-of-Lineages after I was well into my second year in the field, and after I had achieved sufficient mastery of the Idoma language that I was able, with the help of Idoma assistants, to make reliable records of what was said at several proceedings—especially inquest proceedings. I have already published the full, translated text of one of these inquests (Armstrong, 1954). I should like here to present certain other materials and then to comment on the way they illuminate Idoma law and political structure.

I have called the Idoma court-of-lineages an institution. It is not a corporate institution, however, for it is assembled *ad hoc* for each occasion. It is the lineages which are corporations: their spokesmen or representatives sit as a court in particular cases. The make-up of the court is determined first of all by the spread of the interests involved in the case. The general rule is that a matter between members of two compounds will be judged by the council of the sub-lineage, between members of two sub-lineages, by the council of the lineage, between members of two lineages by a court made up of all the lineages. A matter which involves someone from outside the Land is also brought before a court of all the lineages. Since these courts as such have no corporate continuity, they cannot be the repositories for the traditions, records, and experience that are put to use in handling particular cases. It is rather the lineage corporations which are the respositories for these traditions. The spokesman of the senior lineage participating in a court is chief justice, but not necessarily presiding justice. Since the courts have no continuing existence, they can have no sheriff or police arm with which to enforce their orders. Indeed, the term "enforce" scarcely suggests what really happens.

Our terms "crime," "offense," "delict," etc., on the one hand, and "punishment," and "enforcement" on the other contain the implicit assumption of a society and a world at peace, which peace is interrupted by the delict and by official acts of punishment and enforcement of law. The Idoma—and perhaps African—conception seems to me just the reverse of this. The individual and the society live in an ocean of arbitrary force, from which the individual is protected by the religious activities of the elders, by the use of medicines (produced by the elders), by the orderly settlement of disputes as they arise, and by the armed force of the whole society acting defensively. The court, by removing what might be called "the protection of the law" from a convicted person, exposes him to the enemy outside, to private vengeance, or to the tender mercies of the secret men's societies, who would perhaps raid his compound and levy a fine. In the old days such a person was, in extreme cases, in danger of his head or of his freedom. I do not mean by this argument to imply that African society was, on the whole, more "lawless" than our own. I am trying, rather, to explain how their courts were able to give effect to their decisions in the absence of an organized, responsible police force. (The men's societies could not be called to account for their acts. They might be "unleashed"; then, if they seemed to be going too far, there were formal procedures by which the lineage elder could stop them.)

In what sense, then, do the Idoma have courts? The institution to which I give this name lacks corporate continuity and cannot enforce its orders (though it can give effect to them). There is an Idoma word for "justiciable controversy" (ɛρə̆), which one wins or loses in an adversary proceeding. But there is no separate word for "court"—indeed they have borrowed the English word to refer to the courts, "*ukóotu*," of the Native Authority, which the English have instituted. When the traditional court is formed of representatives of the councils of more than one lineage, it is referred to as "*ojuju*," which is a consultation, a "huddle," or even a conspiracy. I call this institution a court because it acts like a court: it has a strong sense of due process, it conducts adversary proceedings and formal investigations, it is structured so as to provide a maximum of impartiality in the decision-making authority, it conducts its hearings publicly and with a very highly developed sense of drama.

I should like at this point to give a number of examples from proceedings which I attended to illustrate the points mentioned above.

The following is the beginning of the inquest held for Odélĭgbo, a middle-aged woman who had lived in the same village, Ákpégĕdĕ, where I lived. The proceedings, as usual, take the form of an argument between the lineage and sub-lineage groups present, the actual words being spoken by representatives of the most senior elders of the respective groups. Owoico presides and represents Ókwúmă, the head of the lineage to which all present belong. Enonce is village-head of Ákpégĕdĕ, where the inquest takes place. He speaks, however, as representative of Ábe, the senior elder of the husband's sub-lineage. Agbó speaks as representative of Ikwúe, the head of the deceased lady's sub-lineage, resident in Ŭpŭ, about eight miles away. The proceedings begin with the formal opening (as usual, every sentence is spoken twice, once by the elder concerned, and again by his spokesman):[3]

Owoico: Enonce, Ijáce! What caused you to send for me? Speak, that I may hear!

Sons of Onyebe (Enonce speaking): Okwuma, ejila! Ecekwu, Audi! Alecenu, Odeejo! Ikwúe, Odu! A corpse cannot be hidden. If a corpse could be hidden, I would hide this one.

Sons of Ode (Agbó speaking): My father, Ikwúe has asked me: the thing that you people are doing, I don't understand it. Tell me so that I may tell the council, since she was my child. I miss her much.

Sons of Onyebe: Why should you miss her more than I? It is in my compound that she put her house, so doesn't that make me miss her more than you?

Sons of Ode: Ábe, Ocagwu, this thing which you and I are dragging out is not good. Ábe lives in Akpegede, I live in Upu. How shall I know what happens to a person in Akpegede?

Sons of Onyebe: I divined: you and I divined! That is why I say that you knew what was wrong with her. . . . Ikwue sent you with a message, you agree. Isn't that his messenger? (I.e. Can't you take the message back?) The divination which I divined, it was the medicine of your compound against which I divined, and the flesh of her son who had died and which she said she had eaten.[4] I also divined against the Okpokplo Anjenŭ (spirit). That was in your presence that we divined thusly!

Sons of Ode (Agbó speaking): I did not sit and divine with you people. I was merely on business of my own. Tell me what killed my daughter!

Sons of Agbooko (Owoico speaking, as chief justice): The thing which Ikwue says is the better. You actually did not divine.

This interchange shows the court calling the sub-lineage which was responsible for the deceased (i.e. the sub-lineage of her husband) to account for its actions during her final illness. Since in the Idoma view the successful performance of the divination necessary to identify the cause of the disease requires the participation of representatives of the sub-lineages and lineages concerned, the problem is essentially an administrative one. An elaborate commentary would be necessary to make the above text fully understandable to a European. It is sufficient for our present purposes to show how formal the proceeding is in a quite ordinary case and to show how the lineage and sub-lineage corporations take part in the action as unit groups. They even can be said to have corporate personality, since they are constantly referred to by the name of their respective most senior elders, who are in fact not even present at the inquest. It was through the study of such proceedings that the nature of the Idoma lineage structure finally became clear to me.

It is sometimes said (e.g. in Marett's article on Primitive Law, *Encyclopaedia Britannica*, 14th ed. (1945), Vol. 13, p. 781) that "Little of course can be found in primitive jurisprudence that corresponds at all to the modern insistence on relevancy . . . etc." This is not the place to ask how primitive or how advanced Idoma jurisprudence may be. Much of what is said in their proceedings is apt to seem irrelevant to us because we do not know the full meaning that the statements have to the participants. Further along in the inquest which we have been discussing, the court explicitly rules a statement irrelevant, however. The court has been discussing a recent series of domestic misfortunes in connection with another claim that an important elder was not properly notified of the illness:

Ocinyabǒ: The wife of Adoka is dead (i.e. Odeligbo, the subject of the inquest). Ábe is dunning Ecekwu (two very senior elders). The wife of Egbudu (who left him) has returned the (bride-wealth) money to him. Ábe is dunning Ecekwu!

Owoicǒ: The matter which you bring before the council, it is out of order! . . . (More literally, "It does not fit.")

Ocinyabo, a notorious chatterbox, and Owoico have been close neighbors for many years. For Owoico to cut him off so bluntly in an ordinary situation would be an insult.

At the inquest on the death of Okwuma, the senior elder of the largest, but junior, lineage of Oturkpo Land, an interesting constitutional issue was brought before the court, not because the issue had anything to do with the inquest, but because the court as it sat was so constituted as to be able to handle the dispute, which lay between the Chief himself and a commoner. A word of general explanation is necessary. In Idoma, as in much of the rest of Africa, one of the basic symbols of kingship is the obligation to "serve" the king with trophies of fierce animals which one has slain. Thus, lion and leopard skins, elephants' tusks, buffalo horns, the red feathers of the uloko bird (a headhunting symbol) are all to be given to the king. He does what he wishes with them, and will probably reward the donor with a title. A neighbor of mine had killed an aardvaark a month before and had been thrown into the N.A. prison by Ujo, the young acting chief, President of the N.A. Court. The prisoner's sub-lineage head brought the matter up:

Una: I am going to tell you about just one thing! (Contains the implication that everybody knows what he is going to say.) My son is today in jail because of an aardvaark. If chiefs are to be served with aardvaark, tell me!

Sons of Odaji (Óceocé, chief justice of the court, speaking): The question which you should bring me another day, is that what you people have brought me today?

Ujo (representing his aged father as Chief of Oturkpo): The man says that since he is here he must speak his piece so they may hear! As you *have* a king, a man named Adaamuda killed an aardvaark and brought it to serve my father. He brought a leopard-skin to serve my father. The sons of Agbooko were my witnesses; the sons of Odaji were my witnesses. Una, speak so I may hear, for you yourself once served my father. Then your son today killed (something) and hid it. Then I arrested him. The sons of Agbooko were witnesses at that time. If service is not being done, you people tell me! (A musket is discharged at the edge of the crowd.)

Una: I did not serve you with the aardvaark. Oko (a recently deceased senior elder) killed an aardvaark and did not serve the King. Eka killed one last year; he did not serve the King. If a man finds a slave, he must serve the Chief. If a man kills a leopard, he must serve the Chief. If you kill an elephant, you must serve the King. Oko and Ogli are friends; should he refuse to take meat to his friend? Owoico killed an aardvaark; he did not serve the Chief. (Owoico's son killed it in fact, but in this relationship Owoico is responsible.) That aardvaark meat I gave you as a gift, does it mean that if a person gives a gift it changes into force?! (I.e. establishes a precedent.) Another time I will not agree to give a gift!

The Talking Flute (accompanying the red Oúnă Mask): There is room in the clearing for the red-monkey and the dog to compete! (An encouragement for the mask to continue hazing the spectators.) (The red-monkey is the taboo, "totem" animal of Oturkpo Land.)

Unknown person: Onyakwú, give the answer to this matter so we may see it, since you are the senior. (Onyakwú is the absent senior elder who is head of the senior Sons of Odaji lineage.)

Sons of Odaji (Óceocé speaking): Una, okala! Onmiinyi, Ojooko! What about the Chiefs which our fathers became? Una spoke now; Ujo spoke now. He said that when formerly they killed (an aardvaark) they had to serve his father; and now you are discussing it. You (Una) did not tell me before. There is the corpse which will spoil! I do not see the answer to that matter today.

Council (of the Sons of Agbooko, presiding): Is this the sort of thing that they save up (for a future occasion?) We refuse! Decide it for them! (N.B.: The prisoner is a member of this lineage.)

Sons of Odaji: All right. Keep quiet! Formerly they used to kill the uloko bird and serve the King with it. They used to kill python and serve the King. With ichaklo and leopard they used to serve the King. They would kill elephant and serve the King, from the time of my father. But nobody ever killed an aardvaark and served the King!

Una, do you hear? You have won the case!

This case not only gives us an authoritative list of the animals with which chiefs were served, but it also sheds a very strong light on the relation of the chief to the lineage corporations. It shows that the court-of-lineages could sit in judgement on the act of the King in a case between the King and a very humble commoner, involving the very honor of the King. And it shows that in such a case the court did not hesitate to give the decision to the commoner. It likewise shows that the Idoma have a body of precedent which may, in some sense, be called law, and that they have a judicial institution which provides judges to give that law authoritative, public interpretation.

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Notes

1. The material for this paper was collected during a field study under the auspices of the Colonial Social Science Research Council, Colonial Office, whose support is hereby gratefully acknowledged.

2. Armstrong, 1954, 1956.

3. Everything in parentheses is editorially added explanatory matter.

4. N.B.: All that any Idoma would understand by this statement is that she dreamed of some kind of meat in some connection with her son. The implication is that her son was perhaps bewitching her.' For a more detailed discussion of dream witchcraft, see Armstrong, 1954.

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THE FEAR IN TAPIRAPÉ CULTURE

Herbert Baldus

Fear, although its biological aspects connect mankind with the animals, assumes among ourselves characteristics which by tradition vary from people to people, that is, cultural traits. Socially determined differences exist in the exteriorization of fear as well as in its causes.

In certain cultures one should not show fear; in others there is nothing to prevent one from exhibiting it. There are peoples and cultural epochs which like to see their women habitually frightened and so to establish a contrast with the calmness of the men. In many South American tribes the women easily weep, tremble, cry or groan because of any danger, real or supposed, and are or pretend to be especially frightened by certain masked dances and other ceremonies performed by the men.[1] Goethe, in "Faust," when Margaret faints, exclaiming "Nachbarin! Euer Fläschchen!" ("Neighbour, your smelling-bottle!"), alludes to a common custom of his time in Europe. The modern European woman who no longer uses smelling-salts shows that such manifestations of terror are not conditioned by nature, but by culture. As regards the real female constitution it may be mentioned that "in an important study made during World War II, *Psychological Effects of War on Citizen and Soldier* (1942), Dr. R. D. Gillespie reported that in the heavily bombed areas of London and Kent almost 70 per cent more men broke down and became psychiatric casualties than women!"[2]

In certain cultures death is feared, in others, not. Circumstances which produce feelings of inadequacy and consequently fear in certain societies don't exist in others. The so-called "imaginary fear"[3] which creates phantoms is found among most primitive peoples and is generally limited, among the peoples with European languages, to the less educated classes.

The Tapirapé differ from other South American tribes by exteriorizing the fear in both sexes, by the frequency and ease of such exteriorization, and by the multiplicity of causes for fear. The naturalness with which on several occasions Kamairahó, the most important Tapirapé leader, said to me: "Akyitié, I am afraid," is only comparable with the almost automatic manner of the Tapirapé's using the interjection "Aký" when feeling any pain, even the sting of a mosquito bite. This caused me to think frequently of the contrast presented in all that by Indians I knew in the Paraguayan Chaco, for instance of the apparent impassibility of the Chamakoko. It is true that the Tapirapé calm down with the same ease with which they are alarmed. A few words of mine were enough to make Kamairahó laugh again, speaking carelessly with his wife about other things. I had the impression that among the Chaco Indians the emotions penetrated deeper, while among the Tapirapé they were like slight circles on the surface of a pond into which a little stone had been thrown, circles which soon disappear into the distance.

But among these Tupian Indians of the Araguaya basin, the exteriorization of fear is not restricted to interjections. We find it expressed in attitudes and institutions. We feel it, although masked, in the mechanical smile with which, from earliest infancy, they greet the person who speaks to them. We suspect it still in other aspects of their obsequiousness.

Fear was present in the formalization of quarrels full of hatred, a formalization which obliged the opponents to avoid direct contact. They had to remain hidden from one another and not interrupt the speech of their antagonist, but only reply when he had finished. One such quarrel began in the afternoon of June 27, 1935. Kamairahó lay in his hammock with open eyes and didn't answer when several people called him. Maninohó, the eldest man of the neighbouring house, begins to pronounce insults. He stays inside his house. The entrances of the houses are so arranged that it is impossible to look from the interior of one house into the interior of another.[4] Kamairahó's wife stays outside between the two houses without looking towards either of them. She faces the men's house in the centre of the village and replies to Maninohó with extraordinary gravity, in a level voice and with calm gestures. They are discussing a question of shamanism. Maninohó seems to be doing the provoking. The other persons present continue with their habitual activities, neither laughing nor speaking. After a while Maninohó's aggressiveness seems to increase. Kamairahó's wife joins her husband and the eldest man of her house places himself close to the entrance but inside, holding a club and railing violently in a loud and monotonous voice. When he finishes, profound silence rules for some moments. Then the other replies in the same tone. Never does one interrupt the other. This rule is also observed when two Tapirapé women have a quarrel as well as among other South American tribes.[5] The quarrel between the two men, one always hidden from the other, lasted until sunset. Late that night the old man of Kamairahó's house, where I was also living, asked me for my electric torch and went with his wife for water by a very roundabout way in order to avoid the proximity of Maninohó's house. These precautionary measures—the lamp, the company of his wife, and the détour—reveal clearly the fear he had of the adversary. Supposing then that fear has contributed to create the institution of formalized quarrels we may consider it a factor of socialization. But is it only a tribal behaviour pattern or also fear that causes the victim of theft to abstain from any exteriorized reaction? Frequently I observed that the theft of some thing obtained from the white men, for instance a necklace of glass beads, was known immediately in the whole village; the victim, however, suffers in silence without any demonstration and doesn't attempt any revenge.

As an institution which is essentially a product of fear appears that of the shamans. Nonexistent in certain South American tribes and little developed in others, it is so important to the Tapirapé that they informed me of certain local groups having renounced their independence and joined the inhabitants of another village because there were no more shamans among them.[6] This becomes credible when one considers Charles Wagley's[7] quotation of one of his informants' words: "Without our panĉé, all we Tapirapé die." According to the same author,[8] "the greatest prestige which Tapirapé culture offers accrues to the shaman." This means that the first place in Tapirapé society belongs to the person who defends the community against the ever terrible dangers of the supernatural world. It is collective fear which gives him this status. But the easy and continuous contact with the forces of destruction makes the shaman for his part also a man to be feared, and consequently exposes him to other dangers, to dangers which we would not call supernatural. When cases of death occur in the village, and especially several in a short time, it is usual to attribute the cause to a shaman and therefore many shamans are murdered. Then it is also collective fear which, in such cases, moves a Tapirapé to defend the community against the supposed menace and to kill the shaman who is suspected of having become an evil sorcerer.

Though, also as regards the institution of shamanism, fear works out as a factor of socialization by exorcising the dangers to the tribal life, we cannot say the same about the fear which produces in many Tapirapé a disposition to adapt themselves complacently to the white man. During my stay in 1935, in the village called Tampiitawa, I could distinguish clearly between the great group of timorous people led by Kamairahó who received with open arms all that came from our civilization, and a smaller group composed of decided and valorous people who didn't want anything to do with us. So there were two opposite trends in their "foreign policy" and consequently germs of tribal disintegration.

Of the direct causes of fear I want to mention the following: The Tapirapé were alarmed by my fury when I saw a man whipping his wife. To relate this case means to confess a most ethnocentric and therefore in no way ethnological behaviour. But I think that in order to be an ethnologist it is necessary to be able not to be an ethnologist in certain situations. At noon on July 15 I see that behind the house in which I was living with Kamairahó and thirty other Indians, the athletic Inamoréo, nearly twenty-two years old, is beating a woman. With a cane and the blade of his bush-knife he is whipping his wife. She is so small and tender that she seems to be a child in spite of being perhaps twenty years old. I run towards the couple to interfere. The little woman is weeping despairingly, but doesn't go away. The man tries to tug her towards the house which is their house and mine also. She struggles. There he enters the house. Finally she follows. I enter also. The man holds the knife. I have nothing in my hands, but I am bound by my upbringing; I am the weapon of my culture and my culture is my weapon. I say to Inamoréo that people do not beat a woman like a dog. (I refer to this animal because frequently, among the Tapirapé, it can only thus be prevented from eating the people's food.) Inamoréo pronounces insults and his wife sobs. Now I explain to all the people present that beating a woman is a bad thing. Later, when I am sitting together with Kamairahó and his wife, I tell them that if a white man should whip his wife, another white man would kill him with a revolver. Jokingly I add: "If you, Kamairahó, beat your wife, I have to kill you with my revolver." With these words I go away.

After a while Kamairahó passed me with a depressed face. I ask him: "Where are you going?" Without smiling he answers: "I am afraid of you." I embrace him, I kiss him impetuously in the Tapirapé manner, that is, rubbing my nose against his nose; but all this is in vain. He waits patiently until I let him go, continues walking, and tells the men and women whom he is passing that he is afraid of me, the white man who kills those who beat women. May I observe here that I always went around without a weapon among the Tapirapé, keeping the revolver, as Kamairahó knew, in my hammock hanging close to his hammock, and even there the revolver was at hand only to be at the disposal of the same Kamairahó in order to protect him against the event of nocturnal attack by the Kayapó Indians, the most terrible neighbours. Kamairahó enters his house and immediately from there sounds an agitated and long talk. With the speed of lightning the news spreads through the village that Kamairahó is afraid of me and is lying in his hammock and that I would kill with a revolver the husbands who whip their wives. A middle-aged woman joins me, saying: "I am afraid of you." I assure her again that I am not bad, that, on the contrary, I am good, and in order to demonstrate it, I embrace cordially a man on her side so that I don't need to embrace her. And I say to my English companion who at this moment joins me: "Certain research-workers who were killed by the Indians with whom they lived, as, for instance, Boggiani among the Chamakoko, were murdered only through fear." At that instant, as if he had understood my words, the man I have just embraced asks me if the Shavante really have killed the white man by the name of Father John and if they have murdered him with a club. I don't know from where the Tapirapé had received this information and if his question represented a reaction or only conversation. What I know is that the man couldn't have understood my comment about the murders through fear.

An hour later I see Kamairahó going to the brook carrying two calabash rattles which he had obtained from the Karajá neighbours, but never used. I follow him and ask him what he intends to do with the rattles. He explains that they are dirty and that he wants to clean them with water, and adds: "When you go away and I remain here, the rattles will go with you." He continues with a mournful face. Besides, everyone looks sad, and when I enter our hut, I am not received as usual with the friendly exclamation: "You arrive!" But, looking to this or that person, I immediately receive a polite smile. Even at night Kamairahó says to the Englishman that he is still afraid of me, and when my companion reassures him, confirming the jocose character of my threat, he is invited immediately by Kamairahó to eat bananas and by other inhabitants of the house to have kauí, a fermented drink, in payment for this consolation. Meanwhile Inamoréo, who had beaten his wife, has already gone peacefully to sleep with her in the hammock and explains later that he whipped her only with the arrow—which is not true. I invite Kamairahó for a dinner of deer. He eats well, but with a sad face complaining of a headache. He receives aspirin.

It is necessary to consider that the fear described above may have been caused less by the threat with the revolver than by the agitated manner with which I expressed my indignation and which contrasts with Tapirapé behaviour. Even Kamairahó's wife, who used to quarrel much, always pronounced insults in a monotonous voice and with apparent calm, quite different, for instance, from the Karajá women who shout without reserve. But it may be mentioned that the Tapirapé were also afraid of the Englishman when a woman had stolen a great quantity of kitchen oil from him, putting it in her hair, and when dogs of the village tore his towel to pieces. And my companion was less explosive than I!

Graver causes of fear are the Kayapó attacks. I have already mentioned that, on account of these, I was asked to sleep armed. When a rumour maker is talking about the approach of these Indians, all the village is immediately alarmed. And there are reasons for this, because in 1947, soon after my second visit to the Tapirapé, these warlike neighbours assaulted the village, plundered it, and burned it, killing and kidnapping women.

Fear was produced also by the eclipse of the moon on 15th of June, 1935. When at night I was awakened with the words "chahy vuy" "moon blood," all the people were dejected and the men were talking only in low tones. Really, the moon appears dark red like coagulated blood. Next day everybody was still preoccupied with the phenomenon, considering it a bad omen of carnage.

But while the eclipse was an uncommon cause of fear, there were other causes in every moment and in any place, for the Tapirapé lives surrounded by malevolent spirits. Here is the so-called "imaginary fear" regarding which Mira y López[9] observes that "even the bravest warriors, able to throw themselves in the open against a rampart of fire or lances, recede terrified in view of the suspicion of a tenuous and invisible enemy." In fact, the Tapirapé who opposes the jaguar with only bow and club as I had occasion to observe, and who is not afraid of dying, as they frequently assured me, is terrified by the presence of the souls of the dead and of other bad beings. It is not only the soul of a dead shaman which causes fear. I knew a kind father who became afraid of the soul of his beloved little child when this was deceased. And whoever is not a shaman feels a dread of dreaming while asleep, because during the dream he has to travel through the dangerous supernatural world. And not infrequently boys who wanted to drink water at night in the nearby brook asked me to go with them, because a ghost had been seen taking a bath. Unfortunately, whenever I arrived, the well-scrubbed ghost had already gone away. Frequently the boys sang loudly when going to the brook at night in order to calm their fear as I have observed in Europe when children went for something in a dark cellar.

Although the Tapirapé didn't react in any way against the fear produced by the eclipse, they were not always so passive as regards the fear caused by the white men, the Kayapó, and the spirits. When my English companion fell ill, they didn't hesitate in demanding his departure, and when a little girl of the house where I was living fell sick, all the people around me wore a threatening air as if I had been the magic author of the disease. The fear inspired by the Kayapó was combated not only by the Tapirapé holding clubs during the nocturnal collective dances, but also by their parodying their neighbours by a pantomime which represented a supposed attack against peaceful tribesmen. The fear of the spirits gives not only great prestige to the shaman, official protector against them, but also puts in the hand of every Tapirapé a torch which he agitates without interruption if by night he has to go out of the house in order to relieve himself.

So for the frightened Tapirapé also, life is struggle, action and reaction, fear and wrath. And this fearful people is the gayest I ever found in my life, and Kamairahó, the pessimist who more than anyone declared himself to be afraid, had a glad smile on his face everytime when I saw him awake in the morning.

São Paulo, Brazil.

Notes

1. Cf. Herbert Baldus: *Ensaios de Etnologia Brasileira.* S. Paulo, 1937, pp. 156–158.
2. Ashley Montagu: *The Natural Superiority of Women.* London, 1954, p. 92.
3. Emilio Mira y López: *Quatro gigantes da alma.* 3a. edição, Rio de Janeiro, 1955, pp. 39–40.
4. Cf. Herbert Baldus: Os Tapirapé. *Revista do Arquivo Municipal,* 110 : 192, 1946. S. Paulo.
5. Cf. Amilcar A. Botelho de Magalhães: *Pelos sertões do Brasil,* 2a. edição, S. Paulo, 1941, pp. 497–498; Martin Gusinde: *Die Feuerlandindianer, I: Die Selk'nam,* Mödling bei Wien, 1931, pp. 456–458.
6. Cf. Baldus: *Ensaios de Etnologia Brasileira,* p. 86.
7. Xamanismo tapirapé (Tapirapé Shamanism), *Boletim do Museu Nacional,* n.s., Antropologia, 3 : 79, 1943. Rio de Janeiro.
8. *Ibid.*
9. *Op. cit.,* p. 39.

YORUBA CONCEPTS OF THE SOUL

William Bascom

Like many other African peoples, the Yoruba believe in multiple souls. As this paper will bring out, the exact number, nature and functions of these souls indicate both regional and individual variations in a basic pattern of belief. In a realm where beliefs were not dogmatically formulated and where interpretations are not subject to empirical verification, the room for individual speculation is ample, yet a fairly consistent underlying pattern is apparent. Christian doctrines have been preached among the Yoruba for more than a century,[1] and Moslem beliefs much longer. Their influence is to be seen in some interpretations, even though diviners and cult members were most frequently consulted in an attempt to determine the traditional patterns of belief; but the belief in multiple souls is found even among practicing Mohammedans and those who have been to mission schools. The regions of Yoruba country compared here are represented by the town of Meko, near the Dahomey border and formerly under the kingdom of Ketu, Iganna, which is also near the border, but under the kingdom of Oyo, the capital city of Oyo, and the city of Ife.

The Yoruba generally distinguish at least[2] three separate souls. The first is the breath (ẹmi)[3] which resides in the lungs and chest. In the simile of an Ife informant, this, with the nostrils, is like the blacksmith's bellows with its two orifices. The breath is man's vital force; it gives him life and makes him work. The second is the shadow (ojiji),[4] which may also be spoken of as the "cast" (idanda), like the skin shed by a snake. The shadow has no function during life; it does nothing but follow the body about. The third and most important is the ancestral guardian soul (ẹlẹda, olori) which resides in the head and is associated with the individual's destiny and with the Yoruba belief in reincarnation.[5]

One can see the shadow, and hear and feel the breath, but no one sees, hears, or feels the ancestral guardian. The breath is sustained by the food which the individual himself eats; the shadow is without substance and requires no nourishment; but the ancestral guardian must occasionally be fed through sacrifices known as "feeding the head" (ibọ-(o)ri). At death, all three souls depart from the body (ara), and normally reach heaven eventually.

According to Meko informants, it is the breath that leaves the body during sleep, visiting distant places in dreams. When a person awakes he can tell about things that he has seen and done in other towns, whereas others know that his body has remained in the room with them. When the breath is far away, a person cannot wake up quickly, and it is considered dangerous to waken a baby suddenly before its breath can return; it will cry, and become sick; but it is not likely to die. Hence a mother first looks under the cloth with which she covers her child, and if she sees it is sleeping soundly, she does not disturb it. Ife informants agreed that it is the breath which departs from the body in dreams, but held that there is no danger of awakening a person when it is far away, since it returns immediately. An Iganna informant, on the other hand, maintained that it is the shadow which travels in dreams, arguing that one can see a sleeping person breathing, and that if the breath leaves a person or if the shadow does not return, he must die.

Most informants disclaim knowledge of how evil magic and "witches"[6] operate. In Meko they denied the existence of soul catching as a means of causing illness or death, but said that it is the breath which leaves the sleeping body of a witch to perpetrate evil. They credited witches with eating the bodies (ara) of their victims, but maintained that they do nothing to the breath or other souls. An Ife informant, who claimed and was credited with a dozen deaths by magic, said that there are people who make medicines to trap the breath and prevent its return to the body, causing their victim to die within four days. He also described a medicine which gave him the power to see the souls of living persons and to predict their death from the soul's behavior. He explained that the breath is recognizable, resembling the living person, and that if one sees the soul of a medicine man dragging away that of a person, tied and bound, he knows that person is going to be killed. He told how once he had visited a sick relative and had known that he was going to die because as he departed he saw the boy's soul following him.

The role of the breath in Ife and in Meko is ascribed to the shadow in Oyo and Iganna. This is also true in the case of the farewell visits paid immediately after death to relatives who are away from home. When a man dies in Meko, for example, he may appear to a relative in Abeokuta, perhaps on the very day of his death. After exchanging greetings, the relative may enquire what has brought him to Abeokuta, and the deceased explains that he has come to buy something and is about to return home again. He then asks, "Did you hear that anyone at home has died? Did you get a letter?", adding that he came from Ilaro (or some other town than home), and that someone on the lorry had said that there had been a death in their house, but had not known who it was. The relative is unaware that his visitor is dead, but soon afterwards, perhaps the next day, he receives word of the death from relatives at home. The dead appear in this way only to those who have not yet heard of their demise.

Belief in these farewell apparitions is widespread, although in Iganna and Oyo they are ascribed to the shadow rather than to the breath as in Meko. Meko informants argued that it is the breath, rather than the shadow, because the dead talks to those he visits. They point out, as other informants also state, that this is the last time that the dead speak to the living. Afterwards, Meko informants explained, the presence of a dead person may be felt, as when someone seems to be standing with you in a room at night and you cannot see him, but you feel a chill. This is the shadow, because it does not speak. And a dead person may be seen in a dream, standing silently at a distance, looking at you but saying nothing; if you speak to him, he does not reply. This is also the shadow they reason, rather than the breath, because it cannot speak. It is impossible to speak without breath, and a dead person cannot talk to anyone who already knows that he is dead. Therefore they conclude that after its farewell visits, the breath goes to heaven and does not reappear.

Each individual has a predetermined day by which he must return to heaven. This day is set for him when he is born by Ọlọrun, the God of the Sky or Heaven. It cannot be postponed by prayers, sacrifices, charms, magic, or any other means. There is no way to lengthen the span of life allotted by Ọlọrun, though it can be cut short by the other deities (orisha), by witches (ajẹ), and by charms or magic (ogun), and there are charms to insure that a person will not die until his time is up. Those who die naturally because they are old and have lived out their allotted time are spoken of as "one who has (his) day" (ọlọjọ), meaning that he has reached the last day given to him by Ọlọrun, but a child who dies when only a few years or even a few days old may have done so also. Those who live out their allotted span of life go directly to heaven. Those who are killed before their time is up are the ghosts that are seen wandering about. They remain on earth until their day finally arrives.

A man who had been born and raised in Iganna, but who was a practicing diviner in Oyo, explained that these ghosts go away to towns where they are not known and settle down as traders. They can talk to people who had not known them previously, and to them they seem like normal, living persons. The ghost of a woman may marry and have many children, and one may marry a ghost without knowing it. If someone whom the ghost knew before death comes to town, it disappears. If someone who has lived in Oyo for perhaps ten years is not known by anyone who visits Oyo from the town he says he came from, there is good reason to suspect that he is a ghost. It is the shadow that becomes the ghost, but since it can talk to strangers and pass as a living person, the breath remains with the shadow. When the day assigned by Ọlọrun arrives, the ghost "dies" a second death and goes to heaven.

The dead return home when their children offer food at their graves, or to fight with their former enemies or those who have harmed them, but they are not seen. If they appear to one with whom they are fighting and are recognized by him, he will die. Generally, however, ghosts stay far from home where they can be seen without danger of being recognized. In Abeokuta there is a market (Idoku) where ghosts are often seen at night. If they are approached by someone they know, they turn away, and the one who has seen them feels a chill that precedes fever. When they are greeted they do not reply, because they cannot speak to anyone they knew before death. Although they walk on the ground, their tracks cannot be seen; and if they are recognized, they disappear. Also, he has heard that ghosts have a special quarter in Ife, and that there is also a hole of death (ikoto iku) near Ife where many ghosts live, and for this reason many people fear to go to Ife to trade.

Another man from Iganna, who served as an interpreter and later became a Court Clerk, confessed that at one time he had been afraid to visit Ife, where according to Yoruba traditions the world was created and the deities once lived, because he had been told that it was inhabited by the dead. And a man from Ife itself told how, when he had been assigned to Ila as Court Clerk, he had to hire carriers in Ife to move his loads because the people of Ila feared to visit Ife for the same reason. Oyo informants spoke of the dead holding their own markets near Lagos in olden times.

The shadow was not mentioned in discussions of the soul in Ife, and in Meko it has no functions, even though it follows the body during life and departs at death. In Meko the breath is what makes the farewell visits, after which it goes immediately to heaven, while in Ife it becomes the ghost which may remain on earth. In Iganna and perhaps Oyọ, the shadow becomes the ghost which makes the farewell visits and may remain on earth, but the breath is believed to remain with it because the ghost can talk. Some Oyo informants at first disclaimed knowledge of what happened to the shadow, the breath, or the ancestral guardian, but later said that the shadow makes the farewell visits. Ife informants distinguished between ghosts and spirits (iwin) which have not formerly lived as human beings, though Iganna informants used the word *iwin* for both.

Without going fully into the concepts of the afterworld, it must be noted that there are two heavens located in the same part of the universe, which some, but not all, believe to be in or beyond the sky. Those who have been cruel or wicked (ika) or are guilty of murder, assault, theft, slander, the use of bad magic, or of harming their fellow men in other ways, are punished for their evil deeds on earth in the bad heaven (ọrun buburu, ọrun buruku), also known as the heaven of potsherds (ọrun apadi). Ife informants mentioned walking in the midday sun as one of the punishments, but they described the bad heaven as hot like pepper, not like fire. Farrow to the contrary, it is not dry, barren, lacking in water, or "heated with charcoal like a pottery-kiln."[7] The symbolism of the potsherd represents something which is broken beyond repair and thus utterly lost, for the souls who are sent here can never be restored to the living through reincarnation. Suicides, similarly, cannot be reborn; they can never go to heaven and, having renounced the earth, belong to neither. They become evil spirits (iwin buruku, iwin buburu, eburu) and cling to the treetops like bats or butterflies.

The good heaven (ọrun rere) is also referred to as the heaven of contentment (ọrun alafia) or the heaven of breezes (ọrun afẹfẹ). Here the air is fresh and everything is good, the wrongs of earth are righted, and losses are restored; but life is much as it is on earth. Those who have done good on earth remain here until they are reborn. The living pray that they will go to heaven and return through reincarnation, not because heaven is a bad place, but because they wish to return to earth in another generation, so that they may be with their children.

The ancestor reborn in a child may be identified through physical resemblance, similarity in character or behavior, through dreams in which the ancestor tells someone in the family that he has returned, or through divination for the mother during pregnancy or for the newborn child. The personal names "Father Returns" (Babatunde) and "Mother Returns" (Yetunde), may be given to children of the same sex as the reincarnated ancestor, but an ancestor may be reborn in a child of a different sex. According to Meko informants, resemblances of children to their living parents is not related to reincarnation, but according to Oyo informants, a father and his son or daughter may be reincarnations of the same ancestor, explaining striking similarities between them. Thus the soul of the ancestor may be shared by or between more than one individual.

An individual is nearly always reborn into his own clan, so that the guardian soul is that of a patrilineal ancestor. However, an informant from Ondo held that if a mother loves her son very much, she may be reincarnated as his descendant, in which case the soul leaves the clan of her father. Ife informants also held that a child could sometimes be the reincarnation of an ancestor of the mother and that the soul reverts to its own clan when the child dies. However, an Iganna diviner held that the soul is not always reborn into the same clan, but can go anywhere it likes, citing as evidence statements of those who say that when they die they are going to come back as the son of a king or to a different town, or even to Europe.

According to this Iganna diviner, the breath, the shadow and ancestral guardian soul are all reborn in a new body after which his ghost is never seen again. However, Meko informants maintained that although all three are reunited in heaven, if all were reborn there would be nothing in heaven to protect the living except the deities. Moreover, sacrifices may be required for a grandfather even after he has been reborn as a child, and he may reappear in a dream which signifies that he is reincarnated in a child who has recently been born. Therefore the breath (and the shadow) remain in heaven as the ancestral spirit, while the ancestral guardian soul is given a new body, a new breath, and a new destiny by Ọlọrun when he is reborn. Thus each living person has the guardian soul of an ancestor who may already have been reincarnated in his other ancestors, and may be reincarnated many more times in future generations as his own descendants.

The ancestral guardian soul, having no material manifestations, is a concept which presents both the greatest opportunities for individual speculation and the greatest difficulties of interpretation. Unlike shadow and breath, the ancestral guardian soul is not a translation of the Yoruba terms which refer to it. Of these, the two most commonly used derive from the verb meaning to create (dá) or form or cause. One (ẹda) is literally "a creation" or "a creature," while the other (ẹlẹda) may be translated either as the "owner of the creature," as in the "owner of the house" (onile), or as "creator" as in "drummer" (onilu). The semantic difficulties involved go far deeper than the problems of translation, for some informants object that *ẹda* or "creature" refers to a company that has been formed, or to a chicken or to man himself, but not to any spiritual entity associated with man; while some informants identify the "creator" with Ọlọrun, the Sky God, maintaining that for all men and animals there is but a single "creator" (*ẹlẹda*).[8] The latter may account for the doubts, contradictory statements, and differences in opinions as to whether the ancestral guardian is on earth, or in heaven, or both.

To illustrate the confusion which results, the following contradictory statements of an informant, whose other remarks indicated no desire to obfuscate, may be cited in sequence: "The soul or breath (ẹmi) and the creature (ẹda) are the same. The creator (ẹlẹda) is the guardian of the soul. The creature (ẹda) lives in man; the creator (ẹlẹda) lives with God, but is not Ọlọrun. It is the same as Ọlọrun, who is the Creator (ẹlẹda), the God who made man. There are two creators (ẹlẹda), one in the head and one in heaven. The one in the head is the same as the one in heaven. Each person has his individual creator (ẹlẹda) in heaven. When one speaks of each person having his own individual creator (ẹlẹda), this refers to that of the individual on earth, i.e. to the individual himself. But there is only one creator (ẹlẹda) in heaven for everyone, i.e. Ọlọrun."

The ancestral guardian soul is also referred to as the head (ori), where it resides, and as "the owner of the head" (olori), though some informants object again that the latter refers only to living individuals such as chiefs or presidents who are "heads" of social groups, and that it has no spiritual connotations. Similar speculations arise as to whether the breath (ẹmi) is identical with the "owner of the breath" or the "breather" (ẹlẹmi), a term which is easily derived but not commonly used in this context. There seems to be good reason to believe that Yoruba morphological rules for the formation of nouns from verbs have contributed to the speculations and varied interpretation about the spiritual counterparts of man.

Finally, the ancestral guardian soul is known by a term (ipọnri, ipọri, ipọnrin), the literal meaning of which could not be determined. It refers also to the thumbs and big toes (both known as ataparako, ataparunko, atanpa), to the ancestor who is worshipped when the big toe is fed, to the ancestor whose praise names are given to an individual in Ife, to the ancestors in general, and in Iganna and Oyo to the object in a shrine which symbolizes the deity and through which the deity is fed.

The ancestral guardian soul is located by some informants in the top of the head or crown (atari, awùjẹ̀). Here, an Ife informant explained, one can see the beating pulse of a newborn child, and from the crown the breath departs from the body at death. It is located by other informants in the forehead (iwaju, ori), to which the word head (ori) may specifically refer. The forehead is associated with the individual's luck, which is a part of the destiny (iwa) assigned to him by Ọlọrun when he is born. The ancestral guardian is also associated with the back of the head or occiput (ipakọ, õrun), which faces backwards and guards the rear or the past. It protects him against evil in places he has left behind, and if he is told of something bad that happened in a place where he has been in the past, he thanks his occiput for having heard about it.

As the occiput warns against what lies behind in the past, so the big toes tell of good and evil that lie ahead. Oyo informants explained that, regardless of the sex of the living individual, if the ancestral guardian soul is male, stubbing the left toe is an omen of good fortune, while it is the right toe if the ancestral guardian is female. All other informants, however, explained that each individual determines for himself which toe portends good and which one evil. If a man stubs his right toe on his way to Lagos and when he arrives finds all that he came to buy available at reasonable prices, while on another trip he is robbed after stubbing his left toe, he knows how to interpret this important omen in the future. The only parts of the body which can "talk" are the head and the toes, the toes speaking through omens.

One Ife diviner maintained that the ancestral guardian soul (ẹlẹda) in the crown, the head (ori) in the forehead, and the occiput (õrun, ipakọ) in the back of the head are three distinct souls. They remain in the head until death, when all three go to heaven, where the ancestral guardian soul gives the account of all the good and evil that the person did on earth. As in a trial in court on earth, a man who has been good is released and can be reborn, while one who has been bad is held and punished. It is the ancestral guardian also who is a member of a person's "council in heaven," and who takes sacrifices made to the head to heaven. The only way to sacrifice to the ancestral guardian is to sacrifice to the head, and anything that is given to the forehead or the occiput goes to the ancestral guardian soul, but is shared with the other two. The most respected diviner in Ife apparently agreed that the forehead (ori) and the occiput (ipakọ) are distinct from the ancestral guardian (ẹlẹda), and that sacrifices to them go to the ancestral guardian and to the breath as well.

Most informants, however, believed that all three parts of the head are controlled by a single soul, that of the ancestral guardian. Another Ife diviner said that the forehead and the occiput are like elder and younger brothers, both junior to the ancestral guardian soul in the crown. However, each individual has two ancestral guardians, one in his head, and the other in heaven. The one in heaven is his creator, but it is not Ọlọrun. It is his individual spiritual counterpart which is doing exactly the same things in heaven that the individual himself is doing on earth, although it is always in adult form even when the living individual is still a child. If one has the full support and protection of his ancestral guardian in heaven, he will live out the full span of life allotted to him by Ọlọrun; but if he does not, he will die before his time. If a child dies at an early age or is born dead, its breath and the ancestral guardian in the head report to the one in heaven, which may send them back to earth immediately to be reborn again; this time the child will live to a ripe old age. Some individuals know charms or medicines to harm or kill a person by "poisoning" his ancestral guardian soul, but as long as it remains well he himself cannot become sick.

Similar problems arise concerning the big toes and the thumbs. Farrow lists *ipori*, referring to the big toe, as the third soul. An Iganna informant initially said that the big toe is a separate soul, and that there are two ancestral guardians, one in the individual's head giving him luck, while the other follows behind him, watching over him and warning him of impending evil. Later, however, he revised his opinion, maintaining that there is only one ancestral guardian in the head, including the forehead, crown and occiput, and that this is what goes down into the big toes. At death, the breath, the shadow, and the ancestral guardian depart, leaving only the body. Similarly Oyo informants spoke of the ancestral guardian as following one like a shadow, though it lives inside the head, and at first described the occiput as its assistant. They insisted there is no separate spirit in the big toes, only the ancestral guardian which controls both the head and the toes. Later, they made it clear that the terms ẹda, ẹlẹda, ori, olori, ipakọ and ipọri all refer to the ancestral guardian, and that man has only one breath, one shadow, and one ancestral guardian. Meko informants confirmed that there is only the ancestral guardian in the toes, thumbs, forehead, crown and occiput, though some said it did not live in the occiput. There is nothing which leaves the toes, thumbs, forehead or back of the head at death, when only the ancestral guardian, the breath, and the shadow depart, all three meeting in the same heaven where they are eventually reunited.

Farrow names as the second soul *ipin-ijeun*, "the sharer of the food," citing a proverb which says "There is no orisha like the stomach. It receives food every day." However, Meko informants said that this was just a joke, and Oyo informants described the proverb as making fun of the orishas, saying that *ipin-ijẹun* is just a praise name for the stomach. The diviner from Iganna identified *ipin-ijẹun* as the small intestine, not a soul but a part of the body found in chickens as well as man. Although he referred to the "head of the stomach" (orinu, ori inu) or "ancestral guardian of the stomach" (ẹlẹda inu) as one who can spoil your luck by pushing you into a fight, he regarded this as but another aspect or manifestation of the ancestral guardian soul.

Thumbs were mentioned as having significance only in Meko, where they are referred to as the *ipọnri* of the hand (ipọri ọwọ) to distinguish them from the big toes, the *ipọnri* of the foot (ipọri ẹsẹ). It is believed that the ancestral guardian lives in the thumbs and big toes as well as the head, and they are examined if a man is seriously ill. When he is going to die they are thin, with little flesh, showing that "his blood has begun to dry," and if they are cut they do not bleed. No other functions of the thumbs were mentioned, but in "feeding the head" some of the food is touched to each of the big toes, the thumbs, and the forehead for the ancestral guardian before it is eaten. If a pigeon is killed, a spot of blood is put on each of these with the finger.

In Meko and in Iganna, the head is fed only when one is told to do so in dreams or by a diviner, who indicates what foods must be offered. Some individuals never do so, while others may feed their head only once or twice during their entire lifetime. Or a diviner may specify that the big toe be fed, in which case the spot of blood is put on the good toe, also for the ancestral guardian in the head.

In Oyo, each married adult who is not a Moslem or Christian convert has a special pyramidal-shaped object made of appliqued cloth and leather which is known as "feeding the head" (ibọri, ibọ ori). Each year, at the annual festival for his deity (orisha), he divines with coconut and kola to find whether his head requires food. If it does not, he chews the coconut and kola with guinea pepper and blows some onto the bottom of this object. If a live animal is required, its blood is touched to the big toe which indicates good and to the bottom of the *ibǫri*, but never to the head. Only the pieces of coconut and kola are touched to the forehead before divining, while he prays "Head, please, oh. Don't let anything happen to my children."

In Ife, the individual feeds his head annually at a ceremony which he speaks of as "my own festival" (ǫdun mi). He touches the kola nut with which he divines to his forehead, the plate of yam loaf and soup which is prepared for him, and if an animal is killed, a spot of its blood. He remains in his room dressed in fine clothes and seated on a mat, and is visited by friends and relatives whom he entertains with food. "One becomes an elder on the day of his festival," as the proverb says, because everyone who visits him must prostrate before him. Close relatives who know his ancestral praise names recite them, praying that he will have money, children, and a long and prosperous life.

Each individual holds his personal festival on the same date, and bears the same ancestral praise names as the ancestor reincarnated in him. Sacrifices at the grave of the deceased are also offered by his children on the date that he held his personal festival. On this day the spirits of those who lived the full span of their lives, reaching a ripe old age, may return to earth and sit with the living, but they cannot be seen unless the face has been washed with a special medicine. The ancestral praise names (ekiki ipǫnri, oriki ipǫnri), though taken from the ancestor reincarnated in an individual, go back to a far more remote ancestor (ipǫnri) who first held them. Within the clan there are only a limited number of ancestral praise names which are shared by many clan members, all of whom hold their personal festivals on the same day. It is maintained that this ancestor (ipǫnri) and the ancestral guardian soul (ẹlẹda) and the head (ori) are all the same.

Thus it seems that while the soul of a recent ancestor is reincarnated in an individual, it itself is a reincarnation of a part of the soul of a more remote ancestor which is shared with a number of other living clan members. All clan members bearing the same ancestral praise names are descended spiritually by a series of reincarnations from the remote ancestor (ipǫnri) who first held these names, as well as patrilineally from the founder of the clan. Although related to the clan, these spiritual descent groups differ from any of the known kinship groups, since it seems that the ancestral praise names of an individual may differ from those of his siblings or of either parent. Although further details were obtained, the full significance of this could not be investigated because the ancestor whose praise name one bears is the most sacred and most secret aspect of Ife culture. Inquiries about this ancestor are taken as a most serious personal affront, because knowledge of his "history" (itan ipǫnri) or praise names gives one the power to kill a person by summoning his ancestral guardian soul, and some informants hold that one will die if he even talks about his *ipǫnri*.

The importance of the ancestral guardian soul was repeatedly expressed in a variety of ways. "The *ipǫnri* is worshipped by everyone, by kings and by the poor alike." "The head is the individual's principal deity (orisha). It is not like Shango or Ǫya, because it did not live on earth and turn into stones or rivers or iron; but these deities themselves had heads, and one could not worship them if he did not have his head. The head is more important to everyone than their own deity. The head is the eldest and most powerful of all deities." "Of all the deities, the head is the senior and most important. It is greater than the deities that turned into stone. They cannot talk and no one ever sees them eating. If one sacrifices to them or to the Egungun, the food is put on the ground. It is only the sacrifices to the head that you eat yourself."

Despite the individual and regional differences in interpretation documented above, an underlying pattern is apparent. Specific functions are associated with certain parts of the body, and may be personified as separate souls. The forehead controls luck, the crown guards against evil, the occiput guards the rear and the past, while the big toes warn of good or evil which lies ahead. The breath is the vital force, giving the power of speech and making one work. The shadow follows the body but has no function during life. The stomach controls the personality or character, and the head controls intelligence, although this and the related ritual of washing or cooling the head can only be mentioned here. The pattern of personification of these functions easily leads to elaborations on the concept of multiple souls, with further proliferations suggested by the patterns of formation of agentive nouns, but to some degree held in check by the recognition that a single phenomenon may have several different aspects.

At birth each person receives his destiny, which encompasses his luck, his occupation, and his personality or character, and at the same time he is assigned a predetermined span of life on earth. If he does not live until his time is up, his ghost completes the balance of allotted time on earth in another town. Ghosts are associated with markets, not with cemeteries which formerly did not exist, since adults were buried inside the house. Immediately after death, ghosts pay farewell visits to relatives who are away from home, but they cannot speak to anyone who already knows that they are dead. Except in the case of suicides, the several souls are reunited in heaven, where evil deeds are punished. Only those who have lived good lives on earth can be reincarnated, after being briefly reunited with their ancestors. When a soul is to be reborn, usually as the child of its own descendant, it is assigned a new destiny and a new span of life on earth and given a new body, perhaps of a different sex. All souls are not reborn, but every living person is a reincarnation, in most cases of an ancestor in the patrilineal clan. The clan exists in heaven as well as on earth, and individuals reside alternately in the two realms. Children who are born to die (abiku) travel back and forth even more frequently, remaining on earth only a few years or perhaps a few days. These beliefs present a number of logical problems[9] which some individuals attempt to reconcile, giving rise to other speculations and interpretations.

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Notes

1. Christian influences have been disseminated far more widely through the schools, nearly all of which have been mission-controlled, than through the churches. An equally important fact, which is usually overlooked or neglected, is that the only Yoruba-English and English-Yoruba dictionaries that have been available for the past century have been prepared by and for missionaries. These have been based upon the work of Samuel Crowther, a Yoruba who was captured as a slave, and set free by the British in Sierra Leone, where he was converted to Christianity. Crowther became one of the early missionaries to Nigeria, and later the first African Bishop of the Anglican Church. Crowther's "Vocabulary of the Yoruba language," published in England in 1843 and 1852, and an English-Yoruba dictionary published in 1911 by the Rev. E. J. Sowande, another Yoruba convert, were incorporated into the "Dictionary of the Yoruba

Language," published by the Church Missionary Society in a 1913 and 1937 edition, and reprinted photographically in 1950 by the Oxford University Press.

2. In Ife, however, the only reference to the shadow recorded was that at death the breath and ancestral guardian unite in heaven "like a shadow."

3. ẹmi, or emi (Ife).

4. ojiji, or ẹjiji (Iganna), ojimijimi, ojimjim (Meko), oji (Farrow, note 5, below).

5. Of these, Parrinder mentions only *ẹmi*, which he translates as soul (p. 122). Farrow gives three souls: *olori*, an equivalent of the ancestral guardian soul, *ipin-ijeun*, and *ipori*; he also mentions as equivalents of "the personal soul" *iwin* (the spirit), *okan* (the heart), and *ojiji* or *oji* (the shadow, shade or ghost) (pp. 130–132).

English-speaking informants in Ife and Meko equate the breath with the European concept of the soul, and suggest as a translation for the ancestral guardian, "angel" or "God's workers" (onṣẹ Ọlọrun), a Dictionary translation of "angel." The latter, offered by a Moslem who had learned English on his own, outside both mission and government schools, suggests the far-reaching influence of the Dictionary.

6. This is not an especially felicitous translation of the Yoruba term (ajẹ), but it has been widely used in the literature and is accepted by the English-speaking Yoruba. The Yoruba concepts associated with this term also vary individually and regionally, showing differing degrees of difference from and resemblance to the European conceptions of both witch and vampire.

7. Farrow, p. 133. The belief in punishment after death is said to be traditional, and an Ife diviner stated that those who worship the Yoruba deities (orishas) believe in it more than the Christians, and therefore do not do evil. Other informants pointed out that they know that there are rewards and punishments in the afterworld from the behavior of those who are dying. A cruel person cries out that someone is hurting him, or putting a rope around his neck, but a kind person may say that someone is bringing him food or drink. The Yoruba concepts of the two heavens differ from Christian concepts of heaven and hell, but the beliefs of some individuals have been influenced through missions or through the Dictionary. While Crowther originally translated *ọrun-apadi* as "(Lit. the invisible world of potsherds), place of punishment, hell," the Dictionary translates it simply as "hell." There is also a possibility that the distinction between sky (ọrun), the top of the sky (oke orun), and the face of the sky (oju ọrun) were introduced in an attempt to find a suitable equivalent for heaven.

8. This interpretation has been supported for a century by the translations of *ẹlẹda* in Crowther's Vocabulary (Creator, Maker, Supreme Being) and in the Yoruba Dictionary (Creator, the Supreme Being).

9. One which was not satisfactorily explained is that a newborn child may be assigned an evil character as part of its destiny, which would mean that it is doomed to the bad heaven from which reincarnation is impossible.

A NEW ECOLOGICAL TYPOLOGY OF THE CALIFORNIA INDIANS[1]

Ralph L. Beals and Joseph A. Hester, Jr.

Anthropologists have long been aware of the importance of ecology, particularly among peoples of relatively simple technology, but only rarely has it been possible to demonstrate ecological relationships satisfactorily in much detail. Birdsell (1953) recently has shown a very high correlation between population distributions and rainfall in Australia. Steward in several papers has suggested relations between ecology and social organization (for example, Steward, 1936, 1937, 1955). The problem has long been of interest to workers in California, particularly in various publications of C. Hart Merriam and in the work of A. L. Kroeber. The most ambitious undertaking in this field undoubtedly is Kroeber's *Cultural and Natural Areas of Native North America* (1939).

A major difficulty in most anthropological attempts to deal with ecology has been the unsatisfactory state of data concerning the environment. Few anthropologists have either the time or the skills necessary to obtain sufficiently detailed raw data where these are not already available. In California the wide variation in elevations, soil types, climatic factors, and flora and fauna has precluded the possibility of simple ecological correlations such as Birdsell secured in Australia, and has made any broad-scale analysis difficult. In recent years marked improvement in the data for California on climates, soil types and floral and faunal distributions permit an attempt at a more fine-grained analysis than that provided by Kroeber.

Re-examination of the data for California has led us to the provisional formulation of six major ecological types and several sub-types. This typology is based upon the fact that separate regions in California differ markedly from one another in the amount and kinds of food resources they offered the California Indians and upon the similarity of economic adaptations of the various groups within each region. As is the case with many typologies, our types are abstractions and dividing lines are not clear cut. In some cases food resources

SUMMARY TABLE OF ECOLOGICAL TYPES

Ecologic type	*Subsistence staples* (In order of decreasing importance; all use seeds, bulbs and greens in varying amounts as supplements to the staples.)
I. Coastal	
(a) Tidelands Gatherers	Shellfish, surf fish, acorns, game.
(b) Sea Hunter and Fishers	Sea fish, shellfish, game, acorns.
II. Riverine (salmon cultures)	Fish, acorns/tule, game.
III. Lake	Fish, tule/acorns, waterfowl, game.
IV. Valley or Plains (a mixed type)	Acorns/tule, game, fish.
V. Foothill	Acorns, game, fish.
VI. Desert	
(a) Hunters and Gatherers	Pinyon/mesquite, game.
(b) Farmers	Farm produce, mesquite, fish.

differed not so much in kind as in amount. In such cases the differences between types depend upon the relative importance of various food resources. Particular local groups of California Indians in some cases[2] had easy access to more than one type of environment and hence sometimes are not readily assignable to a single ecologic type. The typology is shown on p. 415.

ECOLOGICAL TYPE IA. COASTAL TIDELANDS GATHERERS

Tidelands Gatherers inhabited the coast from about Estero Bay to the Oregon border. Their economy was characterized by extensive use of shellfish and surf fish, with acorns and game playing a secondary role in the diet. They possessed only balsas (raft-like boats of tules) or relatively small dugout canoes, both specialized for inland waters: hence they seldom ventured off shore. Usually, or perhaps always, they were closely related to neighboring inland groups. In the south this sub-type tended to intergrade with the Sea Hunters and Fishers. Where rivers are sizable, they blended into the Riverine Ecologic Type. And insofar as they used acorns and other land foods, they showed some aspects of the Foothill Type, differing markedly, however, in emphasis on food supply. In general, the degree of variation within the sub-type depends on the size and usefulness of rivers or on the accessibility of inland foods.

The Tidelands Gatherers were closely limited to the cool, humid coastal fog climate with some possible exceptions among the long-vanished peoples about San Francisco Bay. This climate exists as a narrow coastal belt from Estero Bay to the Oregon border, an airline distance of more than 500 miles. The belt is often as little as five miles wide and its greatest inland penetration is some 21 or 22 miles along the Eel River from the sea. The total area occupied by Tidelands Gatherers may have been around 3,000 square miles, but by far the most important dimension for this group was not area but the number of miles of productive seacoast, estuary shores and river banks available.

Some inland areas undoubtedly fall within the range of most Tidelands Gatherers but most of the coast is rugged with intervening bays and beaches. In places, for many miles the coast-range mountains drop abruptly to the sea, or rise steeply behind a narrow coastal shelf. Hence, most of the accessible inland range is relatively poor in resources. North of Marin County, areas adjacent to the coast are mainly dense redwood forest, sometimes mixed with other conifers, or poor grasslands. To the south, redwoods occur in groves of diminishing extent almost to Estero Bay, but the country in places is more open. Nevertheless, oaks are rare near the coast, the unproductive laurel being a much more common tree, and dense chaparral covered extensive areas, impeding travel but supporting sizable deer herds. The interior demand for sea shells likewise permitted many of the Tidelands Gatherers to secure inland products through trade.

Coastal dwelling groups of Indians speaking the following languages were Tidelands Gatherers:

Tolowa
Yurok
Mattole
Wiyot
Sinkyone
Yuke
Pomo
Miwok
Costanoan
Esselen
Salinan

Coastal dwelling groups of Diegueño, Luiseño, Juaneño and Gabrieliño in southern California probably belonged to a similar type, but they vanished too early to permit definite assignment. Moreover, the greater proportion of sand beaches in the south not only supplied few shellfish but were poor surf fishing areas with aboriginal technology.

ECOLOGIC TYPE IB. SEA HUNTERS AND FISHERS

Sea Hunters and Fishers were found from about Estero Bay southward to just beyond Santa Monica Bay and on the Channel Islands. They took large sea fish, sea mammals (on land), and exploited the shellfish along the coast and on the islands. Although they used the same basic economic adaptation as the Tidelands Gatherers, the technological addition of the ocean-going canoe and deep-sea fishing techniques tapped a rich source of food unavailable to the latter.

It is probably no coincidence that the distribution of the seaworthy plank canoe begins at the southern terminus of the coastal fog belt. Southeast of Point Conception the Santa Barbara Coast is relatively sheltered from strong westerly winds. This, plus greater visibility and the proximity of the Channel Islands, facilitated the most successful marine adaptation found in California. Peoples of this ecologic sub-type were the coastal and island-dwelling groups speaking various languages of the Chumash and Gabrielino families. Linguistically related groups living inland belonged to a different ecologic type.

Most groups belonging to this ecologic sub-type were found along approximately 70 to 100 miles of sheltered shoreline (70 miles from Point Conception to Ventura, perhaps 20 miles of Santa Monica Bay, and an additional 10 miles at San Pedro Bay), and on the Channel Islands, plus a few at canyon mouths along the abrupt coast between Point Mugu and Point Dume. They undoubtedly ranged inland a moderate distance but, except for a few valleys, the coast range either rises abruptly from the sea or from a narrow coastal plain only two or three miles wide to crests averaging 2500 to 3500 feet elevation. Except for narrow strips of poor grassland, coastal chaparral and coastal sagebrush constituted the dominant vegetation near the coast. None are highly productive of vegetable foods. Inland and at higher elevations some oaks, and in places pine, existed, but game (including sea mammals along the shore) appears to have been the most important food derived from the land. Acorns, although the major vegetable food, apparently were of much less importance than animal foods. Because of early disruption of these peoples by the missions, it is impossible to know how closely their economies were integrated with those of the related inland groups belonging to a different ecologic type. It is unlikely that they often ranged inland beyond the crest of the coastal ranges, and then only for specific resources.

ECOLOGIC TYPE II. RIVERINE

Riverine groups occurred chiefly along the main streams and certain tributaries of the Klamath, Eel, and Sacramento Rivers. Groups of this type based a major part of their subsistence upon the annual runs of the large king salmon. (Species of smaller salmon entered some of the short coastal rivers but only the large king salmon was sufficiently important to have modified Indian economy to a significant extent along California streams.) Despite occasional failure of the annual salmon run, groups of Riverine type possessed marked economic stability. Further characteristics were compact settlement patterns (including large villages in the Sacramento Valley region), incomplete exploitation of vegetable resources, and some degree of social and economic dominance over neighboring hunting and gathering groups.

Groups of Riverine type usually had their settlements on or near rivers and their major subsistence came from the streams and the immediately nearby valley bottoms and hills, with occasional visits to more remote spots. Except for the groups in the Sacramento River drainage, all other groups lived in rugged and heavily forested Coast Range areas. The forests there are predominantly coniferous, mostly redwood, Douglas fir or mixed types, providing few food resources except along the streams and in and near unforested areas. Although the most numerous and preferred oak in northwest California, the tan-bark oak (*Lithocarpus*), flourishes best in partial shade, it does not tolerate the heavy shade within dense mature coniferous forests. The same is true of most plants affording browse for deer or food for other animals, including birds.

In California there are about 650 miles of rivers and major streams in which king salmon are found, or were found before irrigation and the damming of streams began. Where these supported sufficiently large runs of salmon and advantageous fishing conditions, salmon provided a substantial portion of the staple diet. Acorns were the most important vegetable food while game was a poor third as a food source. It is important to note that salmon apparently were not usually taken in the ocean, in San Francisco Bay or in the slack waters of the system of sloughs in the interior valley. In the upper reaches of many streams, salmon runs apparently diminished in size and were of minor importance to many peoples with good acorn and game resources who consequently fell into a different ecologic type.

People in whose habitat important salmon streams occurred included groups speaking the following languages:

Hupa
Tolowa
Yurok
Chimariko
Wiyot
Bear River
Karok
Shasta
Achomawi
Okwanuchu
Pomo
Yana
Miwok
Yokuts
Wintun (River Wintun)
Patwin (River Patwin)
Maidu (River Maidu)
Nisenan (River Nisenan)

Riverine groups of the last six named languages will be considered with the Valley Ecological Type. Other groups, basically Coastal or Foothill in ecological type but with a partly riverine economy, included some speakers of each of the following languages:

Wilkut
Chilula
Mattole
Lassik
Wailaki
Yuki
Huchnom
Sinkyone

ECOLOGIC TYPE III. LAKE

Peoples of the Lake type lived on islands or lake shores and gained the bulk of their subsistence from the water and its immediate vicinity. The major locations were Tulare, Kern, and Buena Vista lakes in the San Joaquin Valley and Clear Lake north of San Francisco Bay. Other smaller lakes and tideland marshes may have supported groups of similar ecologic type but none are reported in the literature.

Lake-type groups exploited primarily fish, waterfowl, and other lakes foods. The San Joaquin Valley Lake-type people also made extensive use of the bulrush or tule for food, clothing, and housing and had easy access only to relatively barren plains in a Desert-type environment. The Clear Lake groups, on the other hand, had Foothill-type environment readily accessible to them and utilized land products to a greater extent.

People belonging to this type included groups speaking languages of the Pomo and Yokuts families.

Owing to marked differences in the area of the shallow lakes in the San Joaquin Valley from year to year, it is difficult to estimate the area of primary subsistence. The secondary range included the few fertile areas with water supplies in a large area of the southern part of the west side of the San Joaquin Valley. The lake-dwelling Pomo, on the other hand, often had small but well-defined mainland territories in which they hunted and gathered.

ECOLOGIC TYPE IV. VALLEY

Valley-type ecologies are found among people in the Central Valley who occupied neither the Lake nor *Tular* area of the southern San Joaquin nor areas of woodland-grass about the edges of the Valley.

Settlements tended to be larger and more permanent than in many parts of California, and sometimes, in areas subjected to flooding, were on artificially raised elevations. Most groups lived along the edges of permanent streams or water courses and gained much of their livelihood from fish (salmon, sturgeon, and lesser fish) and gathered fresh or brackish water shellfish. For groups in the central region, tule was of considerable importance. Most groups also had access to vegetable products and game on the grasslands, and many had access to some oak groves, especially in the Sacramento Valley. The economy thus was of mixed type and groups along the Sacramento River north of the delta were essentially Riverine in their economy, while those with access to sizable oak groves approached the Foothill type of ecologic adjustment.

The western part of the Central Valley was a region of relatively low rainfall and scanty resources. This is especially true of the southern part, and the account of the earliest Spanish expedition (Fages) along the west side of the San Joaquin Valley from Suisun Bay to about Pacheco Pass speaks of it as barren and virtually devoid of human occupation outside the tule swamps. The east side supported more vegetation but extensive areas between rivers are described as containing only the California poppy (*Eschscholtzia*) and members of the geranium family. Neither was a significant food source.

Except for the important salmon streams (the Sacramento above the American River junction and the clear-water sections of the San Joaquin and its larger tributaries) and the occasional groves of valley oaks (most common in the eastern part), the most important subsistence areas were the hundreds of miles of stream, slough and tule-swamp borders. Especially in the so-called delta region and the lower San Joaquin, the basic economy for many groups possibly differed little from the southern San Joaquin Valley version of the Lake ecologic type. If this was true, then our Valley type disappears, divided between Riverine and Lake types (the latter perhaps better identified as tule-swamp type) with a few marginal intermediate groups who either had nearby oak groves on the Valley floor (as in the Lodi-Stockton area) or periodically crossed the twenty or thirty miles of relatively barren plains to reach foothill oak groves. Unfortunately the very early disappearance of most groups in this area and the unusually meager descriptions of the early Spanish visitors make a choice of these alternatives difficult if not impossible.

Groups belonging to the Valley type included speakers of the following languages:

River Wintun
Valley Patwin
Valley Maidu
Valley Nisenan
Valley Miwok
Valley Yokuts

ECOLOGIC TYPE V. FOOTHILL

Peoples of Foothill type lived in foothill or mountain regions with a variety of vegetable resources, of which the acorn usually was the most important. They are the "classic" type of the California Indian. They may have been the most numerous, and because of their generally mountainous and more interior habitat, more groups survived to yield at least memories to the ethnographer. Their primary subsistence came from those parts of the upper Sonoran and lower transition life-zones occupied by a combination of woodland, woodland-grass, grassland, or chaparral vegetation types. Fish and/or game quantitatively were a minor part of the diet. The primary areas of occupation were the western slopes of the Sierra Nevada below the limit of winter snows, the Transverse ranges, a substantial part of the Coast ranges, including some of the intermontane valleys, and coastal plains and valleys back from the sea in southern California.

Depending upon the terrain, Foothill type people lived either along streams in valleys or, especially in parts of the Sierra Nevada, upon the ridges between streams. Settlements in most places tended to be relatively small, with a restricted "home range," often extending over two or more life zones and providing the bulk of the food supply, and a somewhat wider hunting and occasional gathering range. Within the life zones occupied, because of local soil and climatic conditions, as well as the preferred adaptations of specific plants, most plant foods occurred in scattered locations rather than being distributed equally throughout the territory. As this was true of forage for many kinds of game, animal life likewise was unevenly distributed. At higher elevations groups of Foothill type bordered on the heavily forested Sierra upper Transition Zone, an area of deep winter snows and relatively poor resources.

In general, the peoples of Foothill type occupied the most favorable areas for Indian life in California (except the better river and coastal localities) and showed the highest general population density. With few or no exceptions, peoples of Foothill ecologic type occupied relatively permanent settlements most of the year, but with either brief trips away or seasonal movements of varying magnitude and duration. Known settlements tended to be concentrated in areas of woodland and woodland-grass vegetation cover in which occur the great majority of oaks in California. The density of population within these zones varied primarily with rainfall and soil conditions, while the extent and duration of movement from permanent settlements was related both to richness of resources near the home village and the accessibility of alternative environments. Thus some groups visited the sea coast and some the desert, while yet others penetrated even into the boreal zones of the high Sierras in summer, although this often was a "food deficit" area requiring carrying in supplies. For many, however, movements took place within fairly short distances following the seasonal availability of supplementary foods occurring in scattered and restricted areas.

Groups of Foothill type included speakers of the following languages:

Foothill Miwok
Foothill Yokuts
Tubatulabal
Western Mono
Kitanemuk
Alliklik
Kawaiisu
Nomlaki
Pomo (except Lake and Coast)
Some Athabascans perhaps
Inland Esselen
Inland Salinan
Inland Gabrieliño
Inland Diegueño
Cupeño
Nisenan (Foothill)
Maidu (Foothill)
Yana
Wintu
Shasta
Fernandeño
Serrano (except Vanyume)
Hill Patwin
Yuki
Inland Costanoan
Pass and Mountain Cahuilla
Inland Chumash
Inland Luiseño
Juaneño

ECOLOGIC TYPE VIA. DESERT HUNTERS AND GATHERERS

Desert Hunters and Gatherers were found in the Colorado and Mohave deserts. Except in the most favored spots, the type may be considered an impoverished extension of the Great Basin cultures, with whom the California Desert Hunters and Gatherers were all related linguistically and historically. These were the only people in California for whom the pinyon nut was the most important single item of food. The mesquite apparently replaced pinyon in importance for some groups and it is possible that further analysis may permit establishment of low desert and high desert sub-types. In most areas use of the meager resources was further limited by the absence of water, and life centered as much about permanent or seasonal springs and the occasional stream as it did about supplies of food. The type is characterized by extremely low population densities, use of a wide variety of vegetable foods, and extreme simplicity of culture.

The best part of the area was in Owen's Valley, the east slope of the Sierra Nevada and along the Mohave River. Here some permanence of settlement apparently was possible. In the remainder of the area frequent seasonal movement occurred with the individual family commonly the unit.

Peoples of this ecologic type included all or part of the groups speaking the following languages:

Desert Shoshoni
Northern Paiute
Southern Paiute
Washo
Vanyume Serrano
Desert Cahuilla

In addition to these, some groups primarily of Foothill ecologic type occasionally ranged into the desert, for example, Kawaiisu, Kitanemuk and some of the Serrano. However, they had little incentive to do so.

ECOLOGIC TYPE VIB. DESERT FARMERS

These included the Yuma and Mohave along the bottom lands of the Colorado River and the Kamia (or "Farming Eastern Diegueño") of the overflow lands at the south end of the Imperial Valley. At an indeterminately late date a few Chemehuevi also farmed along the Colorado River in Chemehuevi Valley. The Desert Farmers cultivated corn, pumpkins and beans on lands flooded by the Colorado River during high water, usually in June. The Yuma and Mohave apparently obtained around 50 to 60 percent of their food from farming, the Kamia much less. The second most important food source was the mesquite which grew densely along the river wherever the water table was within 50 to 70 feet of the surface. It also grows in the Imperial and Coachella Valleys where ground water is sufficiently near the surface.

Away from the river and a few overflow or seepage areas from the mountains, the land accessible to the farmers was as inhospitable as any part of the Colorado Desert and was used mainly by the scattered Desert Hunters and Gatherers. The fertile lands subject to flooding or supporting mesquite constituted a small fraction of the state, perhaps not exceeding 1000 square miles. The Yuma and Mohave also utilized lands on the Arizona side of the Colorado River.

DISCUSSION

In a general sense our data suggest that within each of these ecological types there are characteristic manners of resource exploitation, settlement patterns, population densities and distributions, and cycles of seasonal movements. To some extent there is a relation between the size of social units and the total habitat area, but a closer fit appears between the size of social unit and the abundance and distribution of the major sources of subsistence. We believe a substantial part of the variance of individual groups from the patterns characteristic of each ecological type will be explainable in terms either of intermediate locations between ecological zones, or through local modifications of habitat and resources.

Cultural correlates of the ecological types are still to be worked out in detail. It is reasonably clear that the different ecological types place differential emphasis upon various widespread technological skills and require some specialized developments. There is some variation in the relative economic importance of the sex roles in different ecological types. It is also evident that in some cases rather strikingly different ecological relationships could provide the conditions for rather similar social organizations. It is equally clear that conversely a single ecological type can provide the sufficient conditions for two alternative types of social organization. At present we have only promising leads for investigation rather than conclusive results. Neither can we at present estimate the extent to which variance from our ecologic types may be influenced by cultural and social factors. Given the nature of the data on the California Indians, it is unlikely that we will be able to provide the kind of quantitative validation used by Birdsell in Australia. On the other hand the new possibilities for an ecological approach provide suggestive and fruitful new ways of examining the California Indian data. We believe the ecological typology presented will be helpful to this end.

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Notes

1. This paper is the product of a research program conducted by Ralph L. Beals, Director, and Joseph A. Hester, Jr., Associate Director, and a staff of assistants at the University of California, Los Angeles. The initial identification and definition of the ecological types herein described was done by Dr. Hester and is based upon an extensive survey of ethnographic, biological, and geographic literature. The complete bibliography would exceed the length of this preliminary paper and its publication is reserved for a more extended report. Writing of this manuscript was done by the senior author while a Fellow at the Center for Advanced Studies in the Behavioral Sciences, and the Staff of the Center provided technical assistance.

2. As none of the surviving California Indians even approximate aboriginal living conditions, the description is couched in the past tense.

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AUTOBIOGRAPHY OF A GUATEMALAN INDIAN

John and Mavis Biesanz and Martin Ordoñez

Note: The writers believe this to be a unique document in social-science research in Guatemala. Not only has most research been directed toward studies of tribal Indians, but also such publications as touch upon "Ladinoization" of the Indian population lack detailed case studies. The term "Ladino" is used in Guatemala to distinguish non-Indian from Indian natives; it refers essentially to cultural differences, as most Ladinos have varying degrees of Indian ancestry.

Martin Ordoñez is a 42-year-old Guatemalan, meticulously dressed in Western business clothes, his strong, broad face adorned with a black mustache, his hair thinning at the crown. He speaks frankly and with animation. These data were given us in three interviews totalling thirteen hours, and the final manuscript was checked and approved by Señor Ordoñez.

I was born in Sololá, a village overlooking Lake Atitlan, in 1917. I was the youngest of many children. My mother, a simple Indian woman from the country, was father's third wife. Although my father dressed in Indian clothes and spoke Cakchiquel as well as several other dialects, there is some doubt that he was really Indian. He spoke Spanish perfectly, although he was illiterate. He died when I was six or seven, so I have never been sure, but mother told me he had been pressed into military service as a youth, deserted the hard life of the barracks, was captured, deserted again, and decided to become an Indian, as he noticed they were left alone in those days. He became one of the principal leaders of the Indian community.

Father sent all his children to school against mother's wishes. He insisted we must learn to read and write and speak Spanish. The others hated school and loved the *costumbres* (the Indian customs), cultivating the corn field and slaughtering pigs, our family business. I hated this work. My older brothers gave me a hoe and *machete* and showed me how to use them. "My hands hurt!" "They'll soon be calloused." They fixed up the tump line and *cacaxte* to teach me to carry loads. "They're too heavy." "We'll start you with smaller loads."

At every chance I sat down away from my brothers, wrote on anything I could find, read scraps of newspapers I'd found. "That's not work," they said. I was always glad to return to school. I was lucky to be the youngest; my help at home was not so necessary. The teachers told me, "It is easier to make a living in an office than in the fields."

I attended the Indian school but the teacher beat me so much that mother put me in the Ladino school where I was the only Indian. The children were cruel, called me names, and spit on me. But the teacher helped me. One day in sixth grade I heard of a scholarship. Without really knowing what it was, I tried and won. It was for high school in Guatemala City, and was given by a lawyer who like me had been a Sololá Indian and was given an education by a Ladino for whom he worked as a servant. It provided lodging and tuition for an Indian boy.

Mother did not want to send me, but two or three Ladino friends of my father urged her to. "He will earn money when he has a title." The mayor promised to pay for new clothes. "But go in your *típica* clothes," he said, "so Lawyer C. will know you are Indian."

When I arrived in the capital at the lawyer's house, he snapped, "Take off those clothes at once and dress in your new ones." I didn't know how to put them on; Indians don't wear underwear. His sons showed me how. In a few minutes I was changed into a Ladino. How soft, how light, the clothes felt, but how warm and strange!

"Tomorrow," said the lawyer, "you'll cut your hair and clean your nails." Thus began the hard, abrupt change from one culture to another. I had eaten on the ground, with my fingers. Now I had to sit at a table and use silverware. I had to wash my hands and face, brush my teeth, comb my hair. I hated it. "They don't make me do this at home." I was sick often; the doctor said it was just that I didn't want to study.

After eight months I sneaked away one night when they took us to play soccer, and went home. When the next school term began they told mother I had to return or she would have to pay for the previous year. I said "I'll earn the money, Mother. Think of it, there at school I had to get up and take a cold shower every day, brush my teeth, make my bed, take off my clothes when I went to bed. I didn't like it."

I spent six months in Quezaltenango as apprentice to a carpenter; then my sister died and mother called me home. In Sololá I was apprenticed to a tailor, but I didn't like it. Again I tried carpentry, then shoemaking, but didn't like them either. In 1930 my godfather, a dentist, offered to teach me dentistry, so again I went to Quezaltenango. After a while my brother died, and mother sent word to come home. Again we were told we would have to repay the lawyer unless I went to school. We had no money, so I decided to return.

All this time I had not put on Indian clothes. Mother kept asking me to put on a pretty, colorful, well-adorned costume. I would say, "In a week." I kept putting it off. I liked Ladino clothes. I even served as houseboy to a man in Sololá who gave me clothes. But other Indians said to mother, "Why do you have that man in the family with those clothes? It lowers the family's prestige."

When I returned to the capital my old classmates were still there. They accepted me as a companion; I played soccer and swam, and did well. The professors forbade them to tease me. I felt equal to any of them. A professor of Spanish, a wonderful man, stimulated me to study.

As we grew older all the other boys talked of their *novias* (sweethearts); I did not dare. Neither a Ladina nor an Indian would accept me. Otherwise I was happy there from 1930 to 1936. The first time, the change was too abrupt. I was uprooted; it was like going from hot to cold water. But in Quezaltenango I had been only with Ladinos, so when I returned to school it was easy to become accustomed to the new life.

I received my title of Teacher and was assigned to a village on the lake. Mother still wanted me to wear Indian dress, but I said I could not teach if I wore it. I began to give her money. Since then she has not urged me to change.

After three years I was promoted to a job in Sololá, then taught in several other towns, and was back in Sololá in 1952 when I was asked to work in the Indian Institute in the capital as a specialist in Indian languages. Since then I've lived here.

In the whole country there is a feeling of distance between Indians and

Ladinos; they don't like each other. It is traditional; the Indians remember the barbarisms of the Conquistadores, who didn't respect their women, not even the princesses. They hate the Ladinos. There is some friendship, but without free trust and mutual confidence. The degree of hate and mistrust varies from one community to another, but everywhere the Indian knows that if he gets too close to the Ladino he will suffer for it. The poor Indians are not clean, so the Ladinos reject them.

In Sololá I am always an Indian to the Ladinos. Anywhere else I am catalogued as Ladino. When I was teaching there I was nominated for Mayor. Some people said, "You would be a bridge between the Indian and Ladino communities." That was true, so I accepted. But some Ladinos who are friends to my face made counterpropaganda behind my back. "Will you let an Indian dominate you?" That eliminated me—the racial discrimination which on the surface does not seem to exist but very definitely does exist. The Ladinos do not want Indians to climb. They call Indians who change *mestizos* or *mixtados*; these are insults. They mean "an Indian in Ladino dress." When mother is gone, I shall not return to Sololá as I feel this contempt.

They do not admit me to society in Sololá, and I do not intrude. If a friend asks me to a party, I go, and people are civil, but not cordial. Sometimes, to mock me, they say with exaggerated courtesy, "Good morning, Doctor," or "Adiós, Licenciado." When they want something, though, like a recommendation for a job in the capital, I'm "Martincito," and get the best chair in the house.

One suffers with this discrimination. The only ones who are erasing it are the scientists who study ethnic relations. The Ladinos deny their findings. "We don't have Indian blood," they say; "not any more." To the tribal Indians it does not matter. If they are rudely told, "Get out, Indian," they shrug their shoulders and go. They are not affected. But I have dignity and self-respect so such treatment does affect me.

To the Indians I am more Ladino than Indian. Only if I put on the traditional costume would I be fully accepted. When I was small, they criticized me, called me *tacmayín*—which means "neither one nor the other." But now my brothers respect me very much. They, and others in the community, too, consult me on all kinds of matters, as if I were a doctor or lawyer. I am often able to advise them, or convince them to see a doctor rather than a *brujo*.

Mother came to visit me at the Institute one day. If I had been like some others I would have acted as though she were a servant. But why should I deny my mother? I confess that for about five years when I was first teaching I did feel ashamed of her and of my Indian background. But it is nothing to hide. If I denied it, people would know anyway and think less of me for lying. Some who have made the change deny their families. But my mother, in spite of her early opposition, did a lot to help me through secondary and normal school. When I go home to Sololá I take her to the movies. When we are together she is called Señora and treated respectfully, and given a good seat. When we walk together, Ladinos come to their doors. I hear some say, "Is *she* his mother?" "Yes, an Indita."

When I am not with her, she is pushed aside like any other Indian. But among the Indians she is a *principal*, highly respected because my father served in the politico-religious hierarchy of the Indian community and because she herself has served.

Mother does not love me as much as the other brothers. "You have money. You don't need me so much." She gives freely to them—clothes, firewood, land, money. I am the only one who gives her money. When I do, she asks, "Is that all?" She thinks because I am working in the capital I am making a lot.

Marrying is a big dilemma. In chatting with Indian girls in Sololá, I would joke about marriage, and they said, "No, you have different customs. I wouldn't know how to fix your clothes, or iron, or darn socks." Of course, I could teach one if I were serious, but I have been serious only about Ladinas because I taught Ladino children and thought by marrying a Ladina I could best incorporate myself into Ladino civilization. But I was wrong. At first I thought I'd find one from another village, but several proved unfaithful. If I had brought one to Sololá she would soon have been sorry and disillusioned. Then I found a poor but cultured girl in Sololá. We set the date. But she was sent away by her parents and her friend secretly told me it was because I am Indian. I know some men with Ladina wives; whenever a crisis comes up they are contemptuously called Indian. I live with a Ladina but will not marry her because it is simpler this way. If I ever do marry perhaps my wife will be Indian. That would give me more peace of mind.

Many Indians come to the capital, leave off the costume, and work for a while. But when they return they put on Indian costume again. Otherwise they cannot be chosen to serve in the hierarchy and acquire status. Some, especially those who have served in the army and learned to read and write, use their new knowledge to exploit others; they take away their land and their women.

Others who have changed to Ladino have the same troubles as I; some have it worse because their own families are harder on them. My family has some Ladino customs because they live in town. Those who come from the *monte* have more problems. Some change their surnames and pretend they are of German or Ladino background. I did not have to because mine is already Spanish.

I feel equal to anyone. I am happy and satisfied away from Sololá. I miss some things. Indian society is systematically organized according to centuries of traditions. It is very nice to have everything so orderly. Children learn by example.

But I do not follow a single Indian custom. I do not kiss the hands of the *principales*, not even my mother's. Why? They are unwashed. I stay at a hotel when I visit mother. When I was teaching in Sololá, I lived with her for a while, but she said, "Why don't you find a place where you can eat at a table, with silver, where there are windows, and mopped tile floors?" She was right.

I have tried to change some of my family's customs. Mother slept on a rush mat and a dressed animal skin spread over a board platform. I bought her a bed and a mattress. She uses it, but spreads the mat and hide over the mattress. I cannot teach her to wash dishes properly. I tell my nephews not to stick their dirty hands in the pot of food and eat with their fingers, but my brothers resent my meddling. They are Catholics but believe in *brujos* (witches); I am a Catholic by education and conviction as well as blind faith from childhood. They use a weekly sweat bath; I shower in cold water every night, like all Ladinos.

When I work in the fields, it is for exercise, as a sport. I like to cut firewood and wield the hoe; it is a healthful change.

It is hard to be an Indian and try to assimilate the national culture. The Indians who stay in their own environment are happy. But I do not regret having made the change, in spite of all my problems.

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LES COMMUNAUTÉS D'ENTR'AIDE DES BAMBARA DU SOUDAN FRANÇAIS

Solange de Ganay

Dans la plupart des villages bambara de la région de Segou (Soudan Français), il existe une "communauté de culture" dite "*tyike to,*" qui est un élément capital de leur structure sociale. Mais, avant d'en examiner l'organisation et le fonctionnement, il paraît indispensable d'expliquer d'abord ce que recouvre, pour les Bambara, la notion générale de *to.*

Le mot *to* exprime aussi bien l'idée d'amoncellement, de groupement que celle d'ordre et de durée. C'est ainsi que ce vocable désigne toute association soumise à une règle. L'on dira qu'un corps humain, une pensée, le monde entier sont des *to,* parce que leurs éléments se groupent selon une certaine loi, condition de leur existence et de leur durée.

De manière plus courante, le *to* désigne, chez les Bambara, tous les types de groupements humains: famille, classe d'âge, habitants d'un même quartier, d'un même village forment autant de *to.* (Seules, les associations à caractère religieux portent un nom spécial: *dyo*). Cependant, le mot: *to* s'emploie plus spécialement, lorsqu'il s'agit de groupements à caractère d'entr'aide: *gwa to* pour le quartier, *tyike to* pour le village et *muso furu to* participant aux dépenses occasionnées par les mariages. Il existe aussi des *dye to* (sociétés d'assemblage) réunissant, toujours dans un but d'entr'aide, les habitants d'agglomérations différentes.

Mais, lorsque, dans un village bambara, on parle simplement du *to,* il s'agit habituellement du *tyike to* (communauté de travail) qui est, en effet, le *to* par excellence.

Etymologiquement, *tyike* vient de *tyi* qui signifie: ordre, loi et, par extension, ce qui est commandé, tâche, travail, culture. Le *tyike to* est la collectivité qui exécute un travail. Mais, ce caractère d'exécutant ne doit pas cacher le rôle beaucoup plus important joué en réalité par l'association.

Rôle économique tout d'abord: car, du *to,* dépend la prospérité matérielle du village, puisque l'agriculture constitue son principal champ d'activité.

Rôle social ensuite. D'une part, ce *to* est, en effet, un organisme d'entr'aide au profit non seulement de ses membres actifs mais aussi des gens âgés qui n'en font plus partie, des malades et d'une manière générale, de tous ceux qui ont besoin d'être secondés, dans les travaux agricoles ou autres. D'autre part, le *to* est l'âme des diverses manifestations collectives de la vie du village.

Rôle éducatif, enfin, car la Société doit assurer la formation complète de l'individu: elle apprend aux jeunes à mieux cultiver la terre, elle développe leur sens de l'émulation, de la solidarité, de la discipline personnelle.

Le *tyike to* est ainsi responsable de l'ordre, de l'entrain, de l'union qui doivent régner dans la communauté villageoise, de sa stabilité et c'est donc grâce à lui que s'établira la renommée de celle-ci.

Sur le plan métaphysique enfin, le *tyike to,* symbole d'action, d'entente, d'union et de durée, représente le principe Créateur par rapport à l'ensemble de la création figurée par le reste des villageois.

Tout habitant du village doit obligatoirement passer par le *to*: l'entrée dans la Société est déterminée par l'âge, plus précisément par l'aptitude physique au travail, et non par le critère social. Cependant, les gens de caste ne peuvent accéder aux fonctions de chef. Les garçons sont intégrés dans la Société, avant même leur circoncision, dès qu'ils sont assez forts pour participer aux travaux des champs. Lorsqu'ils auront acquis les qualités qui feront d'eux des hommes et qu'ils seront capables de les transmettre aux autres, on estime qu'ils pourront quitter le *to*. Jadis, les hommes ne quittaient le *to* que vers quarante ans. Mais, actuellement, dans de nombreux villages, ils en sortent bien avant cet âge.

Quant aux filles, elles sont enrôlées dans l'association, deux ans avant leur excision et y restent jusqu'à leur mariage. Elles font ensuite partie du *to* du village de leur mari, tant que celui-ci en est encore membre.

Dans le but d'assurer la bonne formation de ses membres, cette Société est assez fortement hiérarchisée. Son organisation rappelle celle de l'ancien royaume bambara, en vue des guerres; la hiérarchie de celui-ci aurait eu, en effet, pour modèle, celle du *tyike to*, qui existait dans chaque village bambara, la lutte de ses membres contre la famine pouvant être assimilée à une véritable guerre.

La société des *to deu* (fils du *to*) est dirigée par le *to tigi* (maître du *to*) appelé aussi *to fama* (chef du *to*) du nom donné aux anciens rois bambara. Celui-ci est responsable de la bonne marche du *to*, il en convoque les membres, lorsqu'il le juge nécessaire, il sert d'intermédiaire entre eux et ceux qui demandent l'aide du *to*. C'est également lui qui préside aux diverses activités sociales du *to* et organise la fête annuelle. Il est enfin le trésorier de la société.

Le *Fama* est assisté d'un *syere* (suivant) qui lui sert d'interprète.

Vient ensuite le *to dyo kuntigi* (c'est à dire le "chef des archers," par analogie avec l'ancienne hiérarchie du royaume) qui surveille les travaux exécutés par les *to deu* et, enfin, le *to dyeli*, ou griot de la société, qui est habituellement de caste. Ces 4 personnages font partie de la classe des *to tigi* (litt. possesseurs du *to*).

Quant au *tyi tigi*, il sert d'intermédiaire entre les cultivateurs qui ont besoin d'aide et le *Fama*.

Dans certains villages, le *to* est divisé en deux groupes comprenant en nombre égal, les enfants et les membres plus âgés, l'un sous les ordres du *Fama*, l'autre sous ceux du *syere*. Mais, les agglomérations importantes peuvent avoir deux *tyike to*, qui auront chacun leurs propres chefs.

Le *Fama*, ainsi que son second, était jadis élu par les Sociétaires eux-mêmes. Actuellement, dans nombre de villages, il est choisi, dans la plus ancienne classe d'âge du *to*, par les 4 *to tigi* sortants et les vieillards du village, pour sa valeur et son aptitude à diriger le *to*, de sorte qu'il n'est pas nécessairement le plus âgé de sa classe d'âge, comme il devrait l'être théoriquement.

D'autre part, les *to tigi* restent en fonction beaucoup moins longtemps qu'autrefois: la durée du mandat d'un *Fama* ne peut excéder sept ans et elle est généralement de trois ou quatre ans, alors que, jadis, certains *Fama* pouvaient le rester, jusqu'à un âge avancé.

Rajeunissement des membres du *to*, du *to tigi*, changement plus fréquent de celui-ci, toutes ces tendances récentes montrent un souci de rénover le *to*. Car, cette institution rencontre actuellement une opposition de la part de certains de ses membres (surtout ceux qui ont atteint l'âge d'homme) qui cherchent à s'émanciper: ils se refusent à travailler en commun, sans être rémunérés personnellement, ne veulent plus payer les amendes, désirent échapper à la tutelle du *to tigi* etc. . . . Plutôt que de conserver de tels éléments capables d'entretenir une certaine indiscipline, au sein du groupe, on estime qu'il est préférable de leur laisser quitter le *to* et de faire de celui-ci un groupement de jeunes plus respectueux de l'autorité du chef (qui appartient maintenant à leur génération).

D'autre part, ce rajeunissement du *to* permet désormais de faire de cette association l'"áile marchante" du village, sa fraction active, capable de créer un climat d'émulation, agissant efficacement sur tout l'ensemble du village.

La tête de la branche féminine du *to* (la *mbogotigi kuntigi*) est choisie par chaque nouveau *Fama*; elle a, sous ses ordres, une *to dyeli mosoma* qui transmet aux autres filles les ordres qu'elle même a reçus du *Fama*, par l'intermédiaire du *to dyeli*.

La *mbogotigi kuntigi* est, en principe, la plus âgée de la plus ancienne classe d'âge du *to* mais on choisit sa cadette immédiate, si, pour des raisons de santé, de conduite ou de classe sociale, elle ne remplit pas les conditions requises. Elle doit être, en effet, de classe noble (c'est à dire ni castée, ni descendante de captifs, ni née hors mariage) et de parents natifs du village.

Dans certains villages, la mère du *to Fama* (la *Nene*) joue un rôle important, au sein de la société.

Le but du *to* étant de former les jeunes, au point de vue agricole, moral, social, une discipline assez stricte y est de rigueur: en cas de manquement, des amendes sont infligées. Cependant, la mise en jeu des mesures disciplinaires est assez rare. Car, le *to* fait essentiellement appel à la conscience des individus. Il a pour but de faire agir ses membres, non pas par peur des sanctions, mais par goût de l'émulation. Il s'agit de faire preuve de persévérance, de montrer l'effort dont on est capable, pour arriver le premier et rester le dernier au travail, pour accomplir sa tâche vite et bien, en sachant se passer de nourriture etc. . . . bref, d'acquérir une discipline intérieure. On ne peut qu'être frappé du caractère élevé de cette éthique.

D'une façon générale, le *to* participe activement à la vie économique et sociale du village; il est notamment un organisme d'entr'aide soit au profit des particuliers, soit pour des travaux d'intérêt général.

Si un villageois ne peut faire seul un travail urgent ou dépassant ses propres capacités, il fera appel au *to*. L'association participe ainsi à la construction des maisons, au transport du bois de charpente, de chauffage mais, surtout, aux travaux agricoles ayant besoin d'être exécutés rapidement. C'est à l'occasion du second sarclage que les *to deu* rivalisent d'ardeur pour devenir "champions de culture." Dans cette compétition, ils sont encouragés à accélérer le rythme de travail, par les tambourinaires et par les chants et les battements de main des filles.

Enfin, chaque fois que l'un des membres du *to* doit, selon l'usage, cultiver le champ de ses futurs beaux parents, l'association toute entière (et non pas seulement la classe d'âge du garçon) lui apporte son aide.

Les filles participent directement à certains travaux du *to* (transport des matériaux notamment) mais dans la mesure de leurs capacités physiques. Ce sont elles habituellement qui ont la charge des tambours de la Société, lorsque celle-ci se déplace. D'autre part, lors des travaux agricoles exécutés par le *to*, elles apportent, dans les champs, l'eau et la nourriture de leur *to ke*, garçon du *to* dont chacune doit s'occuper plus spécialement, pour s'habituer ainsi à prendre soin de son futur mari.

Les travaux qu'effectue l'association sont parfois rémunérés en espèces mais, le plus souvent, ils le sont soit en nourriture, soit en grains ou animaux, qui sont alors conservés pour être consommés, lors des repas communs de la fête annuelle.

Dans les villages où l'association possède un champ qui lui est propre, la récolte en est utilisée, lors de cette même fête.

Le *to* joue encore un rôle important dans les diverses activités sociales du village. D'une manière générale, nous l'avons vu, il est responsable de l'entrain qui doit régner dans l'agglomération. Aussi, doit il favoriser les occasions de faisant naître, développer les moyens permettant de l'entretenir, veiller à ce qu'il se manifeste, lors des diverses fêtes ou cérémonies marquantes de l'année.

Lorsque des griots ou des conteurs passent dans un village, ils sont reçus par le *to tigi* et chaque sociétaire doit leur donner un peu d'argent.

C'est aussi au *to* qu'incombe la réfection des masques qui sortent à la fin de la saison des pluies, l'achat, la garde et l'entretien des divers instruments de musique du village.

Le *to* anime encore les fêtes se déroulant à l'occasion de la circoncision, de l'excision et des mariages: les *to deu* y dansent au son des tambours de la Société. Ils accompagnent également la fiancée quittant son village, pour se rendre chez son futur mari.

C'est enfin le *to* qui est parfois convoqué, lorsqu'on veut donner de l'éclat à une cérémonie funéraire.

Mais, la manifestation pour laquelle le *to* déploie le plus d'activité est, bien entendu, sa propre fête annuelle dite *to nyene dye*, c'est à dire "union sincère et lumineuse." Celle-ci a lieu en saison sèche, vers mars ou avril: l'ensemble du *to* en choisit la date, d'un commun accord.

Quinze jours au moins avant la fête, tous les paiements encore dûs au *to* doivent être effectués. Car, les produits directement amassés seront consommés lors des réjouissances et les sommes versées, consacrées à l'achat de denrées supplémentaires, de tambours, si c'est nécessaire, de vêtements et de bonnets de tambourinaires (dans certains villages), de kolas, de drapeaux etc. . . .

Ce sont le *Fama* et la *Nene*, sa Mère, qui distribuent le mil aux différentes familles chargées de préparer la nourriture et la boisson.

Dans les villages où il n'existe pas de case du *to* (*to so*) permanente, un hangar en branchages est édifié sur la place principale du village, pour servir d'abri aux chefs du *to* et aux notables.

Le *Fama* invite le plus souvent les *to* des villages voisins à venir assister aux réjouissances. La veille de la fête, il convoque les Sociétaires et leur recommande de lui donner de l'entrain, de bien s'y conduire et de ne pas somnoler pendant la veillée.

La fête est le symbole de l'unité du village, la manifestation de sa cohésion interne qu'elle contribuera à renforcer encore dans l'avenir. Tous les habitants du village doivent y être présents, sans exception. Les filles, nées dans le village et mariées à l'extérieur (*namadeu*), y participent également. La réunion de tous les membres de la communauté villageoise, sur la grande place, prouve aux assistants que tous partagent les mêmes croyances, les mêmes pensées et que, de ce fait, l'union règne entre eux.

Les réjouissances durent habituellement trois jours, pour assurer le maintien du village, car, le nombre trois, mâle, est celui de la consolidation. (Dans certain endroits, elle se poursuit le 4ème jour qui est celui des remerciements au *to*.)

Les réjouissances commencent par une veillée (*sinyena*) qui dure toute la nuit et revêt une extrême importance. L'obligation de ne pas dormir jusqu'au lever du soleil est absolue. Des amendes sévères sont infligées aux *to deu* qui somnolent. Car, la veillée n'est pas seulement une réjouissance, elle symbolise l'effort qu'exigent les travaux du *to*, pour être exécutés. Elle est signe de progrès, d'avancement, de renaissance pour le village. Elle l'aide à s'élever, à se multiplier. Dormir au moment de la veillée affaiblirait les actions du *to* pour l'année à venir; la Société perdrait son énergie, son entrain et sombrerait dans l'engourdissement et la paresse, le monde créé lui-même perdrait une partie de son éclat et de son esprit. En effet, cette veillée a aussi une signification métaphysique: elle symbolise la Création faite par le Dieu Créateur, dans la clarté et le mouvement de sa pensée, sans s'obscurcir un seul instant.

Au contraire, si les gens ne dorment pas à la veillée, les travaux du *to* se feront plus facilement et avec plus de courage, au cours de l'année.

Enfin, la veillée bien faite a pour but de réaliser l'accord parfait entre les puissances surnaturelles invisibles qui ont aidé à "asseoir" le village et les membres du *to* qui travaillent et font marcher le *to* extérieurement.

Les trois grands repas organisés par le *to* ont lieu vers midi, pendant les trois jours que dure la fête. Lorsque les plats sont prêts, ils sont apportés chez le *Fama* qui en désigne un certain nombre pour être consommés par les diverses catégories d'assistants: sociétaires, vieillards, notables, forgerons, filles du village mariées à l'extérieur, *to* des villages voisins, etc. Une fois cette répartition faite, tout le reste est déposé sous le hangar et peut être consommé par quiconque, autochtone ou étranger.

Ces repas ont, comme la veillée, une signification profonde. Etant pris en commun, ils sèment entre ceux qui le partagent, la bonne entente, la confiance et la joie. De plus, comme la graine de mil est le symbole de la vie et de la renaissance, ils assurent aussi le renouveau et la perpétuation du village. Enfin, leur abondance est la manifestation de l'activité dont le *to* a fait preuve durant toute l'année, puisque les aliments consommés durant ces repas sont le prix du travail effectué per l'association.

Pendant les trois jours, les danses commencent après ce repas et se poursuivent durant toute l'aprés-midi. Certaines rassemblent la communauté toute entière, telle la *bo dyala*, à laquelle hommes et femmes participent. D'autres sont accomplies par des groupes plus restreints: danse du *Ndomo du* par les incirconcis, danses des *to dyo* et des *wolo so* par les garçons du *to* et les descendants de captifs de case (*wolo so*). Celle des *to dyo* rappelle les bouffonneries des esclaves des anciens Rois bambara, chargés de faire rire ceux-ci.

Dans certains villages, trois masques apparaissent à la fête du *to*: ils représentent un homme, son épouse et un jeune homme poursuivant la femme de ses assiduités.

A la fin de la fête, ou parfois dès le fin de la veillée, le chef du village (*dugu tigi*) et le chef du *to* remercient tous les participants (*to* du village même et des agglomérations voisines, assistants divers).

Le *to tigi* leur rappelle ensuite la signification générale du mot: *to* (société), en montrant le rôle capital joué par cette notion, dans le monde: "Sans Société, l'homme ne peut pas vivre." Le *to* de village symbolise l'union du monde entier: c'est pourquoi le *to tigi* exhorte la communauté à rester unie, grâce au *to*: "Ne soyez pas comme un arbre seul. L'homme seul ne peut ni être heureux, ni se rendre utile. . . . Soyons solidaires et attachons-nous à la bonne marche du *to*, pour maintenir le village et continuer l'œuvre de nos ancêtres." Le chef du *to* explique enfin le sens profond des différentes parties de la fête: veillée, repas etc. . . . Toutes ses paroles sont toujours interprétées ensuite par un griot ou un forgeron (de même que jadis, la Parole des Rois avait besoin d'un intermédiaire qui la transmette au peuple).

En conclusion de ce bref exposé, il nous paraît utile d'insister sur l'évolution récente du *to* et sa signification. En fait, malgré des tentatives pour rénover le *to*, pour en faire un groupement de jeunes, dirigé par les jeunes eux-mêmes, comme nous l'avons vu, cette institution rencontre parfois des oppositions, non pas à cause de son fonctionnement, mais parce qu'elle est le symbole de la structure sociale traditionnelle, considérée comme périmée par certains évolués.

Respect de l'autorité du chef, obligations à l'égard de la communauté, les principes même sur lesquels est fondé le *to* vont évidemment à l'encontre de la tendance à l'individualisme et du désir de liberté complète qui se manifestent dans le monde noir, un peu dans tous les domaines. Il est possible que, sous ces diverses influences, l'institution du *to* arrive à disparaître, mais ce serait alors toute la structure sociale communautaire bambara qui s'effondrerait.

Paris, France.

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A COMPARISON OF EASTERN KERESAN AND TEWA KINSHIP SYSTEMS[1]

Edward P. Dozier

The Pueblo Indians of the Rio Grande area in New Mexico speak languages belonging to two different linguistic stocks, Tanoan and Keresan. Some recently developed archaeological hypotheses (Reed 1946 and 1949) derive the Tanoan and Keresan speaking peoples from two different geographical regions. The Tanoans are believed to have come first, followed by the Keresans sometime between 1100 and 1400 A.D. Although both puebloid in culture, they were distinctly different in their kinship systems and other features of social organization. The Keresans had and still possess a kinship system that is fitted to lineage and clan structures. The Tanoans did not have a lineage or clan system but were and are still organized along a bilateral, generational model.

At present there are five Rio Grande or Eastern Keresan speaking villages with a total population of about 3500. The Tanoan linguistic stock is divided into three families, Tewa, Tiwa and Towa. There are five Tewa pueblos, four of Tiwa, and only one Towa-speaking pueblo. The Tewa number about 2500, the Tiwa population is approximately the same as the Tewa and the one Towa pueblo has about 700 inhabitants. Over-all population of the Tanoan pueblos is about 5700.

In the present discussion, only material from the Eastern Keresan and the Tewa pueblos will be presented. The Western Pueblo Keresan communities of Acoma and Laguna, west of the Rio Grande drainage are excluded. These pueblos have kinship systems that are like those of Hopi, Hopi-Tewa and Zuni; all of these systems have been fully described by Eggan in his recent book on the Social Organization of the Western Pueblos (1950).

The Tewa have been selected from the Tanoans because data for this group are most complete. The studies that have been made of other Tanoan pueblo groups, however, indicate that kinship patterns are basically similar among all of the Tanoans, contrasting sharply with those of the Keresans.

The two pueblo peoples have occupied the same geographical area for a period of five or six centuries. For almost 400 years they have also been in close contact with Spanish-Mexican culture and for the last one hundred years with Anglo-American culture. Despite extensive mutual association of Keresan and Tanoan speaking peoples and both of these groups with the European cultures involved, the two kinship systems have remained essentially distinct. Influences are apparent, especially of Tanoan ones on Keresan, but the systems have not merged and the important characteristics of each system have remained. In contrast to the obvious influences of one pueblo group upon the other, Spanish and Anglo-American kinship systems seem not to have profoundly affected these pueblos. This situation appears to demonstrate the persisting nature of kinship structures and the conservatism of pueblo culture in general.

Before we discuss the significance of this phenomenon it is important to sketch briefly the essential characteristics of each system.

THE TEWA KINSHIP SYSTEM

Tewa Kinship Terms.[2] Tewa kinship terms are almost perfectly bilateral. There is extensive use of senior and junior reciprocals and an emphasis is laid on age or generation.

In Ego's generation, siblings are distinguished as older and younger, but distinctions are not made in terms of sex. Parallel and cross cousins are raised one generation or lowered one, depending on whether they are older or younger than Ego. The term used is also dependent on whether the uncle or aunt is younger than Ego's parent. That is, cousins are designated as either older uncle and younger uncle or as older aunt and younger aunt. Appropriate senior and junior reciprocals are made in response.

Ego's own children are called "my children". Since terms are always reciprocal, a man's older siblings' children are parents' younger sibling (diminutive, i.e. the junior reciprocal), and younger siblings' children are parents' older sibling (diminutive). The same kind of distinctions are made by a woman, but different terms are used. Children's children are "little grandfathers" or "little grandmothers". Great grandchildren are similarly addressed and referred to with the junior reciprocal.

In the ascending generation, mother's sisters and father's sisters are distinguished by separate terms whether older or younger than the parent; mother's and father's brothers are also similarly distinguished.

Men and women of the bilateral extended household above the parent generation, with the exception of the grandparents, are called by father and mother terms.

Tewa terminological structure is thus neatly bilateral and equalizing. Alternate terms for grandparents, parents, younger and older sisters exist. Whether, at one time, one set was for the maternal side and the other set for the paternal side is not clear. Inconsistencies that exist in the terminological system today involve the use of alternate terms in a village, although within a given household the set of terms used are consistently applied.

Tewa Kinship Behavior.[3] The basic social and economic unit of the Tewa pueblos is the bilateral extended family with patrilineal tendency. This unit consists of a man and his wife, one or more married daughters and their husbands, one or more married sons and their wives, and all unmarried sons and daughters of the married members of the household. Other daughters of the old couple and their children may align themselves with the households of their husbands, and occasionally one or more sons of the old couple may also join their wife's extended household. Initially a couple may shift household allegiances, but once mutual work, food sharing, and visiting patterns are established, the household takes on remarkable cohesiveness.

Discipline and training, both economic and social, are administered by the parents of the child. Serious cases of discipline may be taken before the oldest man in the household who may be either a paternal or maternal grandfather. In the absence of a grandparent, the oldest uncle, either on the mother's or father's side, may perform this function. More serious cases are handled at the village level by duly appointed disciplinarians.

Residence is ordinarily patrilocal, though there are scattered instances of the establishment of separate residences. Separate residences do not destroy the cohesiveness of the extended bilateral household. Various cooperative activities tend to keep the integrity of the unit. Among the most important are planting, the caring for and the final harvesting of the crops and the distribution and sharing of food stuffs.

Land is village owned and families have only use rights to the land—such rights are passed on to both daughters and sons. The right to the use of land and other property is usually transferred by the oldest surviving member of the household when he or she can no longer assume the responsibilities of caring for such property. Older members of the household are favored with the best plots of land or with choice pieces of property, but younger married members are not ignored. Younger members of a large extended family who are allotted poor land or who cannot get land from older relatives may appeal today to the governor for additional land. One of the main sources of household friction arises over the inheritance of property, but land, since it is worked together and its products shared by all members of the extended family, does not ordinarily present a problem.

Within the extended household, maximum interaction and strongest loyalties exist. In all relations within this unit, age is important, respect and obedience being accorded the older kin.

Terminology and behavior both reflect the bilateral and generational character of the Tewa kinship system. There is no hint of a former lineage or clan system in the kinship terms or in the network of social relations among the kin group. Clan names do exist, but such names appear to be no more than sacred terms, one of which is inherited along family lines. Some Tewa feel that such a clan name is inherited through the mother, others claim that the name comes through the father. As with property, food, and work patterns, we might say that a specific clan name is the possession of the bilateral extended household unit. Tewa "clan names" have nothing to do with government, ceremonies, or the regulation of property or marriage. They are uttered as a litany along with other esoteric terms by ceremonial leaders on such solemn occasions as society retreats and masked dance ceremonies. We might suggest that Tewa clan names appear to be an imperfect and undigested diffusion of the clan concept from Keresan neighbors. The fact that the northern Tiwa, farthest removed from the Keresans, do not have such clan names would tend to support this hypothesis.

Let us now turn to a brief characterization of the Eastern Keresan kinship system.

EASTERN KERESAN KINSHIP SYSTEM[4]

Keresan Kinship Terms. Classificatory principles permeate Keresan kinship terminology and kinship usages suggest affinity to the general Western Pueblo pattern. The use of separate terms, in many instances, by men and women for the same relative is also characteristic of the system. Inconsistencies appear in all areas which we can probably explain, at least in part, on Tanoan influence and the resultant breakdown of clan importance.

Ego distinguishes brother and sister; parallel and cross cousins are classified as siblings in most of the groups.

Ego's own son is classified with brother's son, mother's sister's son's son, father's brother's son's son and father's sister's son's son. Daughter terms parallel those for son in usage. Here the attempts to equalize the maternal and paternal kin is evident.

Grandparent-grandchild terms are reciprocals, suggestive of the Tewa system, although in the latter, they are not true reciprocals; rather it is the application of one term to the senior relative and the addition of a diminutive suffix when the junior member is addressed. Grandparent-grandchild terms are used for grandparents, their siblings, and the children of anyone designated as "son" or "daughter".

Mother is classified with her sisters, father's brother's wives and with father's sisters. The grouping of mother with father's sisters is a departure from the Western Pueblo pattern and suggests the characteristic pattern to equalize the relationship between maternal and paternal relatives.

Father is grouped with father's brother, father's sister's husband, and, in some groups, with mother's brother. The classification of father with mother's brother seems not too well established, but exists as an alternate pattern in Santa Ana, Santo Domingo, and Cochiti. The attempt to approximate a bilateral arrangement is again apparent here.

The term for "mother's brother" is applied to mother's brothers generally and at Zia a diminutive term for uncle is used for cross cousins (Hawley 1950: 505). In a few of the Keresan villages, notably at Santa Ana, two patterns are evident. Leslie White (1942a: 159) characterizes these patterns for Santa Ana as follows:

> We note that we have two patterns: (1) mother's brother is called "uncle" and his children "son" and "daughter" and (2) mother's brother is called "father" and his children are "brother" and "sister". We believe that the presence and use of these two patterns at Santa Ana today indicate a transition from one type of kinship system or pattern, to another. It is our considered opinion that the pattern in which mother's brother is called "uncle" and his children "son" and "daughter", which is characteristic of the Crow type of terminology, is the earlier of the two patterns. We believe that this pattern is breaking down and giving way to the pattern in which mother's brother is called "father" and his children "brother" and "sister".

A separate term for mother's brother, indicative of the specialized function of this relative, as among the Western Pueblos, seems reasonable as the older pattern. The alternate pattern suggests the loss of the specialized functions of mother's brother, as is indeed the case as we shall see in our discussion of kinship behavior.

The brief sketch of Eastern Keresan kinship terms above indicates affinity to the Western Pueblo model of kinship terminology; the inconsistencies, on the other hand, show a persistent trend toward a bilateral arrangement.

Keresan Kinship Behavior. Although our data on kinship behavior among the Keresan pueblos are meagre, the material shows similarities to Western Pueblo patterns and also tendencies to equalize the relationship between maternal and paternal relatives. Thus terminology and behavior appear to be consonant with one another.

We may conveniently describe the behavior of kin in terms of the matrilineal, extended family which appears to be an important unit of Keresan social organization. Mother, mother's sisters and mother's brothers are most important in the socialization of the individual. The child is reared within the context of the maternal kin and until he is married spends the greater part of his life with them. Father and father's relatives have different relations to Ego. They are concerned, particularly, that an individual is properly guided along the life cycle. One of father's sisters is especially important in this respect and sees to it that her niece or nephew is properly launched from one crisis period to another in his or her development.

The behavioral patterns sketched above show some inconsistencies. In some households, notably at Cochiti, the father exercises disciplinary powers, while the mother's brother and father's brother are respect relatives but without specialized functions to Ego. In general, however, Keresan kinship behavior patterns follow basic Western Pueblo practices, that is, the greater importance of the maternal relatives, the importance of mother's brother, and the specialized functions of father's relatives, particularly of father's sister.

Keresan Clans. Indications of the former importance of the matrilineal clan in terms of government, control of property, religion and ceremonies is clear. These functions have been assumed for the most part by the esoteric societies where membership crosscuts an entire village. Nevertheless there are still hints of clan roles and functions. In Zia, for example, the cacique must come from a specific clan. In Zia, also, clans conduct initiation ceremonies and the leaders of certain religious societies come from specific clans (Hawley 1950: 506–507). Thus, although many of the important functions of the clan have lapsed, it is still an important institution in Keresan life at present. Actually, losses have been greatest in government, and in ceremonial and property control. The clan in all the Keresan villages still serves the important function of marriage control. Marriage into one's clan is strictly prohibited and violations appear to be non-existent or minimal. Marriage with a member of father's clan is also prohibited, but such unions do occasionally occur without apparent serious social censure.

The following statement about Santa Ana clans by Leslie White (1942a: 157) would seem to apply for the Eastern or Rio Grande Keresans generally (with the possible exception of Zia):

> Clans at Santa Ana are matrilineal, exogamous, "nontotemic," kinship groups. They have nothing to do with ceremonialism (except the one instance of the Siyana clan), nothing to do with officers or societies of the pueblo. There are no clan properties, either in land or in houses. The functions of the clan are: (1) The classification of relatives, (2) The regulation of marriage (in so far as clan exogamy operates), and (3) A certain degree of cooperation, solidarity, sharing of experience, among clansmen. Thus, the clan is a kinship group almost exclusively; its functions in the fields of religion and ceremonialism, and in pueblo political or ceremonial organization, are virtually non-existent.

CONCLUSIONS

The Keresan and Tewa kinship systems reviewed in this paper have been remarkably resistant to change. The Tewa system seems to have changed the least, whereas the Keresan system appears to be adjusting to a bilateral system, rearranging terminology and behavior to equalize the relationship between maternal and paternal relatives. Is this the result of Tewa or Tanoan influences generally or of acculturation to Spanish-Mexican and Anglo-American kinship patterns and usages? It is my belief that the situation is not a case of simple diffusion from other kinship systems encountered by the original Keresan migrants. I feel that the peculiar physical and social factors in the new environment caused the Keresans to make the major adjustments. One might cite the Wittfogel-Goldfrank hypothesis (1943) of the importance of irrigation as a partial explanation, at least, for the decreased importance of clan and lineage among the Keresans. The demand for greater communal effort and central direction necessitated by the need to control water supply and its distribution, particularly the construction of irrigation canals, would necessitate major social adjustments. A clan system of the Western Pueblo type where governmental and ceremonial functions and land control is in the hands of a number of clans loosely integrated into a village would be largely ineffective in an environment that required large-scale cooperation and communal effort to utilize water resources most effectively. One would expect adjustments toward a greater village control and centralization under these circumstances. The Tanoan model where governmental and ceremonial functions are in the hands of one or two important societies whose membership is drawn from the whole village would be the logical group from whom to borrow the institutions that would facilitate tighter control over a larger number of people. In the process, a clan and lineage based kinship system would deteriorate and reorganization would occur along a bilateral model where both maternal and paternal relatives are equalized. This is what actually seems to have happened to the Eastern Keresans.

As an alternate explanation, or perhaps a supporting one, I would propose the importance of Spanish civil and church authorities in the process of Keresan social organizational reorientation. Spanish colonial policy was a deliberate one of forced acculturation. The threat of a loss of native ceremonies and practices through pressure would tend to bring about reorganization in terms of a tighter village control with attendant loss of clan importance. The difficulties generated by Spanish contact brought Keresans and Tanoans into closer association, the Pueblo Rebellion of 1680 and the period until hostilities ended in 1697 is evidence of the close cooperation and intermingling of Rio Grande Pueblo Indians. There was ample opportunity during this period and later for the borrowing of Tanoan cultural patterns evident in the Eastern Keresan Pueblos today. A fact that lends support to this hypothesis and indicates the relatively recent breakdown of Eastern Keresan clan and lineage type kinship system is the Laguna case. Laguna appears to have a kinship system of the Western type with associated importance of clan in government, religion and land control.[5] Yet we know that the people of Laguna were Eastern Keresans until the Pueblo Rebellion of 1680 and that sometime between 1697 and 1698 they founded the pueblo of Laguna some fifty miles west of the Rio Grande. If the deterioration of clan and lineage based kinship system had occurred prior to Spanish contact we would expect Laguna to show greater affinities to the Eastern Keresans. Indeed, Anglo-American acculturative pressures have tended to perpetuate and encourage continuance of strong centralized villages among all of the Rio Grande pueblos.

The important point in our attempt to explain the greater proliferation of Tanoan traits among the Keresans is that contact alone does not bring about borrowing between cultural groups. There must be a functional need for the borrowing of traits and complexes. In the case of the Rio Grande pueblos, the Keresans had a more pressing need to borrow, either because of an adjustment to a new physical environment or to offset Spanish and Anglo-American acculturative pressures. It is reasonable, in view of these circumstances, that they borrowed heavily from Tanoans whose social organization was more adapted to the physical environment and for resistance to outside pressure on native ceremonial life.

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Notes

1. Grateful acknowledgment for assistance in obtaining materials in the field for this paper is made to the Social Science Research Council's Southwestern Project on Comparative Psycholinguistics and to the Graduate School, Northwestern University.
2. Compiled from the writer's ethnographic notes.
3. Summarized from the writer's published and unpublished materials. See, for example, Dozier 1955.
4. The information on Keresan kinship system presented in this paper has been obtained from Parsons 1923, 1932; Goldfrank 1927; Hawley 1950; White 1932, 1942a, 1942b.
5. That is, before rapid disintegration set in at Laguna during the recent American period.

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SOME REFLECTIONS ON CHIGA ETHICS

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In the past decade there has been a revival of anthropological interest in ethics. I do not propose here to review the accumulating literature which encompasses data from comprehensive value studies, character and personality discussions about the structure of conscience, shame and guilt, and even field reports by philosophers. I shall limit myself to the reexamination of some of my Chiga field data from the perspective of ethical analysis.

Fortunately it is not necessary, in order to do this, to take a stand on the precise identification of moral and ethical phenomena. We have enough experience in anthropology to know that it is both feasible and fruitful to plunge into exploration by focusing on a wide area of interest. Though ethics has long been neglected as a special department for description, partly no doubt because it has no separate institutions of its own, no market place nor church, nothing but an occasional grandfather's lecture, or the teaching at an initiation school, to call its very own, it does have a particular subject-matter. It must obviously deal with approved and disapproved conduct, the rules of prescribed and proscribed behavior, the patterns of virtues and vices, the socially approved ideals, aims and goals—in short, the content of morality. And it is also concerned with its structure, the kinds and situations of appraisal, questions of validation and justification, modes of deliberation and decision, attitudes about the person and about responsibility, analyses of such properties as intensity, centrality, and self-consciousness, the structure of conscience, and so on. I shall, of course, comment on just a few of these elements here.

Anthropologists are especially sensitive to the relations between people's moral precepts and the structure of their society. Indeed, the anthropologist sees morality not as a set of copy-book maxims but as an integral part of the patterning of relations between people. It is particularly interesting for us, therefore, in looking at any people's ethics to examine the structure of the moral community, the classical in-group which counts, morally, and which is often pictured as a moral core within which common human qualities of kindness, sympathy and mutual help are pervasively operative. I should like to suggest that while Chiga morality is, as would be expected, essentially kin-bound, there are certain problems and complexities which appear when we focus sharply on the scope, application and quality of their moral bonds.

Among the Chiga the individual patriarchal household is the basic social unit. The household is, in turn, part of a whole set of paternal lineages, whose cohesion varies with the degree of kinship. The boundaries of different levels, are, however, not fixed. They are subject to fissioning at any level, and for varying reasons. There is no authority to assure cohesion or determine lines of fission, no chiefs or formal positions of power, no absolute subordination outside the patriarchal household.

The obligations, ideals and virtues that the Chiga would explicitly state as good—if pushed to such unaccustomed reflection—would mirror these relations tidily. A man must take vengeance for his brother's murder. He should take care of his own family, and help his brothers, within reason, lending a hand with the milking if one has a sore arm, sharing ointment to wear to a feast. But he need not feel obligated to lend a cow for a bride-price payment; that is beyond the call of routine brotherly duty. He should normally go to his brothers' aid if they are attacked, but he need not go along on a raiding party. It is good to be brave in fighting (as the Chiga reckon bravery, which means countenancing some pretty treacherous, sharp practices), to be quick to resent an injury, but not to be ungovernably hot-tempered. A man should order his life so as to be beholden to no man, so that, even without riches, he will not be any man's serf.

A woman should function in the framework of subordination to her husband. She should be hard-working, modest, and reasonably clean. She should be respectful and obedient to her husband and her mother-in-law, and she should not commit wanton adultery—that is, she should not bestow her extra-marital favors beyond the respectable bounds of place and person (never, that is, beyond her husband's brothers, and only in the house, never brazenly in the open fields). Children must assume adult work responsibilities gradually, but with a good will. They must obey their parents in all important matters, especially marriage arrangements. And they must accept junior status as long as their parents are alive.

On the whole, within this system, the classical distinction between in- and out-group morality holds for the patrilineage. Obligations of mutual help hold between brothers; they cannot apply in the same sense to strangers, or even more distant kinsmen. Fratricide is a terrible offense, punishable by death at the hands of the close kin group, but murder outside the lineage is a vaunted act, and if it falls within the pattern of the vengeance feud, a required one. However, there is a very interesting borderline area of problems created by the shifting boundaries of the kin group itself, which suggest that moral factors enter over and beyond the limits set by absolute bonds of kinship. Take, for example, the case of Kabaka, who had no living close agnatic kinsmen. He belonged to a common large lineage, but not the same segment of it as any of his neighbors. Repeatedly I was told that "in the old days" (before the *pax britannica*) Kabaka could not have lived safely as he did, for some one of his neighbors would have been bold enough and brash enough to have taken advantage of his unsheltered and unprotected position to steal his cattle, perhaps even to kill him. No one would have been obligated to rush to his defense; indeed, they could not easily have done so, if they were closer kin to the attacker. However, that is not to say that the attack would have been considered right, or even morally neutral. People did not speak of it with approval, as something they would themselves have done. And indeed, they indicated they would have been concerned for Kabaka, and might, if possible, have warned him, or even given him protection and shelter. There was obviously some area here of possible decision, of moral evaluation and equivocation, in which questions of justice, of personal friendship—and no doubt of expediency—as well as just questions of kinship, could and did enter. Such considerations would also weigh in persuading a group of kinsmen to accept compensation and a scapegoat ritual in place of taking blood vengeance for their brother's death, if the offender was of a distant lineage of the same clan.

We must, furthermore, note that there is some recognition of moral factors even beyond any kin limits. These may have been rare, but they were within the bounds of what was considered possible. For one thing, there is a quite explicit requirement that any case of inter-group killing, to be praiseworthy, must somehow be formulated in terms of the vengeance feud, and not just be an unprovoked act of aggression. In the latter kind of case, a brother might fail to get his brothers' support in case of reprisals. Similarly, an own brother, guilty of theft or failure to pay his debts, was sometimes not supported when outsiders came to claim their property, even by force. This implies some minimal recognition of out-group rights, and moral condemnation of the brother's conduct, though directed outside the kin group. On the same basis, men might refuse to come to free their brother, caught as a thief and held for possible ransom.

In short, the moral community does extend, though in sharply descending order of importance, beyond the immediate lineage segment, to the wider extensions of the lineage, and even to some extent beyond it. This is, perhaps, related to the structural fact that while there are no *group* relations beyond the lineage, there is for each individual a nexus of bonds though his own affinal and uterine kin ties which stretches beyond his patrilineage and sometimes creates for him dilemmas of divided loyalties.

There is another kind of dilemma which is posed by the priority of in-group solidarity. If one should support one's brother irrespective of the quality of his action *outside* the group (a principle which we have seen is to some extent limited but which does have considerable operative effect), how about offenses committed within it? The Chiga do not push the principle of collective responsibility to the point of paralysis which has sometimes been cited elsewhere, where fraticide goes unpunished because the offended group is at the same time the kin group of the offender. However, to a somewhat lesser degree, the quality of moral bonds and the strength of moral sanctions within the group *are* definitely affected. In fact, those informal sanctions so often referred to as though they could just be taken for granted as mechanisms supporting good moral behavior within a moral community do not appear to operate at all in many contexts here. A man who is a thief or a bully is none the less entitled to support and to formal participation in group activities. Though he may be privately criticized and even when possible avoided, he must still play the appropriate role in weddings, or beer drinks, and so forth. This is true even when his offenses are serious ones. A man may without fear of ostracism or any real denial of rights and privileges violate many of his obligations. He may use cattle held in trust for a younger sibling in order to get himself a wife, or may beat his son and force him out of his home. Public opinion is perfectly clear in such cases. These men are wrong, they are wicked evil-doers. There is even a technique for crystallizing this public opinion formally, for the matter may be brought to a kind of trial by the wronged individuals. However, such a trial has no authority-status at all; it is advisory, and helps toward the clarification of issues if there are any. But the malefactor does not need to accept the verdict, or to act upon it. The moral assessment is there, but there are no teeth to it. Men—Chiga men—are notoriously impervious to mere criticism, however well founded, or even to ridicule; and there are no overt sanctions of a more pressing sort. (For women the situation is somewhat different. A woman may be punished by her husband or father; and women are more thin-skinned, and concerned about their good name.) I do not know just how far the limits of this tolerance for deviance in social behavior would go, but it is clear that a man may violate many of the basic requirements of appropriate behavior and even of kin solidarity without losing its reciprocal benefits.

In such a situation of minimal social sanctions we might expect there to be a rich development of supernatural or religious ones; ancestor worship would seem to lend itself ideally to this. However, Chiga ghosts are as little concerned with the moral quality of their descendants' behavior as are their living kinsmen. They are maliciously, wantonly, evil, the source of dangers and mishaps, not of good. The only point at which such a ghost intervenes in social action is when his own interests are involved. So a brother's ghost may haunt his closest living kinsman until vengeance has been taken for his death. Or a ghost may send a dream, not to remind a man of his moral obligations, but to sanction his violating them; for example, the ghost of a man's father's sister may demand in a dream that he marry her daughter, a marriage which is ordinarily severely frowned upon. (This does not, incidentally, by any means imply that the girl's father will submit to this supernatural blackmail and arrange the misalliance.) But otherwise, not incest, nor murder, not any aspect of required respect or mutual help enters into men's relations with their ancestors. And other spirits are even less involved. Ghosts and gods therefore do not sanction or support morally, nor are they involved in any explanatory or justificatory way.

There is of course another possible kind of involvement of religion with basic ethics. Religious ritual is in itself, as we know, often supportive of group solidarity, and a seniority system operative in the performance of rites is also an obvious support to a system of authority relations. Neither of these is important for the Chiga. The worship of ancestors is subject to the same kind of fissioning as is the relationship between living lineage members. A man sets up his own ghost huts when a diviner orders him to do so; he need not, nor does he typically, remain dependent on his older brother for his religious ministrations. In any case, Chiga ghosts are powerless beyond the third generation, so any bonds they might create would be thus limited in any case.

Although ancestral spirits are not directly involved in sanctions relating to moral behavior, supernatural forces do enter through mechanisms of human intervention. Oaths and curses play an important role. A son's obedience to his father is not supported by direct supernatural intervention at any level, but there is the threat of a curse which an outraged father may lay upon his head. Uttered at his deathbed, a father's curse, "May all his belongings go up in smoke," will not only plague his son with direct misfortune, but will also serve to ostracize him, for the effects of such a curse are contagious. They will affect anyone who thereafter associates with him in helpful friendship. And the Chiga are themselves convinced that the relationship between a man and his pact brother, with whom he has sworn a typical East African oath of bloodbrotherhood, is stronger than the bond between brothers. For brothers, despite lineage solidarity principles, may cheat each other. "Do I sit at my brother's heart?" a man will say, expressing his profound distrust of any man's deeper motives. "If your own hyena eats you, he will at least pile up your bones," they may add, underlining their conviction that formal cohesion and respect for surface obligations and mutual responsibilities may go along with real treachery. The pact brotherhood, however, is supported by more than moral sanctions, approved principles of goodness and badness. It is supported by the threat that "the little red one," the blood which you have swallowed, will actually swell up and burst within you if you should break your pact in any way. That such a threat is felt to be more effective than brotherly friendship or sympathy is an interesting comment by the Chiga themselves on the weakness of the purely moral bonds of in-group solidarity.

There is a further point on which I would have liked to comment. The Chiga view of morality is obviously a highly prudential one. They tend to look at it from the point of view of the possible transgressor: what is to make the man who wants to do wrong behave himself? Such a view, in any system, moral or legal, by its very formulation emphasizes the role of sanctions. It does not really follow that the essence of moral considerations for the average man or woman among the Chiga is really so prudential. For while they do not trust their brothers to behave, they do, for the most part, accept for themselves obligations that their system imposes. This is somehow achieved without any explicit stress or moral education; there are no initiation schools, no formal lectures to the young, no disciplinary punishment. Moral comments are made, and heard by the children, in contexts of discussion of concrete actual situations that have occurred. They are not directed *to* the children particularly. Moral qualities are viewed as natural traits, not particularly affected by training and experience. It would be interesting to pursue this question further—what are the processes by which conformity is established and standards accepted in a society which has such a high tolerance for aberrance, so little overt stress on morality and personal responsibility, and no organized moral training and discipline? I do not in fact have the answers, as this was not the focus of interest in my own field work. However, it seems to me that if we are interested in the comparative phenomenology of moral experience, or in the functional role of moral attitudes and moral sanctions, it would be particularly rewarding to pursue the examination of such cases as this in which morality is peripheral and minimal in its overt formulation.

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SEQUENCE AND STRUCTURE IN FOLKTALES

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This paper reports on an investigation of some gross structural differences between folktales from two genetically related Micronesian cultures, Truk and Ponape, and evaluates the socio-psychological meaning of these differences. The tales analyzed here were collected in the field in the years 1949–1953, while the structural analysis is an extension of certain ideas previously set forth in my doctoral thesis.[1]

By the gross structure of folktales I mean the series of logical relations between major segments of the tale text—from, as a minimum, relations between whole sentences to those between sentence groups, episodes, and the major divisions of the tale. This gross structure is distinguished from the levels of structure which linguists customarily study by its greater inclusiveness. The sentence, which is the maximum whole customarily analyzed by linguists, is here treated instead as the minimum *component.*

While the study of gross structure of folktales or other types of discourse in a sense utilizes the same material as customary linguistic analysis—that is, both utilize recorded texts consisting ultimately of sentences, words, phonemes—the gross structure of the tale has no logically necessary connection with structure on the less inclusive syntactic and phonemic levels. The absence of such a relationship to syntax, etc. is demonstrated by the fact that if a tale—or any other text—is translated sentence for sentence from any one language into any other, the structure of the individual sentences of the text must necessarily change, while the gross structure of the whole tale or other text, i.e., the relations among sentences and sentence groups, remains unaltered.

Having defined the subject of investigation we may now ask, how and why does gross discourse structure vary from one text to another? One obvious answer is in terms of the type of discourse itself. Entirely apart from the undisputed differences of content, one would expect to find different sorts of gross structure in, say, a magic spell, a discussion of work plans, and a conversation of lovers.

But my purpose here is to relate differences in gross text structure to general differences between the two cultures from which the texts under consideration come. For this purpose differences attributable to type of discourse are of little relevance, and a comparison of the cultures with respect to a single type of discourse is sufficient, at least for exploratory purposes. Folktales appear to be highly suited to such a comparison, as they are similar enough across cultures to facilitate comparison, while they are stereotyped enough to hope for intra-cultural stability and consistent cross-cultural differences.

In examining the tale texts from Truk and Ponape it readily became apparent that there was considerable structural variation among the folktales of either culture taken by itself. This is hardly surprising, since part of the interest of a tale comes from its capacity to surprise the audience, and this would be lessened if each tale were cut to the same pattern. Another fact which became apparent, and one not surprising to students of the folktale of other cultures, is that tales with very similar gross structure exist in both cultures.

But these observations do not rule out the possibility that, in spite of the wide range of structural variations among the tales in a given culture, there may nevertheless be a greater frequency of certain structures among the tales of some cultures than others. The investigation reported here has in fact led me to the conclusion that there is such a difference between the body of folktales of Truk and Ponape; namely, that Trukese tales develop more by series of repetitive episodes, constituting a "theme and variations," while Ponapean tales develop more by pairs of opposed episodes, in which the outcome of the second is the negative of the outcome of the first, e.g., the hero makes two attempts, the first failing and the second succeeding.

For a manageable measure of these structural differences a comparison was made of the gross structure of all pairs of cognate tales in my collection of several dozen from each culture. Five cognate pairs were found and insofar as they showed structural differences they were for each tale pair in sum in the directions stated above: the Trukese versions developed more by series of repetitive episodes; the Ponapean, by pairs of opposed episodes.

Both of these contrasts may be illustrated in a pair of cognate tales which may be referred to as "The Boy and the Birds." After illustrating the contrasts in some detail with respect to this one pair, I shall present summary information on all five pairs.

Both the Trukese and Ponapean versions of this tale are concerned with a boy who is mistreated by one or both of his parents and flees home in anger. He encounters some birds, who help him escape by giving him feathers, making his arms into wings. The parent(s) pursue, offering choice food, but the boy flies off with the birds and the parents fail to retrieve him.

The greatest structural contrast between the Trukese and Ponapean versions of this tale is with respect to repetition. In the Trukese version there are five repetitive segments, constituting about two-fifths of the bulk of the tale, describing the father's pursuit and vain attempts to placate his fleeing son with choice food, and the son's alternate alighting and flying off with the birds. In each of these segments the conversation and actions of the father, son, and birds are given in fair detail, and the exact location named.

In the Ponapean version a similar series of repetitive episodes is assumed by the narrator, but the episodes are not described separately in distinct segments of the text, nor is it even clear how many such episodes are supposed to have actually occurred. All we have in the Ponapean version of this portion are general iterative statements on the order of "The boy and the birds would alight farther on; the mother would pursue them, offering her son the food. As she approached the boy would fly off again." Consequently, this portion of the Ponapean version is much shorter than the corresponding portion of the Trukese version.

But the differences between the Trukese and Ponapean versions of the "Boy and the Birds" tale do not result in all segments of the Ponapean version being more abbreviated. However, where the Ponapean version is more extended than the Trukese, it has been expanded in a different way, namely, by opposition rather than repetition.

This is illustrated by the portion where the boy acquires his feathers. In the Trukese version the birds conspire to give the boy feathers and thus lure him away from his parents, while the boy is an unsuspecting dupe and is readily deceived by the plot. In the longer corresponding portion of the Ponapean version, besides a content difference, there is a structural elaboration which is lacking in the Trukese, consisting of a double pattern of attempt-failure followed by an opposing attempt-success. Thus the Ponapean hero approaches the birds to ask their help and they first retreat in fear, but then, as he persists, the birds change their minds and agree to give him feathers. Again, once the birds have agreed to assist him, he is first interrupted by his sister and must temporarily give up putting on feathers, but he then manages to escape her and resume the feathering, this time successfully.

Structural differences analogous to these may be found in each of the other pairs of cognate tales investigated. Rather than describe these each in detail I have devised indices of repetition and opposition which enable numerical comparison of the strength of these two structural principles in the tales. The index of gross structural repetition, which I shall call *R*, is the product of two ratios: (1) the ratio of all segments involved in repetition (including here *both* the initial statement and its repetitions) to the non-repetitive remainder of the tale and (2) within the segments involved in repetition, the ratio of all later segments which constitute repetitions or variations of an earlier segment, "theme," or "model," to that earlier segment or model. If we let r represent the total segment involved in repetition and $\bar{r}$ the remainder, and let A_1 represent the "model" (or model*s*, if there is more than one series) and let $A_2 \cdots A_n$ represent its variations we may write the following formula for the repetition index:

$$R = \frac{r}{\bar{r}} \times \frac{A_2 + \cdots A_n}{A_1}$$

The index of opposition, *O*, may again be regarded as the product of two ratios: (1) the ratio of the total segment involved in opposition, o, to the remainder of the text, $\bar{o}$, and (2) the ratio of the product of the initial statement in opposition, A, times its opposing segment, $\bar{A}$, to the product of the total segment involved in opposition, $\bar{o}$, times the remainder of the text, $\bar{o}$. We may then write the formula for the opposition index as: $O = \frac{o}{\bar{o}} \times \frac{A \times \bar{A}}{o \times \bar{o}}$, which reduces to $\frac{A \times \bar{A}}{\bar{o}^2}$. In the formulae for both *R* and *O* length of segments is measured in number of sentences.

The computation of these indices for the five cognate pairs of tales yields the following results:

	R		*O*	
Tale	Truk	Ponape	Truk	Ponape
I	3.01	.67	.0004	.05
II	2.31	.38	.07	4.17
III	.34	.19	.0009	.015
IV	7.68	.00	.01	14.67
V	18.26	.03	.002	.07

TABLE: Values of *R* and *O* for five tales, Truk and Ponape.

It will be noted that for both for indices, *R* and *O*, there is a considerable range within each culture, and some overlap between the ranges of the two cultures, even with such a small sample. However, for each cognate pair of tales the difference is always in the same direction, Truk being higher with respect to repetition and Ponape with respect to opposition. Because of difficulties in the actual application of these formulae I do not want to attribute too much significance to these precise figures, but since most of the tales show such marked differences between the two cultures, we may allow for quite a large margin of error without affecting the direction of the differences.

These structural differences between the versions of cognate tales from the two cultures appear to me large enough and consistent enough to conclude that, whatever the prototypical tales were like before Truk and Ponape became culturally differentiated, since that time there has been a preference on the part of Ponapean audiences for preserving or introducing pairs of opposed episodes, while there has been a preference on the part of Trukese audiences for preserving or introducing repetitive episodes.

What do these preferences mean psychologically? What sort of people would prefer tales developing by opposition and what sort by repetition? An emphasis on development by opposition (e.g., the pairing of a failure and a success) joined to a de-emphasis on repetition, as is characteristic of Ponape, suggests a people who are stimulated to greater effort by an initial failure, who are persistent in the face of difficulties and are interested in individual achievement. The relative lack of repetition in Ponapean tales once a goal is achieved suggests a people who are self-confident and lack patience with a fictional hero who must keep doing the same thing over and over again with minor variations, as if not sure of his initial success.

On the other hand, an emphasis on development by repetition together with a de-emphasis on development by opposition, as is characteristic of Trukese tales, suggest a people who feel uncertain of the ability of their fictional heroes to accomplish things definitively, and who seek reassurance by returning to essentially the same event again and again. The relative lack of opposed episode pairs suggests a readiness to accept defeat at the hands of others, or, in more positive terms, a willingness to yield and cooperate with others. People like this would expect that if they are to succeed in something they will be able to do so without unduly prolonged individual effort, and that if they cannot succeed in this fashion they may as well give up.

These implications from folktale structure as to certain predominant personality traits of Trukese and Ponapeans are further confirmed by a consideration of the social and political organization of the two societies. In Truk, the typical individual belongs to a fairly large cooperative extended family and matrilineage. Distinguishable statuses within the society are few compared to Ponape, and such as there are are mostly ascribed automatically according to chronological age and family membership. In such a society one would expect the people to be unambitious, hesitant about the expression of aggression, and ready to yield to the other members of the kin group.

In Ponape, while the typical individual also belongs to an extended family and matrilineage, these kin groups are more fragmented and less cooperative than the corresponding groups in Truk. There are numerous statuses distinguished in Ponapean political organization. These consist of series of carefully ranked political titles and there is a wide range of prestige from highest to lowest. Moreover, assignment of titles on Ponape is by no means automatic. While hereditary seniority plays some part, the most significant factor is individual achievement. Almost anyone, no matter how low or how high, can hope to rise further, chiefly by exerting himself in contributing to public feasts. In such a society one would expect the people to be ambitious, persistent in achieving goals in the face of adversity, and self-confident enough to pursue their own interests even when opposed by others and to accept personal success promptly when they achieve it. These characterizations of Trukese and Ponapean personality are, I believe, consistent with actual psychological and behavioral differences between the people of the two cultures.

In conclusion, I would note that there are a number of other structural variables which may be distinguished in folktales besides these which I have handled here, such as the sequence and proportion of generalizing and particularizing segments, or temporal and atemporal segments, the insertion of lesser episodes within greater in the form of quoted reports of the principal characters, etc. The content of folktales also offers a rich field for study and psychological interpretation, of course.

This study, I hope, illustrates the promise of the structural analysis of sequences in folktales as one means of making psychological inferences about societies which may be correlated with social structure and other aspects of culture. Folktales, compared to the psychological tests which have often been used for this purpose, seem at no disadvantage with regard to stability, representativeness, or interpretability. Moreover, collections of folktales are available for a larger number of the world's cultures, including some which have become extinct or drastically changed and are no longer subject to direct observation.

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Notes

1. Fischer, J. L., "Language and Folktale in Truk and Ponape: a Study in Cultural Integration." Doctoral Thesis in Social Anthropology, Department of Social Relations, Harvard University, 1955 (typescript).

*

CIRCUMPOLAR FOREST NORTH AMERICA AS A MODERN CULTURE AREA

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My objects in this paper are, first, to call attention to some aspects of contemporary cultural configurations in the circumpolar forest of North America and, second, to apply ethnological theory to the understanding of that culture in synchronic terms. The paper grew out of the realization that, in contrast to contemporary Mestizo America or South Asia, the northern forest has largely been neglected as a significant culture area of the modern world.

You will note that I have no intention of confining anthropology to the study of native people. I assume that anthropologists are not confined to studying cultures that are small in scale, remote from the civilization of Europe and America, and primitive. On the contrary, I take anthropology to be the study of culture wherever culture may be found, regardless of type of social unit—whether it be a military base or a native village. Unfortunately, outside of a few books like Robert Marshall's *Arctic Village*, or personal memoirs, the culture of the white man in the North has received practically no attention. This consideration will force most of my remarks to be confined to the Indian population.

I

I have used just the plural word "configurations" in referring to culture in the northern forest. No single, homogeneous culture spans northern Canada and interior Alaska. I do not only refer to tribal differences. The major configurations of culture to which I refer exist throughout the northern forest. They are two in number and are attached to two relatively distinct ethnic categories: the indigenous Indians and the largely immigrant whites, whom I shall also call Euroamericans. In other words, despite the long history of acculturation and the profound degree of assimilation that has occurred on the part of the Indians, two types of culture may still be distinguished practically throughout the circumpolar zone. It is likely that the most recent, rapid migrations of Euroamericans into the northern forest to build radar stations, to mine gold, uranium, and iron, or to man air fields have actually widened, rather than narrowed, the gap between these two cultural configurations.

II

To speak of a primary distinction between the cultures of the Indians and Euroamericans in the northern forest is not to suggest that each of these configurations is equally homogeneous. It is only possible to speak of a *single* white culture if one is content to make a few, rather gross, contrasts between that and the Indian way of life. Obviously, a mining camp in Quebec and another in the Northwest Territories will have far more in common than they will share with the group which operates a radar station. The military air fields again have a distinctive way of life that contrasts with missionary culture.

The homogeneity of the Indian culture, on the other hand, is far greater. Many, if not most, elements of the contemporary Indian way of life transcend the linguistic barrier separating the Athapaskan and Algonkian languages. To switch to a historical outlook for a moment, this transboreal homogeneity does not date only from the period of contact with Europeans. In other words, it is not the end-product of acculturation but can even be discerned on the pre-contact dateline. Clark Wissler's culture-area scheme ignores the unity of the northern forest. The homogeneity of the Indians is furthermore present not only on the cultural level but is apparent on the personality level as well. The characteristics of Ojibwa personality which Hallowell found in Manitoba and which turned up in Wisconsin were observed in sixteenth century Quebec and Ontario and are also clearly apparent among certain Indians of interior British-Columbia.

III

The geographical situation of the northern forest is closely related to elements in both Indian and Euroamerican cultures. This can hardly be surprising since culture is, at least in part, a means of adapting to environmental problems. What is more significant is the manner in which the white immigrants have used their culture to emancipate themselves from immediate dependence on, or contact with, the environment. This they have managed to do by drawing heavily upon the resources of the larger society in which they freely interact. We will further consider the significance of such large-scale interaction in a moment.

Despite recent, rapid influx of people into the North, the density of population remains low, averaging in the forest, perhaps, around one person per hundred square kilometers (or about one person in every forty square miles). Such low density also conditions elements in the culture configurations of Euroamericans and Indians. For example, occupational specialization in the North remains limited as far as a purely northern market is concerned. True, many workers are specialists but they specialize in terms of an occupational role in the larger society, one that includes Canada and the United States. Miners, soldiers, and Indian trappers are engaged in producing surpluses or rendering services for the larger society in return for which they receive surpluses of food, clothing, fuel oil, other manufactured goods, and even draw on educational, medical, and other services provided by the so-called "outside." Because the Euroamericans are better able to avail themselves most richly of these imported goods and services they are also in a position to emancipate themselves from intimate contact with a rather harsh environment.

IV

What do people in the contemporary circumpolar forest do, how are they organized, and what do they think?

For the Indians, fur trapping constitutes the central economic activity, one which has largely replaced hunting. Trapping has been instrumental in breaking down the Indian's former isolation. Furs allow him some degree of participation in the larger society. Just as the missionary mediates the Indian's relations with

God so the trader mediates many of the Indian's societal relations. The trader is usually a white man employed by a large organization to issue food and equipment in return for furs. Life is profoundly influenced by the cycle of the fur trapper, which requires the Indian family to leave the trading post in autumn to enter the forest trapping grounds. Usually the group does not return until the following spring. Trading, day school education, health facilities, and administrative services are practically paralyzed in the cold months while most of native population remains dispersed in the bush. However, a phenomenon called "posting" has emerged as an alternative to wintering on the trapping grounds. Posting means the tendency for a family to remain around the trading post. They avoid the expense, rigor, and uncertainties of a winter in the bush. Posting threatens the fur trade on which the trading organizations are dependent and also interferes with the natives' consumption of fresh meat and fish. The new custom imposes welfare obligations on storekeepers and administrators which would otherwise be borne by the Indians. As near as can be ascertained, posting has been encouraged, first, by depleted natural resources in some areas; second, by the anxieties and discomforts of life in the bush; and, third, by new resources which were lacking in aboriginal times (houses). The depletion of natural resources in Canada has been of serious administrative concern. One solution, the success of which remains to be studied carefully, has been large-scale fur farming. In Quebec, for example, beaver preserves containing from 7,000 to 17,000 square miles have been opened and stocked. Live beaver are transported to these territories and allowed to multiply, their trapping being regulated. In some places provision has even been made to refrigerate beaver meat for consumption during the following summer.

Now let us look briefly on the Euroamerican's occupations. Some are also engaged in trapping. In this status they often follow a style of life close to that of their Indian neighbor. The white trapper is mobile, dependent on dog-team transportation, lives in a log cabin, and hauls his own wood and water. In contrast, the Euroamerican *trader* and *missionary* often possess many more amenities. For example, the trader builds his house out of imported lumber and insulation; he secures electricity from an oil generator; and his bathroom includes running water. When he travels he makes increasing use of the airplane and snowmobile. Obviously such a way of life would not be possible without intense dependence on the resources of the larger society.

My remarks about social organization will be confined to the trading-post community. Often this small settlement contains less than 200 people and includes one or more stores, a mission with a day school, and some forty tents or cabins which house the summer Indian population. Despite the segmentation of the trading-post village into two ethnic divisions—Euroamericans and Indians—the unit represents a true corporate group. Interaction, however, is rather narrowly confined to such channels as trade, labor, schooling, religion, administration, and health.

Formal organization is slight, taking the trading-post village as a whole. It is also slight within the Indian segment of that community. Government chiefs have little power. The Hudson's Bay Company manager or the resident priest probably has far more influence in native affairs than any individual Indian. Kinship ties continue to be of considerable importance among both the Athapaskan- and Algonkian-speaking tribes but are based on bilateral descent and a small noncorporate kindred. The nuclear family possesses a very large degree of economic independence. This unit is expected to secure its own capital goods, cooperates as a team for the exploitation of the natural environment, and supplies most of the affection and security desired by its members.

The interaction of whites and Indians in the trading-post community is maintained through a number of complementary sentiments. For example, both segments are induced to interact in trade, clinics, or church because each values what the other is culturally equipped to give. The Indian traps and sells his fur because he is attracted to the resources of the store. The trader would hardly remain in the North without the profit he secures through buying furs. The missionary is a teacher and ritualist on whom many Indians have equally come to depend. Fear of illness motivates many Indians to appear for medical examinations and preventative inoculations. Both the Indian and Euroamerican agree that in many respects the white man's culture is superior to the Indians'. In the light of this belief it is not surprising that both revivalistic or perpetuative nativism remain absent among the Indians. Nor to my knowledge have movements in the line of the Melanesian cargo cults—so-called reformative nativism or anticipatory assimilation—made any appearance in the northern forest.

Tension between the Indians and Euroamericans in a trading-post village also exists and can often be traced to the uncomplementary orientations and sentiments which each side maintains. For example, the Euroamericans are generally conscious of their participation in a long stream of history. They are aware of belonging to a widely extending society, one that includes nearly the whole globe. In a word, they are much larger in scale than the Indians, who possess little consciousness of their historical position and are only vaguely aware of other Indians living in other parts of North America. The white man, at least ideally, recognizes the limited autonomy which he enjoys in many of his societal relationships. He understands that prices are determined by impersonal market conditions. Administrative decisions are apt to be made by many men acting on large amounts of information. The Indian, on the other hand, is apt to see prices, the supply of goods, and administrative policy originating with the concrete, visible manager, priest, or Indian superintendent. "Government" for him remains a nebulous concept without vital significance.

V

I would now like to offer some general observations about the two cultural configurations which exist in the boreal forest.

The northern forest reveals a situation of ongoing acculturation in which the dominant tide of diffusion is from Euroamerican to Indian culture. A kind of symbiosis has appeared in both cultures. It is hardly conceivable that this tide—which began over 200 years ago—has come to an end or that it is even slowing down. The tide has always been slow and gradual, no doubt because of the relative marginality of the region. Recent indications suggest a speeding up of diffusion. Military necessities, combined with the development of aircraft, have greatly reduced the isolation of the northern forest. Perhaps such a speeding up of diffusion will bring a sharper break in the continuity which the Indians have so far managed to preserve between the aboriginal and modern cultures.

Many elements in the Euroamerican circumpolar forest culture of trader, trapper, and missionary reflect specialized adaptation to the northern situation. White culture in the North presents a paradox of intense dependence on outside society and relatively great self-sufficiency. With all his amenities, the white man's survival still depends on many decisions he must make and carry out nearly independently—like seeing that sufficient wood is on hand for the long, cold winter. Physicians, repairmen, or policemen are not always available when there is a crisis. The machine shop is a crucial element in a northern mission, one needed to make repairs when the generator or pump breaks down. The dairy barn and hay field permit continuance of a customary diet and reduce the high freight charges incurred in importing foods. The canoe furnishes access to the sole summer highways of the country. In this setting of self-sufficiency invention is encouraged, even if it is only of the transitory, hay-wire variety that never makes a man famous or brings him royalties. Increasing use of two-way radios, snowmobiles, x-ray machines, and other forms of highly complex equipment are being accompanied by increased outside maintenance, costly as such maintenance will be. In this way the growing technological development of the North, at the same time that it further emancipates man from his environment, promises also to reduce his autonomy and self-sufficiency.

Behavior in the circumpolar forest may still be described as loosely structured. By definition, loose structure means that behavior lacks extreme routinization; people are individualistic; norms are tolerant; even norm violations may be tolerated. The character of loose structuring is most evident in Indian culture. But even for Euroamericans, at least outside of military stations, life possesses a less hurried and compulsive flavor than it does in our cities. True, the trader also observes certain routines, but he is largely his own boss in day-to-day matters. He can arrange his work and dress to suit personal preferences. Among the Indians a fairly wide range of alternative channels of conformity remains acceptable although the degree to which they condone actual violation of normative behavior varies. Extremely great tolerance for violation exists in some places, for example, among the Kaska Indians of northwestern British Columbia. These people exert little pressure on one another and the official police force of the large society is circumvented by everyone. The expression "It's up to you," used in a situation where a margin of indecision exists, expresses the degree to which it is accepted that many norms will not bind another person. Only a fraction of such tolerance appears among the Cree Indians of Attawapiskat, Ontario.

VI

A conclusion may not be the most appropriate place to emphasize the relative poverty of ethnographic materials from which this picture of modern circumpolar North America has been derived. Perhaps the rapidly declining isolation of the North promises an early change in this condition.

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MEDICINAL PLANT AND FOOD USE AS RELATED TO HEALTH AND DISEASE IN COASTAL OAXACA

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This paper describes a generalized theory of health, disease treatment and prevention now in current practice among Afro- and Euromestizos and Mixtec Indians in the Pacific coast of the state of Oaxaca, Mexico. This theory is based upon relationships of qualities assigned by them to foods, plant medicines and body conditions. Description of the theoretical system, how it is used, and the rationale behind its use, along with specific attention to the plants and attitudes toward plants involved are drawn from field materials from this area.[1]

Many investigators in Middle American in the course of ethnographic studies have reported a dichotomous classification of foods based on qualities referred to as *caliente* and *frio* (hot and cold). In this classification, emphasis is placed upon the antagonistic effects upon the body, depending upon the condition of the organism. As Foster and Rowe (1951) have pointed out little attention has been paid to the relation of this classification to traditional medicine, particularly on the use of plant remedies which are also classified as hot and cold.

Foster (1952) relates the use of hot and cold and other qualities in medicine and food practices in Latin America with the classical Hippocratic system and succinctly traces this system. It will be recalled that the system as propounded by Hippocrates and later by Galen depends upon four bodily humors: blood, phlegm, yellow, and black bile, each of which is characterized by two qualities associated with the four elements of nature: fire, earth, water, and vapor. Each humor is, therefore, appropriately cold or hot, wet or dry. As Foster states:

> The proper balance of these humors resulted in health; imbalance produced illness, which could logically be characterized by abnormal cold or heat and dryness or moistness. This concept with subsequent modifications and elaborations, reached Spain and Western Europe via the Arab World. It was transmitted to Hispanic America after the conquest where it remained the basis of medical classification and teaching until the 18th century. Selected aspects of this theory—particularly the concept of heat and cold as qualities of the body, of types of illness, and of foods and herbs—became part of the folk belief of most peoples [of Hispanic America] . . .[2]

The particular area of the *Mixteca Baja* from which material for this paper is drawn is in the *tierra caliente*, hot lands, of the Pacific coast. This part of the coast, known as the *costa chica* of Oaxaca, is included in the political district of Jamiltepec. In this region there are two commercial centers, Jamiltepec, with a population of about three thousand, and Pinotepa Nacional, with over six thousand, dominated by a *mestizo* or *ladino* population. These towns are located between the coastal plain and the foothills of the Sierra Madre del Sur. The lowland Mixtecs who live for the most part in their own villages in the foothills are numerically predominant in the population. A third group of Negro Mexicans, much mixed biologically and descended from African slaves brought to Mexico during the Colonial period, live in lowland villages on the coastal plain. This latter group constitutes a peasant class and shares language and culture of the Euromestizos, who are dominant economically and socially in the region. Except for the large commercial towns, the three groups can be said to live in distinct villages.

The area studied extends from sea level to about four hundred meters in altitude. Temperatures are seldom below 70°F., and about forty inches of rain fall from the end of May until November. The dry season occupies the remaining months, and there is a short dry period during part of July and August.

Shifting agriculture and grazing have left little of the climax vegetation undisturbed. In the hills from about 200–400 meters above sea level, broad-leaf evergreen trees like mahogany (*Swietenia macrophylla*), *guapinol* (*Hymenaea courbaril*), and ceiba (*Ceiba pentandra*) are found. This mixed vegetation extends down to 100 meters or less, depending upon soil and other conditions. Between 100 and 200 meters are rolling hills frequently dominated by ceiba and *guamuchil* (*Pithecolobium dulce*) in the lower parts, and near water courses are *parotas* (*Enterolobium cyclocarpum*). Plains here are covered by grass and thorny *Acacia.* Below thirty meters, remnants of the rain forest with large buttress-root, broad-leaf trees, some of which begin their growth as epiphytes in the tall palms, lianas, and communities of low thorny palms, are separated from the ocean beaches by lagoons, swamps and mangrove estuaries.

In most instances *milpa* land is abandoned after two years and not considered fit for reuse until twelve to fifteen years have passed. Thus there is much of the countryside always in herbaceous weeds and other sun-loving invaders which are soon shaded out by low trees of the interrupted climax vegetation. Many of the weeds are of Old World origin.

Despite the difference in habitat of the Indian and non-Indian populations, most of the commonly used medicinal plants are available to all groups, since many of these plants are ubiquitous in the coast in disturbed areas.

The medicinal plant remedies forming the basis for generalizations made here are those known to the common people, many of whom are recognized to have a flair for curing, but are not specialists. The more esoteric practices of *curanderos*, healers, and *brujos*, sorcerers, are known only in fragmentary form by even the better lay-curers. It is the lay-curing with its concept of disease which reflect the hot and cold Hippocratic system to a greater extent than do the practices and concepts of specialists whose work is concerned with diagnosis and treatment of "unnatural" diseases.

Within the Afromestizo group, among those families that depend upon subsistence and cash-crop farming, the men go out to the fields and bush, and both men and women know a great deal about medicinal plants. Among the Euromestizos who monopolize the artisan, merchant and rancher occupations, although herb remedies are still accepted quite as readily, the women are the experts in the location and use of medicinal plants. Many verbal accounts emphasized the failure of doctors and patent medicines as contrasted with successful treatment based on the knowledge of herbs possessed by an old Negro or Mestizo woman. In the large commercial towns, Mestizo merchant women who deal with Mixtec women take it upon themselves to advise in curing matters. In these towns older Mixtec women are occasionally encountered who also perform this function.

Information was obtained in the following way. Medicinal plants were collected in the field with the assistance of informants from all three groups and specially qualified persons were asked about these specimens afterwards. Cultivated medicinal plants, along with food and ornamental plants were collected from house gardens. All information on the classification of foods was obtained within homes on occasions when several family members and neighbors were present and could consult with one another. Highland Mixtec remedies were purchased from itinerant merchants, who were questioned about their use, and these prescriptions were checked with Mestizo informants.

A plant native to *tierra caliente* is not necessarily *caliente*, and by far the greater number of plant remedies were considered *frio*, cold, or *fresco*, cool. This however, may give little indication of the actual ratio of hot to cold plants were a complete species list to be checked off with a hypothetical informant familiar with all the floral components of the region instead of with only the one to two hundred species known to the ordinary informant. This all-knowing hypothetical informant is less farfetched in the eyes of the *costeño*, local inhabitant, than in our own. When asked the use of a plant not known to them, informants would frequently suggest that such a plant is undoubtedly medicinal but that they, "in their ignorance," knew neither the name of the plant nor its use! Usually this would be followed by the observation that all plants are good for something, or that at least there are many useful medicinal plants which are unknown to the people. These same individuals expressed disappointment that teachers in the Federal Rural school system did not teach their children about these plants and their uses.

There is then, from a botanical standpoint, a taxonomic apparatus embracing most plants and based upon qualities of plants which relate to states of the human body. This is a decidedly ethnocentric system, as well as being one ideally conceived by people who believe that more knowledge would support and extend the limits of effectiveness of the system.

Madsen (1954) in San Francisco Tecospa, a Nahuatl town in the valley of Mexico, found a highly elaborate classification of foods and diseases based on the syncretisms of the Hippocratic classification with the *contrario* system, an indigenous set of concepts which he describes as the balanced opposites forming the universe.

The classification which obtains among the three *costa chica* groups appears to be less complex and less inclusive than that of San Francisco Tecospa, but embraces other principles as well. Rather than four degrees of cold, only two are used: *frio* and *fresco*; hot is usually *caliente*, and only rarely *muy caliente*. There is a category of neither hot nor cold, corresponding to *templado*, temperate, and always a residue of foods and plants of unknown quality which are believed to be classifiable, but have not been definitively classified. Unlike San Francisco Tecospa, no attempt is made to order all things of nature into this system. The common, well-known foods and herbs are so ordered, but the limits can be reached and people are not uneasy if unknowns remain.

Adults among all three groups know and rely upon the hot-cold system for classifying foods. This does not mean to say that there is complete agreement within or among the groups on specific plants or foods. However, there is basic agreement that most, if not all foods can be so classified, and that this classification serves as a guide to maintaining the body state in equilibrium—with an excess of neither—and appropriately protected if the body indicates either a hot or a cold state. There is then, a general theory that can be elicited and inferred underlying this usage: when the body is attacked by a hot disease, cold remedies are needed, and *vice versa*. Moreover, since this geographic area is clearly *tierra caliente*, remedies from *tierra fria* are therefore particularly effective. No one suggests that all remedies from *tierra fria* are cold. On the contrary, when collecting these remedies and information about them it became clear that several of these are hot. But there is a general feeling that remedies brought in from the outside, from the cool highlands, are more effective than local ones. If there is no specific knowledge that a product of cold country is hot or cold, it will be said to be cold since "it comes from *tierra fria*, doesn't it?"

All ordinary diseases depend upon the state of the body and are treated with herbs. The most frequently observed symptoms of these illnesses are fevers, usually connected with malaria, digestive disorders, infections and injuries such as wounds, bruises, strains, and sprains.[3] The digestive disorders range from red or white dysentery to *empacho* or constipation. The dysenteries and diarrheas are usually considered hot, *empacho* is said to be cold. Skin irritations, considered *caliente*, caused by poisonous plants are treated with cooling remedies. Local infections at the site of scratches and insect bites or eruptions in the form of boils and other pustulate infections come under the heading of *granos*. Many of the treatments for fevers, considered *caliente*, include bathing the patient's body in an infusion of a cool herb and administering a tea of the same remedy by mouth. Purges and enemas are supplementary treatment for fevers. *Los aires* or *los aigres*, which may be either hot or cold, are common sources of aches, pains, and stomach disorders, including those called *la bilis* and *latido*. Simple indigestion caused by eating an excess of a *caliente* food such as *chile* is neutralized with a lemon drink, *agua fresca de limon* which is *fresca*, cool. Lemon juice is also used to disinfect cuts, scratches and to ease the itching of skin rashes.

Some of the symptoms mentioned above are often diagnosed as part of the syndromes of other illnesses recognized to be "ordinary" but somewhat less frequent in occurrence. These include diseases specific to adults such as *müina*, a physical upset due to anger, and *congoja*, melancholy caused by grief, both considered hot, and *cangrena*, a cold disease caused by proximity to a corpse, and *vergüenza*, shame felt when the object of gossip or ridicule. *Müina*, *congoja* and *cangrena* are contagious to individuals in sensitive states such as the pregnant or recently delivered woman, a woman with wet hair, or anyone who is freshly bathed. *Coraje* is a hot disease to which new-born infants and small children are susceptible, more so when freshly bathed, caught from an adult with *müina* or *congoja* in the vicinity. Both children and adults are subject to *espanto*, soul-loss due to fright. Mild cases of *espanto*, like all the others are treated with herbs, severe cases require the services of a *curandero*.

There is a tendency to order into opposing categories such as the following. For after-the-fact explanations, persons may be described as possessing *sangre dulce* or *sangre fuerte*, weak or strong blood, but for most persons blood remains undescribed. Diseases are divided into those "*que manda Dios*," that is ordinary diseases sent by God, and those caused "*por la mano de gente mala*," such as those induced by witchcraft by malevolent individuals or by *brujos*. Divinely sent diseases can be treated by traditional and sometimes modern medicine, the other only by *curanderos*.

The criteria used for classifying foods into hot and cold groups are said to be "*lo caliente*" including foods which are irritating, rich in oil or lard, or those which cause thirst, and "*lo frio*" consisting of cooling or refreshing foods. Neither class may be eaten in excess without ill effect. Even though the principle involves use of the opposite for relief of either hot or cold body conditions, it is considered dangerous to cool the body by resorting to bathing or drinking refreshing drinks if it is overheated by a fever or certain kinds of work. Bathing under ordinary circumstances, especially bathing the head, may be performed only during the day, not in the late afternoon or night. Apparently this attitude is influenced by the concept of what constitutes labor, since both among Afromestizos and Mixtecs men do not bathe after work in the fields, or a hard day's travel. They may bathe in streams when returning with firewood, or on days when they do not go to the fields. Euromestizo men have occupations other than labor in the field, and thus bathe more frequently. Women bathe at any time of the day, on any day, until late afternoon.

The principle of opposite treatment is also violated by use of certain hot herbs in treatment of *los aires*, usually a hot disease, and one preventative for contracting *los aires* includes a hot herb, *ruda*, in a hot medium, alcohol.

Actual temperature and physical state may enter into designations of quality in that most cooked foods are hotter or less cold than when raw, and ripeness of fruits tends to make them more hot or less cold than when green.

Among classified remedies, in addition to hot and cold, there is also the category of *amargo*, bitter. This small group of plants is not hot, cold, or temperate. The classification actually depends upon the bitter taste of the plant remedies which are used as purges, abortifacients, or as drugs to stimulate uterine contraction in childbirth.

Space does not permit a detailed tabulation of all foods so that the following serves only to indicate the kind of classification in use:

Corn is considered in general to be hot, but when broken down into various stages of preparation and consumption may have cool forms. A few persons class either *maiz amarillo* or *colorado*, yellow or red corn, as *fresco*, cool, but *maiz blanco* and *amarillo* are usually considered *caliente*. Roasting ears of new corn of all colors are *fresco* to *frio*, *masa*, ground corn, is *fresca*, while *nixtamal*, corn boiled with lime before grinding, and the *tortilla* are both *caliente*. Unwillingness to classify the *tortilla* and *maiz* in general was expressed to us by a Mixtec elder in Jamiltepec. He suggested that maize, being the food that fills, that is, the basic food, could not be restricted by such a quality. He preferred to rely upon another classification which cuts across the hot-cold dichotomy, that is, two kinds of food: the staples, or foods such as corn and beans which satisfy the body, and the foods of lesser importance, the fruits, greens, and beverages.[4] He was particularly careful to distinguish the filling foods that satisfy hunger as those one cannot overeat. Thus with these foods there are none of the consequences associated with eating other foods to excess.

Nine varieties of plantains and bananas are grown in the region and are known to the three groups. Of these, most are classified as *fresco*, while the larger cooking plantain (a staple food among poorer Mestizos), the small and most common *manzana* banana, and one other variety, the *costa rica*, are classed as *caliente*.

Among the *chiles*, all six varieties known in the region are hot, some more so than others. *Chile amarillo* when green is considered cool by some. Dried *chiles* are more *caliente* than when green. There is apparently a relationship between the *picante*, sharp, quality and the *caliente* class, the sharper the chile (*mas bravo*), the hotter it is considered.

Alcoholic beverages are classed as follows: *aguardiente* cane brandy and *mescal* are *caliente*, while *cerveza*, beer, is *fresca*. *Tepachesx*, fermented beverages of cane, pineapple, or corn, are regarded as *fresco*. Bottled soft drinks are *fresco*; ice, however, is *caliente*. The explanation for the latter is that ice is made by hot machinery. Coffee, *chocolate*, and *cacao* are all *caliente*.

Oils, lard, and butter (the latter known only to traveled or wealthy Mestizos) are regarded as hot.

Cow's milk and cheese prepared from it are *fresco*, while goat's milk is classified as either *caliente* or *fresco*. Amusing speculation is aroused by this question among Mestizos because the goat is considered to be a *caliente* animal in the sexual sense, and goat-keeping is a restricted occupation—and anyway goats are not milked. This attempt to classify goat's milk demonstrates the local willingness to include peripheral or unknown foods in the system. The Mixtecs who are least familiar with goat-keeping, unless well versed in Spanish, tended to classify these products as cool, like cow's milk.

Eggs of domestic fowls and of the sea turtle are considered *fresco*.

All of the *camotes* (tubers and fleshy roots) are classified. With the exception of *jicama* (*Pachyrhizus erosus*) a root which is eaten raw and is unanimously considered *fresca* or *fria*, the cooked *camotes*, *camote de palo* (*Mandioca* sp.), *camote de monte* (*Dioscorea* sp.), *camote de bejuco* (*Ipomea batatas*) and *nyamitioco* (*Xanthosoma* sp.) are considered by a majority of Euro- and Afromestizos to be *caliente*. However, most Mixtecs consider the latter two as *fresco*.

Meat of most domesticated and game mammals and birds is considered *fresca* by Mixtecs and Mestizos. Beef is, however, frequently considered *caliente*. Mixtecs who eat little meat of domesticated animals make a distinction between the green iguana, *fresca*, and the dark iguana, *caliente*. Boiled hen, but not pullet, is considered by Mestizos appropriate for mothers after childbirth. It should be noted that informants were equally divided as to whether hens are hot or cold. Some recommended the food for a newly delivered woman because it is hot, and others do so because it is cool.

Eight kinds of cool drinks (*aguas frescas*) are prepared from various fruits or seeds with water and crude brown or white sugar. Both of these sweetenings are hot, but the drinks of which they are ingredients are cool. Sugar cane is considered cool by Mixtecs and hot by both groups of Mestizos.

Coconut water is cool, and therefore, as expected, cannot be drunk when one has a cold or chill, but is also prohibited for anyone with a fever. However, teas of lemon, orange, and *telimon* leaves can be drunk by persons with fevers because these are cool. Among spices and condiments, cinnamon and cinnamon tea are hot, as are black pepper and allspice. Garlic is one of the few plants thought to be very hot.

Among fruits, all of the *ciruelas* (fruits of *Spondias mombans* and *S. purpurea*) are classified as *caliente*, as are mangos. Both kind of fruit are said to be likely to cause stomach-ache if eaten in excess—and in the quantities eaten by children of poorer Mestizos this is the result.

In summary, the hot and cold classification, a variant of the Hippocratic system, in use in the *costa chica* of Oaxaca by Mixtecs and Afro- and Euromestizos forms the basis for a large part of traditional medicine. This system serves as guide in categorizing medicinal plants, foods, and diseases, and treatment of disease. In addition to hot and cold, bitterness is recognized as a distinct category and used to characterize a group of plants for specific functions. The system is in use among the three population groups and there appears to be a wide area of agreement among them in specific classification. However, the system is not all inclusive and is not extended to all nature. Individuals within each group disagree on classification. The system frequently reflects physical state in that qualities of ripeness, sharpness, irritation, action of cooking and other factors are assigned association with hot or cold.

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Notes

1. Field work in 1954 and 1955 was assisted by Northwestern University, American Association of University Women, and the Chicago Natural History Museum, which the authors gratefully acknowledge.
2. Foster (1952), p. 8.
3. Cf. Mijangos Ross (1954), pp. 25–26, 29, 31–32.
4. Perez Hendrichs (1946) reported the same classification for the Balsas Basin.

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LE CONCEPT DE CRIME EN DROIT DJARAI

Pierre-Bernard Lafont

Les Djarai, population montagnarde du sud indochinois, sont des proto-indochinois de langue malayo-polynesienne, qui ont conservé jusqu'à ce jour leurs concepts juridiques archaïques et leurs tribunaux coutumiers ancestraux.

Pour les Djarai, comme pour de nombreuses populations archaïques, les notions de droit, de religion, et d'économie, ne se présentent pas comme dans les disciplines modernes, sous une forme compartimentée, differenciée et indépendante de ses voisines. Chaque discipline s'articule sur les autres, s'imbrique aux autres, les interpénètre et se confond avec elles, au point de ne former qu'un tout indifférencie, dont aucune des constituantes ne devrait être analysée separément. Vouloir analyser le droit de la tribu, en tant que droit, serait impensable pour un Djarai, car ils considèrent que leur construction sociale forme un tout qu'il est impossible d'analyser autrement que dans sa totalité.

Malgré la difficulté qu'il y ait à étudier un aspect de ce tout, sans analyser le tout lui-même, car une telle étude est en opposition avec la psychologie du groupe étudié, nous allons dégager la notion de crime, tel qu'elle peut se definir après avoir été exhaustée de ce tout qui est le complexe social djarai.

Pour un Djarai, la qualification criminelle requiert une condition sine qua non. Il ne peut y avoir crime que s'il y a eu mort d'un individu.

Que cette mort ait été un fait immédiat ou à terme; qu'elle ait été le fait d'une violence physique ou morale; qu'elle ait été le fait d'une pratique de sorcellerie volontaire (par utilisation de la magie noire importée du Laos ou par la traditionnelle absorption de la force vitale de la victime) ou involontaire (c'est le cas de certains sorciers MLAI dont la tête—siège du génie malfaisant qui les habite—quitte le corps pendant le sommeil et sans l'assentiment du sorcier, va dévorer l'âme d'un tiers).

Mais cette tribu qui ignore le sacrifice humain et le crime rituel, ne considère pas que l'exécution d'un individu entraine obligatoirement une qualification criminelle. En effet cette notion est surtout basée sur la causalité.

L'homicide, la plus grave des atteintes à la personne, ne suit pas, en droit djarai, les mêmes échelles de qualification qu'en droit moderne. Les notions de meurtre, d'assassinat, d'empoisonnement y sont inconnues, seule la notion d'homicide existe; notion qui d'ailleurs, diffère sensiblement des notres,ce qui nous oblige à la définir.

Pour un Djarai, un seul fait prime dans l'homicide, c'est la disparition de l'individu, qui était membre d'une famille, aussi la sanction portera-t-elle beaucoup moins sur l'acte lui-même que sur son résultat. C'est ainsi qu'un homicide involontaire (Affaire 67, Chéo-Réo, 1953) sera sanctionnée de la même manière qu'un homicide volontaire (Affaire 89, Chéo-Réo, 1950). On ne sanctionnera pas l'acte lui-même mais la disparition d'un membre d'une famille matronymique. La sanction portera le nom "prix du corps", s'élevera à quinze buffles, c'est-à-dire à l'ancien taux traditionnel utilisé pour le rachat d'un prisonnier de guerre et sera versée à la famille matronymique du défunt. L'homicide n'est cependant pas identiquement classifié. De même qu'il y a des homicides involontaires et des homicides volontaires ces derniers ne rentrent pas tous dans la catégorie des crimes. L'homicide requiert pour être qualifié crime certaines qualités: il doit avoir été perpétré sans cause et avoir entrainé la mort.

Affaire 29, Chéo-Réo, 1950. H'Ao a fait manger des fourmis rouges empoisonnées à Pui qui en est mort. Le tribunal la considérant comme criminelle la condamné à payer le prix du corps à la famille matronymique de la victime.

Affaire 51, Chéo-Réo, 1950. A la suite d'une querelle entre H'Puih et H'Gai celle-ci s'est prétendue sorcière et a affirmé qu'elle rendrait malade le fils de H'Puih. Elle a ajouté que lorsque H'Puih n'aura plus de bêtes pour faire le sacrifice elle mangera l'âme de l'enfant. Or le fils de H.Puih est tombé malade. Le tribunal decide que H'Gai sera responsable de la mort de l'enfant si elle se produit et devra payer le prix de son corps à sa famille matronymique.

Dans l'affaire d'empoisonnement précitée, le droit djarai rejoint le concept moderne de crime; mais l'affaire de sorcellerie met en valeur une divergence absolue entre le concept moderne de qualification et le concept djarai. Pour un juriste moderne l'affaire de sorcellerie devrait se terminer par un non-lieu car il est impensable que l'on puisse tuer quelqu'un par un simple artifice de sorcellerie, or pour les Djarai l'existence de la sorcellerie maléfique est un fait réel, existant, nous dirons même "visible", c'est un crimen atrox. Alors que nous regardons le fait social avec un rationalisme aux yeux duquel aucune construction étrangère ne trouve grâce, les Djarai pensent le fait social en fonction d'une culture, qui pour nous être étrangère, n'en existe pas moins. La différence de qualification est le fait d'une discordance culturelle; les elements culturels des sociétés n'étant pas les mêmes, chacune d'elles établit ses règles de qualification correlativement à son concept de protection sociale.

Le crime de sorcellerie soulève une telle horreur parmi les Djarai que ces gens, qui ignorent les peines corporelles, trouvent normal de mettre à mort un sorcier maléficient. Le châtiment du sorcier criminel fait figure d'assassinat aux yeux des tenants du droit moderne qui voient le crime là où les Djarai voient la justice. Il ne faut cependant pas croire à l'impossibilité d'avoir un concept tant soit peu concret de la notion de crime en droit djarai.

Affaire 115, Chéo-Réo, 1950. Thul, Thanh, Hop, Kjul et Boa ont tué Jo après l'avoir fait tomber dans un guet-apens. Ils "prouvent" avec vingt-quatre témoins que Jo était un sorcier maléficient, sa famille elle-même le reconnaît. Le tribunal considère que l'exécution de se sorcier n'est pas un crime mais une œuvre de justice, car elle arrête ses méfaits. Aussi ne condamne-t-il pas les "justiciers" à payer le prix du corps.

Affaire 13, Chéo-Réo, 1953. Yo déclare que Do l'a menacé de le faire mourir en lui faisant gonfler le ventre jusqu'à éclatement. Pour cela il l'a tué. Comme Yo ne peut fournir de témoins et que les villageois déposent que Do n'était pas sorcier, le tribunal condamne Yo à payer le prix du corps car il le considère comme un criminel.

Deux termes constituent la relation du phénomène crime. Le premier est un acte individuel portant atteinte à autrui, le second un terme appréciatif fonction d'une éthologie qui fait que tel acte est qualifié crime et entraîne une réaction collective. L'aspect phénoménologique du crime considéré en tant que manifestation du comportement humain, ne doit pas nous faire perdre de vue que sa qualification est fonction d'un système social de valeur, le concept de protection sociale étant plus extensif chez les Djarai que chez nous.

Considéré, par le droit moderne, comme un acte n'engageant aucune responsabilité et ne donnant lieu à aucune conséquence pénale, le suicide est envisagé sous un double aspect par les Djarai. L'individu peut avoir obéità une pulsion instinctuelle, auquel cas il n'y a pas d'affaire mais il peut aussi avoir agi à la suite d'une oppression caracterisée, auquel cas la responsabilité du suicide est imputée à l'agent responsable de l'oppression tyrannique qui est qualifiée de criminelle et punie comme telle. L'oppression tyrannique, particulièrement extensive, peut englober les injures, les menaces, les outrages, les voies de fait et les violences permanentes.

Affaire 43, Pleiku, 1951. H'tac declare que son frère Ngeo s'est suicidé car sa femme H'kek le maltraitait sans motif. Le tribunal, vu que Ngeo s'est suicidé un jour où sa femme était absente et qu'il n'avait donc pu suivre son oppression décide que la responsabilité du suicide ne peut être imputée à H'kek.

Affaire 42, Chéo-Réo, 1951. H'gung et son père ont bu une jarre pour sacrifier au génie de la maison mais son père ayant oublié le poulet sacrificiel, elle s'est moquée de lui, lui a dit qu'il devenait trop vieux et que tous les gens de son âge étaient morts. Son père ne pouvant supporter sa honte et sa douleur alla se pendre. Le tribunal considérant H'gung était l'agent de la mort de son père la condamne en tant que criminelle à payer le prix du corps.

Le suicide, qui dans le second cas nous apparaît comme une vengeance indirecte de l'oppressé, moralement diminué, vis-à-vis de son oppresseur, puisqu'il sait que sa mort entraînera de sérieuses conséquences qualificatives, fait figure de crime aux yeux des Djarai. Mais ils exigent pour cette qualification une preuve absolue de la relation de causalité entre l'oppression tyrannique et le suicide, causalité qui à leurs yeux ne peut se caractériser que par une réaction spontanée et immédiate du de cujus.

Le concept de criminalité est lié à une notion de protection du groupe familial matrilinéaire. Ce concept est inhérent à la psychologie du groupe, qui a toujours rejeté les concepts de droit moderne, que Français et Viet-Namiens ont essayé de lui faire adopter. L'opposition de la population étant basée sur cette action:. ce qui est reconnu comme bon pour un non-Djarai ne peut être accepté par un Djarai puisque la société Djarai est basée sur un système qui lui est propre.

C'est ainsi qu'il a été impossible de faire disparaître la notion de crime de sorcellerie, car pour le Djarai la sorcellerie que ce soit par magie ou par destruction traditionnelle d'individus, existe et l'exécution du sorcier maléficient est toujours considerée comme un acte de salubrité publique, comme un acte de justice, qui loin de créer une scission au sein de ce microcosme qui est le village, resserre au contraire, la cohésion du groupe—exécuteur et famille de la victime y compris—car le monde djarai est psychologiquement dominé par une peur atavique des forces surnaturelles et de leurs agents.

C'est ainsi que l'on a vu (Affaire 837, Chéo-Réo, 1950) l'administration française être obligée de relâcher l'exécuteur d'un sorcier maléficient (incarcéré comme assassin sur intervention d'un chef de district français) à la suite de la pression exercée par un groupe de villages en sa faveur et en particulier par la famille de sa victime.

Pour les habitants comme pour la famille de la victime, il n'y avait qu'un seul coupable, le sorcier; et personne n'admettait que l'exécuteur puisse être sanctionné pour une faute inexistente à leurs yeux.

De même que le concept criminel est conforme à la psychologie du groupe les sanctions ancestrales, appliquées par les tribunaux coutumiers sont considérés comme étant les seuls justes et les seuls applicables; car elles synthétisent un concept de self-defense qui est corollaire d'un concept de criminalité accepté par tous (nous avons vu plusieurs criminels venir de leur propre chef avouer leur crime, l'un faisant plus de soixante kilomètres à pied, et réclamer la sanction de leur acte au tribunal) concept qui est une des assises des plus solides de l'unité tribale, car il est unique pour tout le groupe social et propre à cette seule tribu.

Certains, peut-être s'étonneront que je ne fasse aucun parallèle entre le concept Djarai et les concepts d'autres groupes sociaux asiatiques, qui eussent pu être mis en parallèle, je pense plus particulièrement aux WA de Birmanie et aux tribus du centre de l'Inde. Etant donné que cette communication est une communication juridique je ne déborderai pas mon sujet.

L'aspect phénoménologique du crime, considéré en tant que manifestation du comportement humain ne doit pas nous faire perdre de vue que sa qualification est fonction d'un système social de valeur.

Les degrés d'évolution culturel du groupe social déterminant les éléments psychomoteurs de l'acte individuel, en conformité avec une éthologie préétablie, il y a une quasi impossibilité à établir une règle généralisée de qualification criminelle basée sur une étude comparative. Loin de moi la pensée de vouloir faire naître un sentiment de relativisme. Il n'est pas impossible d'établir un concept tant soit peu généralisé du crime, car son processus causal est toujours le même, à savoir: un individu a obéi à une pulsion instinctuelle, qui méconnaît les éléments culturels de motivation de son groupe; la notion de crime étant liée aux notions de sujet de droit, de normal et d'anormal.

Il serait donc prétentieux au stade actuel des études d'ethnologie juridique de vouloir établir, à l'aide de quelques comparaisons, une définition plus large de la notion de crime.

L'Ecole Française d'Extrême Orient.

ASPECTS DE LA ROYAUTÉ BATEKE (MOYEN-CONGO)

Annie Lebeuf

Laissant de côté au cours de ce bref exposé les aspects historiques, juridiques ou politiques, je chercherai seulement à dégager dans quelle mesure l'organisation du pouvoir, les rites qui se rapportent à l'élection, l'intronisation et la mort des principaux tenants de l'autorité illustrent une conception de l'univers propre au groupe considéré.

En insistant sur cet aspect nous touchons là un point de méthode qui mérite que l'on s'y arrête. Cette méthode, qui fut mise au point par le Professeur Marcel Griaule, fait une large place au point de vue que les intéressés eux-mêmes possèdent sur leurs propres institutions. L'ampleur des découvertes de son école, la richesse des mythologies qu'elles mettent au jour nous interdisent actuellement de nous contenter de décrire les peuples qui relèvent de l'ethnographie en termes qui leur sont complètement extérieurs—de n'envisager qu'un point de vue formel.

C'est le point de vue des intéressés sur eux-mêmes, les croyances au nom desquelles ils agissent, les conceptions qui président à ces croyances qui ont, au premier chef, retenu notre attention.

La plupart des peuples que nous étudions ont une conception du monde qui est en général fixée dans un mythe que toutes les activités, matérielles et autres, de groupe recouvrent en partie ou en totalité.

Il nous parait à l'heure actuelle impossible de classer des différentes populations, leurs techniques et leurs institutions sans une connaissance approfondie de ces systèmes.

C'est ainsi que chez les Batéké, ou plus exactement dans le groupe *atyo*, établi sur les plateaux situés au nord de Brazzaville, sur la rive droite du Congo, au cours des différentes enquêtes que nous avons pu y mener, tant sur les techniques que sur les structures sociales, politiques ou religieuses, nous avons cherché à dégager des grandes lignes du mythe fondamental.

Le pays *atyo* est dirigé par un souverain, le *Onko*, improprement appelé *Makoko* par les Européens, qui bien qu'ayant perdu depuis l'installation de l'administration française toute autorité sur le plan politique conserve cependant aux yeux de toute la population un pouvoir spirituel incontesté.

Il partage ce pouvoir avec deux femmes, la *Ngassa* et la *Wãnfitèré* et huit dignitaires principaux.

Choisis au sein des mêmes familles, il existe un système d'équilibre constant entre, d'une part, le pouvoir du Roi et celui de la Ngassa, entre celui de la Ngassa et celui de la Wãnfitèré (c'est-à-dire, entre celui des deux femmes), entre celui des différents dignitaires associés 2 à 2 ou 4 par 4, et enfin entre celui du couple royal et l'ensemble des dignitaires.

Les familles de la Ngassa et de la Wãnfitèré sont les plus importantes du royaume. La première appartient à la famille qui est responsable des cultures, la seconde à une famille de forgerons.

La *Ngassa* est la première femme du royaume—elle est intronisée en même temps que le Roi et renonce après cette intronisation à avoir des enfants. Elle dirige les cultures, fait la première butte, plante la première graine. Son oncle utérin, un des principaux dignitaires, est le détenteur du *Kuli*, sorte de boule faite en matière végétale, substitut d'une graine primordiale, qui est un des principaux attributs de la royauté.

Lorsqu'elle meurt, le Roi était autrefois décapité immédiatement; actuellement il est destitué.

Le Wãnfitèré: A la mort du Roi, c'est à la Wãnfitèré que l'on remet les principaux attributs royaux, c'est à sa famille qu'ils appartiennent et c'est elle qui remettra temporairement ces objets au nouveau roi qu'elle doit introniser.

Après cette cérémonie son rôle est terminé. Elle désigne alors, dans sa propre famille, sa remplaçante auprès de ce dernier, la nouvelle Wănfitèré qui doit être une femme d'un certain âge, renoncer à avoir des rapports sexuels avec qui que ce soit et qui est chargé exclusivement de la cuisine du Roi et de la Ngassa.

Comme toutes les familles *atyo*, sa famille est liée à un certain bois et à un point d'eau où vit un génie protecteur de la famille, émanation du principal génie de l'eau. C'est ce dernier qui est censé résider dans le point d'eau lié à la famille de la Wãnfitèré, lieu où apparut le couple primordial.

Son oncle utérin est gardien de cet eau qui est la seule que puisse boire le Roi.

Les 8 dignitaires sont la porte parole du Roi, le chef des armées, le chef des tisserands, le chef des forgerons, et les chefs des quatre principales terres du royaume.

Tous détiennent des pouvoirs particuliers; ils sont les descendants des quatre couples d'ancêtres qui, dans le mythe, jouèrent un rôle dont nous parlerons ci-après.

L'élection du Roi: Le futur Roi est choisi par un conseil comprenant les 8 dignitaires et la Wãnfitèré du Roi défunt. Il est choisi parmi les fils que ce dernier eut avant son intronisation, ses frères ou quelque autre membre de sa famille.

Ce choix, qui est fait trois ou quatre lunes après la mort du Roi, a lieu à huit clos. Dès le lendemain, deux des dignitaires vont prévenir l'élu qui se sauve immédiatement au forêt. Le chef des tisserands part à sa recherche, le ramène et il est battu à mains nu par l'ensemble des dignitaires. On lui impose solennellement un collier et il est alors considéré comme Onko, mais il ne renonce pas encore à ses anciennes femmes, peut encore avoir des enfants et ne détient aucun des pouvoirs que lui confèrent les attributs royaux. Ceux-ci ne lui seront remis qu'au moment de l'intronisation proprement dite.

L'intronisation du Roi: Celle-ci peut avoir lieu des années après l'élection et c'est au Roi à en décider la date après que les dignitaires et l'ancienne Wãnfitèré aient donné leur accord.

Cette intronisation est marquée par une retraite de neuf jours dans une maison construite à cette fin par le tisserand.

Le Roi doit alors renoncer définitivement à ses anciennes épouses, renoncer à avoir de nouveaux enfants et à avoir d'autres rapports sexuels qu'avec la Ngassa qui lui est présentée au moment où il entre en retraite et en compagnie de laquelle il va demeurer sans sortir ni se laver pendant neuf jours.

L'ancienne Wãnfitèré est la seule personne qui puisse voir le Roi et la Ngassa pendant cette période. C'est elle qui leur fait leur nourriture, composée exclusivement de manioc provenant d'un champ particulier, sur un feu spécial allumé dans l'habitation par le chef des forgerons.

Dans cette pièce l'ancienne Wãnfitèré a déposé tous les attributs royaux dont le Roi doit prendre possession, attributs gardés pendant l'interrègne, comme nous l'avons vu, par la Wãnfitèré.

Les principaux sont:

1 trône au centre duquel est placée une pierre provenant du point d'eau de la famille de la Wãnfitèré,

le kwèmbali, talisman composé principalement d'un vase contenant, entre autres, certaines reliques des précédents Onko, reliques qui sont l'os frontal et l'auriculaire droit de chacun d'eux,

9 enclumes qui personnifient les 8 dignitaires et la Wãnfitèré ou plus exactement le pouvoir de chacun d'eux,

et enfin le kouli, boule de matière végétale dont je vous ai déjà parlé et qui est remise par la famille de la Ngassa à la Wãnfitèré pour être déposée dans cette pièce.

Se sont là les principaux attributs; il y en a de nombreux autres qui ont tous leur importance mais qu'il serait trop long d'énumérer.

Au cours de ces neuf jours le Roi et la Ngassa doivent lutter contre les forces incarnées dans ces objets et sortir victorieux de cette bataille qui leur permet de se les approprier.

Si le Roi ne meurt pas avant le troisième jour de cette retraite, des réjouissances sont organisées par l'ensemble du peuple.

Le jour de la sortie, montés sur un pavois, le Roi et la Ngassa sont présentés à la foule. La nouvelle Wãnfitèré est présenté au Roi par l'ancienne. Pendant ce temps, le feu de la maison de la retraite est transporté par un enfant mâle de la famille de forgeron, à l'insu de tous, dans la cuisine du Roi, c'est-à-dire dans la pièce réservée à la nouvelle Wãnfitèré, pièce particulière que le forgeron a construit le jour même. Ce feu, alimenté par un bois spécial, est le feu du Roi et ne sera éteint que le jour de sa mort.

Chaque dignitaire lui fait alors, en public, un cadeau, symbole du pouvoir acquis au cours de la retraite, pouvoir dont chaque dignitaire était auparavant seul détenteur.

Le Roi s'est alors engagé à ne plus paraître en public, à ne jamais regarder le ciel, à ne pas marcher sur le sol nu, à ne pas regarder le Congo et à ne jamais frapper qui que ce soit.

Après cela, le Roi doit encore passer trois jours chez lui, sans sortir, au cours desquels toute la population de la capitale a le droit de voler et de piller n'importe où.

La mort du Roi: Voyons maintenant rapidement ce qui se passe lors de la mort du Roi.

Seules les deux femmes et les principaux dignitaires sont avertis de cette mort.

Deux d'entre eux prélèvent sur le cadavre un disque découpé dans l'os frontal et l'auriculaire de la main droite, et les placent dans le kwembali qui est remis, ainsi que l'ensemble du regalia, à la Wãnfitèré. L'inhumation a lieu le jour même, en secret, dans la pièce même où il est mort.

L'annonce officielle de cette mort ne sera faite que neuf jours plus tard. C'est à ce moment seulement que pourront éclater les cris de douleur et les lamentations, aucune larme ne devant être versée auparavant.

Pendant ces neuf jours les dignitaires confectionnent avec les branches d'un arbre particulier une représentation du Roi, un simulacre que l'on habille avec les vêtements royaux et auquel on impose le collier royal. Cet ensemble, substitut du Roi, est alors placé dans un catafalque dressé dans la chambre mortuaire.

C'est ce substitut qui sera enterré solennellement, placé dans une fosse, face à l'Est lorsque son prédécesseur a été placé face à l'Ouest, fosse dans laquelle autrefois on jetait au préalable un couple d'esclaves dont on avait brisé les membres.

Pendant les neuf jours qui suivent l'enterrement, nul ne peut chasser, pêcher ou cultiver, et les activités normales ne reprennent qu'après que certains dignitaires aient remis à la Wãnfitèré un offrande particulière.

Que ressort-il de l'ensemble de ces faits? Il n'est pas possible d'entrer ici dans les détails en vous exposant toutes les correspondances qui existent entre eux et le mythe. Mais je voudrais cependant vous en indiquer les grandes lignes.

D'après la mythologie atyo, l'origine de l'humanité remonte, grâce à l'intervention du génie de l'eau, à un couple de jumeaux issus du bris d'un œuf tombé du ciel en même temps que la première pluie sur une roche émergeant des eaux.

Demeurant dans la moitié de la coquille, première embarcation, les jumeaux furent entraînés par le courant jusqu'au centre du pays où ils échouèrent. (*Kari*, le nom de ce lieu qui signifie centre, est également celui du lieu où réside le Roi, et plus particulièrement de son trône qui contient un éclat de la roche dont je viens de vous parler.)

Ces deux jumeaux primordiaux eurent deux couples de jumeaux qu'ils envoyèrent l'un à l'Est, l'autre à l'Ouest, leur confiant la garde d'une espèce animale. Chacun de ces couples eut un nouveau couple de jumeaux qui allèrent respectivement habiter dans les deux autres directions de l'espace et qui eurent à leur tour un enfant unique. Ces derniers, de sexe opposé, s'unirent et revinrent s'installer au centre où ils reconquérirent, grâce à une nouvelle intervention du génie de l'eau, la place que leurs ancêtres, à la suite d'un certain nombre d'évènements, avaient perdue. Le vol du feu, la perte d'une graine ainsi que la perte de leur connaissance au profit des jumeaux des seconde et troisième générations ayant entrainé la mort du couple primordial et la fin des naissances gémellaires.

Dans le rite d'intronisation tout se passe comme si le Onko et la Ngassa, individus nés uniques, devaient au cours de leur retraite devenir les substituts du couple primordial.

Ils sont choisis par 8 dignitaires qui représentent les 4 couples d'ancêtres liés aux quatre directions de l'espace at aux quatre éléments et par la Wãnfitèré du Roi défunt qui intervient comme le génie de l'eau pour les aider à rétablir ordre.

La maison de la retraite est assimilée à l'œuf d'où jaillirent les jumeaux. Seuls les deux impétrants et l'ancienne Wãnfitèré, comme nous l'avons déjà vu, peuvent y pénétrer. Cette maison est construite en un jour par le tisserand, et je ne peux insister ici sur le rôle essentiel que joue le tisserand dans cette population.

La Wãnfitèré leur remet le feu, symbolisé par le kwembali et la graine perdue, le kouli, ainsi que les neuf enclumes personnifiant les pouvoirs et les forces des huit dignitaires et les siennes. Elle leur permet de reconquérir tout ce que le couple primordial avait perdu.

A aucun moment le Ngassa n'est considéré autrement que comme la sœur jumelle du Roi.

Ce couple garant de l'ordre universel est fixé dans l'espace. Il en est le centre et il ne peut se déplacer; un réseau serré d'interdits s'y oppose. Ces interdits vont même jusqu'à l'empêcher de procréer, seul le génie de l'eau pouvant transmettre la vie.

La mort du Roi ou celle de la Ngassa qui entraîne obligatoirement celle du premier inaugure une période de transition. Le Roi a revêtu alors une forme temporaire, celle d'un arbre, arbre qui dans le mythe est une des formes sous laquelle ressuscita le premier homme.

La période de désordre proprement dite est celle des trois jours de pillage qui marque la fin de l'intronisation royale.

Je ne vous ai donné là qu'un aperçu très bref et très complét des rites qui amènent de la part des intéressés des explications qui, pour nous, sont d'une extrême complexité. Ce sont là les premiers résultats d'enquêtes préliminaires qui demandent à être poursuivies sur le terrain.

Mais je voudrais insister sur le fait que les explications qui m'ont été données m'ont été transmises sans que s'y mêle aucun caractère secret. C'est le point de vue du vulgaire sur des questions que les initiés interprètent d'une façon plus profonde.

Si pour l'instant il ne m'a pas été possible d'aller plus avant dans cette connaissance, il m'a semblé cependant intéressant de noter, surtout dans une population qui a particulièrement souffert de la traite des esclaves, de l'installation sur son sol des compagnies concessionnaires et de tout ce que cela entraîne, de noter combien malgré ces bouleversements, certaines valeurs spirituelles étaient demeurées solides.

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THE ROLE OF MUSIC IN WESTERN APACHE CULTURE

David P. McAllester

The discipline of the ethnomusicologist is comparative musicology in its broadest sense. It is obvious that music is a highly integral part of the culture in which it is found. This means that comparative musicology should be, and in our case necessarily must be, comparative culturology. This is why ethnomusicologists are drawn largely from the field of anthropology or at least have had extensive anthropological training.

This insistence on the relationship of music to culture should be unnecessary and would be if it were not for a peculiar trait in our own Western European culture: the bifurcation of the concept of culture. We *can* think of culture in the anthropological sense of the total way of life of a people, but we also think of culture in the sense of "cultivated," with a particular emphasis on art forms and art for art's sake. The result of this cultural trait of ours has been a separation of art from culture-as-a-whole. We are more likely to discuss the creative periods of Picasso than Picasso as a manifestation of the social, religious and economic pressures of his times, or, in other words, Picasso as a manifestation of his culture.

Similarly, in music, we are very prone to a consideration of music *qua* music outside of its cultural context. We are most likely to discuss a song as an art form, as pretty or ugly and why, and in many other ways outside its principal cultural function.

In recent years the functional, whole-cultural emphasis has been brought—I could say "has been brought *back*"—into our intellectual discourse by, among others, the ethnologist. And the ethnologist learned this whole-cultural perspective from his contacts with small homogeneous groups. Such cultures, as we have heard since the days of Herbert Spencer, have not compartmentalized art, religion, earning a livelihood, social organization and the other aspects of their lives. They live their lives whole and their cultures can be seen as wholes by the intelligent visitor.

The other anthropological perspective, the cross-cultural perspective, the comparison of custom across the wide gamut of diverse cultures, is less unique with us, but it has a special dimension when used by anthropologists. In our discipline as we learn the range of variation in human behavior, and, on the other hand, study the great unifying similarities, our comparative view is steadied and controlled throughout by the great lesson we have learned from our less sophisticated subjects, who are also our instructors, that cultural manifestations are meaningful only in their cultural context.

Let me illustrate these few words with an anecdote: I asked an American Indian if he thought a certain song, unfamiliar to him, was beautiful and his reply was: "I don't know. I don't know what the song is for." It was a question no one would have asked him in his culture and an answer I never would have received in mine. I saw then as never before why my teachers demanded that an anthropological education must include first-hand acquaintance with customs and attitudes different from my own.

In this paper I will try to apply the two perspectives, whole-cultural and cross-cultural, to the music of the Apache Indians of Arizona. I should say here that my remarks will apply only to the White Mountain Apaches, since I did not visit the other group of the Western Apaches on the San Carlos reservation, and these observations are based on only one summer's field study.

To get as far into Apache music as I could in a short period of time, I used the familiar techniques of the participant observer. I camped with Apache families, attended ceremonies and even assisted a medicine man in singing over a sick child. I learned some Apache songs since I have found elsewhere that even one or two songs, imperfectly rendered, are tremendous rapport builders. I secured the permission of the tribal council to make recordings and found the recording sessions invaluable with their long discussions of origin, use and meaning of songs. The questions that I asked centered around the following:

How the Apaches "felt" about their music
What musical instruments they used
How old children were when they began singing
Whether there was special effort to teach songs to children
What the different kinds of songs were
Was it a common thing to make up new songs
Were there happy (sad) and pretty (ugly) songs
Whether there were tabus of various kinds in music
What Apaches thought of non-Apache music

I attempted to ask the more general questions first in any interview in order to avoid suggesting specific answers by specific questions.

By such methods I made at least a start toward learning the various kinds and uses of Western Apache music and attitudes toward music. In trying to present some of these I will use the cross-cultural perspective by comparing Apache music with our own, and I will attempt to provide the whole-cultural perspective with ethnographic detail and excursions into various aspects of Apache culture.

DIFFERENCES

One of the first differences to strike me was the difference in function. With us a principal function of music seems to be as an aid in inducing attitude. We have songs to evoke moods of tranquillity, nostalgia, sentiment, group rapport, religious feeling, party solidarity and patriotism, to name a few. Thus we sing to put babies to sleep, to make work seem lighter, to make people buy certain kinds of breakfast foods, or to ridicule our enemies. To the Western Apaches, music has a more direct function. For example in curing, the music is not to *predispose* the patient to getting well but is the direct cure. Taken so directly we call such a conception of music superstitious or magical and a confusion of cause and effect. Perhaps when our knowledge of music therapy progresses beyond its present infant stage we may get over feeling so superior in this matter.

Certainly one of the principle functions of Apache music is healing, and many aspects of Apache attitude towards music and healing are different from ours. I will list some of these:

(1) *Healing is social.* It is performed at a large gathering, the larger the better, by the medicine man, and all who know the chant even partly join in. There are

drummers, dancers and many on-lookers. The whole community, men, women, children and dogs are present, all participating, if only by being there. Healing is also social in another sense: social misdemeanors of the patient may be uncovered by the medicine man by the power of music. In the course of the chanting the practitioner may go into a state of trance. Everyone stops singing. The drummers go on beating the drums softly in unison (called "thunder drumming"). Emerging from the trance, the medicine man may reveal some selfish or other kind of antisocial act of the patient and pray to the supernatural for forgiveness and general blessing for all present.

(2) *Healing music is fun.* Drinking is considered necessary for the right feeling of group empathy and in order that the singing will be free and enjoyable. The atmosphere is not like that of our hospital or sick room but one of boisterous good spirits with shouting, clowning and flirtation going on. In the music itself, hearty yells are frequent and the parodying of words and music may occur. The songs may be interspersed with jokes and *double entendres.*

(3) *Healing music contains great power.* The words in the chants bring power to the patient and blessing to all who attend. Certain types of song are specific to certain ailments. In his trance the medicine man may discover that deer songs should be added at a certain time to a sing made up largely of lightning songs. These powerful songs can also be dangerous if misused. The sanction is the danger of being struck by lightning or bitten by a snake or spider. As presented in the literature, such tabus are often stated as absolute. They may be so among certain groups, though we are beginning to learn a good deal about the difference between ideal culture and actual culture. For the Western Apaches I witnessed the results of a broken tabu. A medicine man sang very special songs for my recording in conditions of some secrecy and with the warning that it might bring lightning, since these matters should only be discussed in the winter. A very severe lightning storm did come up and five people in the community were so frightened by near misses that they had to have the help of a ceremonial practitioner. My medicine man was busy for some time healing one of these cases, and then came back and resumed recording with me. He went on recording the same dangerous songs. There is no doubt in my mind that he felt that they *were* dangerous songs, but it was a danger he could handle. This is certainly not the abject terror of the native before supernatural forces that we heard about from the early missionaries.

(4) *Healing music is not learned in an ordinary way.* Instead of being learned by ordinary memorization, healing chants are learned by ordeal and supernatural help. Putting himself under the tutelage of a ceremonial practitioner, the student listens to the songs for four nights without sleep. Then, perhaps several years later, the songs come to him in his sleep and he is ready, himself, to become a practitioner. Actually, of course, he hears these songs many times at ceremonials during this period, but this is the Apache interpretation of how healing songs are learned.

Other differences are:

(1) *Absence of certain types of songs* seems to correlate with striking differences between Apache culture and our own. There are no *lullabies* as such, though a mother might croon "baby, baby," over and over. Child training is, in general, more relaxed than ours, and Apache mothers do not seem to have to tell their children either to sleep or to eat. Babies are soothed and made much of but the whole attitude, as far as I could see it, was permissive: in fact the Apache from

infancy to adulthood seems to be on a self-demand schedule. This may well relate to the absence of *work songs*. In general such songs seem to go with group labor, and Apaches do not go with group labor. In a real sense they have not learned to submit to what Freud calls "alienated labour." They do not desire property enough to gain it by working at a tedious job they are not interested in. Nor do Apaches have anything like our large literature of *romantic songs*. Love songs tend to be joking and boisterous. The court of love is not an Apache tradition; men do not dream of the ideal woman.

(2) The Apaches do not have the concept of the artistic song performer. Anyone who can make himself heard is considered to have a good voice. A bad singer is one who does not know the song.

(3) The Apaches are parochial in their musical interests. They are not curious about songs from other cultures nor do they know them except for (*a*) very active converts to Christianity, (*b*) some of the younger men who know a few Navaho songs with English words, and (*c*) the children in school who all seem to know "Davy Crockett." In the mission services I attended, the lusty singing of the evangelist and his team and the very weak participation or silence of most of the congregation afforded a notable contrast.

(4) The small inventory of musical instruments seemed congruent with the Apache attitude towards property in general. They make a one-stringed fiddle (one of the few instances of a stringed instrument in the native New World), one type of drum, a water drum of buckskin over an iron pot, a flute with three or four holes made of a bamboo-like reed, and the bull-roarer (a flat stick whirled at the end of a cord to make a humming sound). The latter was said by some of my informants to be used in the Crown Dance, but there was none in the Crown Dance I saw at Cibecue in 1953. I had contradictory reports about the use of rattles but did not see any. These instruments are not kept on hand as prized possessions, though the makings of a drum are present in many households. But if a fiddle or flute is to be used, it is made and then quickly gets lost or broken. This is very like the Apache treatment of property in general: it is not something to take trouble over. Even a comparatively wealthy man lives in the same shack and thatched *wickiup* as his poorer neighbors—the only difference seems to be that he feeds more relatives. Livestock is an exception in this general attitude: horses and cattle are greatly prized.

(5) There is little conscious musical training of children. There seem to be no special inducements offered to children to teach them to sing. There are few special inducements offered to children in any area. They grow up to be like their parents without special urging. There is prestige and wealth to be gained by becoming a medicine man, but this is for young men. Children are not supposed to deal with sacred music. There are no songs which are specifically *children's songs*. Children sing simplified versions of the choruses of drinking songs or social dance songs.

(6) There is little singing done by the women. Those few who can join in a healing chant may do so. It seems to be much appreciated by the men but rather rare nevertheless. This is congruent with the general fact that religion is organized and practiced by the men, and relates very well with the fact that more women than men are converts to Christianity and sing in the mission services.

(7) There is little esthetic discussion in our sense. Appreciation of a song is nearly always phrased in terms of understanding it—of knowing what it is for. One or two informants did speak of preferring songs with long choruses and short

verses since these are easier to learn, but the usual preference was for the important healing songs or the sacred songs in the puberty ceremony. This "functional esthetic" is found very widely among preliterate peoples.

SIMILARITIES

Every similarity between cultures contains also its differences and, in the case of music, reminds one that music is far from being a universal language that communicates across linguistic and cultural barriers. For example, there is music with a specifically *recreational* function, as with us. But such songs have sacred phrases in them, and the singing is usually done by men only with women looking on or dancing but not joining in the singing. Love songs, which are also called drinking songs are most used in a drinking party, and this is the usual recreational situation.

There are songs that *children* sing, as mentioned above, but they are not children's songs as such. There is no literature of nursery songs: there is no nursery.

There are a few *obscene* songs, but they are very few as compared with our enormous body of such material. I was able to record only one which had reference to a man who ate too many cedar berries, had diarrhea and soiled his breech clout. There were similar references in some of the clowning that goes on during almost any kind of singing.

There are *gambling* songs intended to ensure success in the game, but they are either brief comical songs about the various animals that participated in a mythical gambling contest or are cast in the form of sacred chants.

Our spell-binding man with the guitar who is irresistible to the ladies has his counterpart in Apache folklore in the man with the flute. Butterfly songs and the flute are supposed to ensnare the senses of women. The element of magic may be said to be present in both cultures in this case, perhaps to about the same extent. However, among the Apaches almost nobody plays the flute today, and I could find no one who knew butterfly songs.

In this brief sketch I have attempted to give a picture of music in the life of the White Mt. Apache Indians. I have compared and contrasted Apache usages and attitudes with our own and I have tried to include enough ethnographic detail to supply the context in which the music is performed. I hope I have given the impression of a people who have much music and who love music as much as we do, but who come at it with strikingly different values and attitudes.

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DOUBLE DESCENT IN AN IBO VILLAGE-GROUP[1]

Simon Ottenberg

The Afikpo Village-Group, an Ibo sedentary agricultural settlement of 23 villages on the west bank of the Cross River in southeastern Nigeria,[2] possess a system of double unilineal descent, including major and minor patrilineages and matrilineal clans which divide into matrilineages. Associational groupings, particularly age sets and grades organized on a village and village-group basis and village men's secret societies cross-cut the unilineal groupings.

The patrilineal groupings are residential and are found within the villages. An Afikpo village, which may contain from several hundred to more than two thousand members, is divided into a number of distinct residential units, known as compounds (*ɛzi*), which are grouped into wards. A compound usually contains a single major patrilineage (*umudi*), though some are composed of two or more unrelated patrilineages.

The major patrilineage varies in size from less than 50 to over 500 members. It has a male founder, and all living members can, in theory, trace their relationship to him. The genealogies go back at least four or five generations; if the lineage is older the earlier generations have been largely lost and fictional relationships are given. Strangers and prisoners of war have been adopted into the lineage, and some Afikpo have changed their patrilineal affiliation by settling in another compound. If a compound contains two or more major lineages each tends to live in one particular section of it, though there is no strict line of demarcation and intermixing is common. The major lineage is divided into several minor patrilineages (also called *umudi*), usually three or four, each tending to reside in one section of the compound. The founder of a minor lineage is often the first son of a wife of the major lineage founder, and the patrilineal descendants of other sons of this woman also belong to this minor lineage.[3]

The most important symbol of the major patrilineage is its ancestral shrine, *ma obu,* located in the lineage rest house in the compound. It is the embodiment of the founder's spirit, the spirits of the founders of the minor lineages, and of all other dead males of the major lineage. Its priest, selected by the lineage elders, sometimes with the aid of a diviner, is generally the eldest man of the lineage, and he does not necessarily have to come from any particular lineage segment within it. He and the male elders are the leaders of the major lineage, for there is no single designated head whose authority is final. Sacrifices are made at the shrine on behalf of the lineage to ensure good crops, at certain lineage feasts and ceremonies, and whenever there is illness, lack of children, warfare, or other troubles involving the group. Individual members perform rituals here for similar reasons whenever they feel the need. While a major function of the ancestral shrine is thus to provide for the prosperity of the group, it also serves to protect lineage members' exclusive sexual rights to their wives. A bride must perform rituals which bind her to the shrine of her husband's patrilineage. If she has sexual relations with another male it is said to be an affront to her husband's ancestors, and she will be fined and rituals of expiation must be performed at the shrine. The protection of these sexual rights is thus a lineage function.

While there is no strict rule of exogamy for the major patrilineage, marriage, in which residence is patrilocal, is generally outside this descent grouping and the compound, most often into other lineages in the village. Again, marriage within the minor lineage is not expressly forbidden, but it is exceedingly rare. The Afikpo phrase these marriage patterns in terms of positive, rather than negative values, considering it more advantageous to marry outside the lineage rather than prohibiting marriage within it.

While most farmland in Afikpo is matrilineally owned, major and minor patrilineages also own or control some land.[4] Patrilineal land has traditionally been obtained in several ways. It may have been cleared by the founder or another member of a lineage, and passed to his sons, rather than to his full brothers or his sisters' sons, a choice he has the right to make; it may have been seized by a group of patrilineages in a village as a result of some dispute and divided among them; or a major patrilineage may have received it on pledge from a matrilineal grouping in return for aiding them in some way. In some cases the major lineage divides the land among its minor lineages, while in others it does not. In either case the method of allocation of farm plots to individual members is the same. The elders of the lineage concerned delegate young men of certain ages to distribute the land under their supervision, according to age, farming skill, and interest. Sons often receive their father's share when he dies, or at least the eldest son is likely to do so; but this is decided in terms of the needs of the whole lineage, there being no strict rule of inheritance of farmland.[5]

More than 35 major patrilineages, or about one fifth of those in Afikpo, possess a shrine, *ɔtɔsi*, which formerly gave its owners considerable authority. Most of these lineages came from Aro Chuku, though some lineages purchased it from these settlers. The shrine owners, whose lineages are found in most Afikpo villages, were local agents of the Aro slave trade and of the famous Aro Chuku oracle, *Ibini ɔkpabe*. Only the shrine holders, known as *amade*, could have doors on their houses, and they were much feared by others. They judged certain types of crimes, including theft, blocking paths or defecating on them, poisoning and sorcery, and cases of murder within the village-group in which there was doubt as to the identity of the culprit. If the murderer was known they helped capture him, holding him for the matrilineal kin of the dead man.[6] Some of these cases were handled by the *amade* of the village concerned, and others by those from one of the five subgroupings of Afikpo villages. Their activities linked together *amade* from unrelated patrilineages in Afikpo.The power of this group was destroyed by the British in the 1930's, and only its shrines and certain associated rituals remain today. Many major patrilineages own other shrines which are used to ensure their welfare, but none provides the authority that this *ɔtɔsi* shrine gave its owners.

Turning now to the minor patrilineages, we have already mentioned their controls over farmland and the matter of marriage practices. The houses of lineage members are also controlled by this descent grouping. Again the minor lineage sometimes owns shrines, but has no ancestral shrine, using that of its major lineage. Its leaders are its eldest males, seldom more than two or three. These men take an active part in the marriage, divorce, title society, and other activities of its members, give them backing when they are engaged in disputes, provide advice on many matters, and represent the grouping to its major lineage. The minor lineages are ranked within the major one according to the seniority of their founder in relation to that of the founder of the other minor lineages, but this ranking carries little weight with regard to authority. It is the larger minor lineages, regardless of genealogical position, which usually have the greatest influence within the major lineage.

There are also certain activities which involve all the members of a compound as a residential unit, whether it contains but a single major patrilineage or two or more unrelated ones, as is sometimes the case. If there is but one in the compound it performs them alone; if two or more exist they perform them co-operatively. Such activities include the settling of disputes and disagreements among compound members, particularly women, by the compound elders, and the ceremonies of certain title societies which are organized on the compound basis. Again, a woman cannot remarry within a compound when her husband dies. Finally, there are ritual activities associated with shrines for the welfare of all compound members, including one for fertility of yams crops, another for human fertility, and a third which protects the compound against evil entering into it.

Though major patrilineages are sometimes related to others in other compounds in the same or different villages only rarely do two or more of these related lineages own property jointly or join together for common rituals. However, the patrilineages in a village, despite the fact that they are often unrelated, combine together for ward and village activities. These include the control of village farmland[7] and palm groves and the protection of the village. Within the village there are age grades, a men's secret society, and title societies, which cross-cut descent ties. In village activities the older patrilineages, and formerly those that controlled the *ɔtɔsi* shrine, have a somewhat greater position of influence than the others, but village affairs are carried out under the direction of the village elders as a whole, regardless of membership in specific lineages. Again, the villages join together in the village-group for certain purposes, such as the control of the central Afikpo market, but the identity of individual patrilineages is lost or blurred in village-group affairs, with decisions being made by the elders' age grades of Afikpo.

There are at least 32 matriclans in Afikpo. These are corporate, exogamous, non-residential groupings which vary in size from several hundred to several thousand members.[8] Each has a female founder and a traditional history. However, no precise genealogy, except of its individual lineage segments, can be given, and here relationships are traced back about four or five generations.

The members of a clan are dispersed in many villages, often throughout Afikpo. Nevertheless they generally know one another, greet fellow-members by the clan name, and take part in activities together. Each clan has a central meeting place where its only shrine, *Nja*, is located in the compound of its priest. The spirit of the shrine is an impersonal one and is not an ancestor. Its priest, selected by the clan elders, often with the aid of a diviner or the Aro Chuku oracle, does not necessarily come from one particular lineage of the clan. If a new priest lives in a different compound than his predecessor the shrine is moved to his compound, though there is a tendency to select a priest from the same compound as the former one. The majority of matriclan shrines seem to be located in the older Afikpo villages.

Clan members gather together at this shrine once a year, during the yam harvest festival, to feast, perform sacrifices for their welfare, and discuss clan affairs. Individuals can offer sacrifices at any time of the year, and the clan members may gather together to discuss clan affairs whenever one of its members is performing an important ceremony. A clan is led by its priest and its male elders. The priest, usually a quiet person, does not travel or take part in most Afikpo political affairs. He advises the elders with regard to ritual activities but does not dominate their decisions, and the clan, like the major patrilineage, lacks a single controlling head, decisions being made by the elders and priest as a group. Among the elders qualifications for leadership are seniority, speaking ability, an artful balance of aggressiveness and tact, and a good knowledge of Afikpo traditional history. Wealth is not a necessary qualification, nor is membership in a specific lineage of the clan. There is no strict representational system of lineages within the clan. If a lineage has three elders all can speak for it, though the opinion of the lineage head, its eldest member, usually carries the greatest weight. If a lineage has no elders, none of its members attend the elders' meetings unless a matter specifically concerning it is discussed, in which case its head may come.

Clan elders take an active part in disputes, such as those over farmland which is directly owned by a member lineage, over which they are the general protectors, disputes over inheritance of certain types of moveable property which pass matrilineally, divorce settlements and other matters involving its members. If illness, barrenness, or other misfortune strikes the clan, the elders and priest secure advice from diviners, the Aro Chuku oracle, or both.

A clan regulation of exogamy is meticulously observed. Violators must perform sacrifices at the clan shrine, whose spirit is said to be angered by their action, and they must discontinue their relationship. Sororal polygyny is rare, the men feeling that much is to be gained in the way of acquiring farm plots and social contacts by marrying into a number of clans. Membership in a matriclan, unlike that in a patrilineage, cannot be changed by an individual from Afikpo, but a person from outside Afikpo may be adopted into the clan by touching the breast of a woman member whose child he is then said to become. It is customary for persons captured in warfare to be so adopted because they increase the size and strength of the clan, and they are quickly given full rights in the group.

The Afikpo matriclans are not ranked in any special order, though the larger ones are more influential, and they do not join together as clans for any common activity. They have no place in the village organization, and they do not act as distinct units in the elders' groupings on the village-group level, though their elders take part in Afikpo affairs. Previous to British contact there was a continual process of fission of clans due to warfare and the difficulty in maintaining contact between clan members in different sections of the village-group, but since about 1910, when most warfare had ceased, they have been reamalgamating into larger and stronger units,[9] and the process of separation seems to have ceased.

One clan, *ibe osim*, was, and still is to some extent, a slave group. It was not permitted by the people of Afikpo to have the usual clan shrine, *Nja*, but it possessed another which was in the charge of a priestess. It owned no farmland, though its members could farm on land belonging to other clans. For this reason many of its men became active traders as soon as hostilities had ceased in Afikpo, and freedom of travel became possible for nonprivileged persons. Although the normal patterns of exogamy and rules of descent were observed no bride price was paid for its women, marriage for them being common-law and often not permanent.[10] The women of the clan, whether married or not, were sexually available to any non-slave. The clan members could not take part in certain village, compound, and patrilineal religious activities. Although there has, in recent years, been a decrease in the sexual freedom toward women of this clan, the other restrictions are still in force.

A clan is subdivided into numerous small lineages, called *ikwu era*, which seem to average between 15 and 30 members and rarely to have over 50. No matrilineal group intermediate in size between these lineages and the clan exists. Though the matrilineage is not a residential unit its members tend to live in neighboring villages, its women most commonly marrying within a local area. In several neighboring villages one clan may have a number of lineages or none at all. Lineage groupings trace relationships back four or five generations to a female founder whose sisters are often founders of other lineages, or at least members of them.[11] The eldest male of the matrilineage is its head, and is in charge of, and responsible for, many of its activities. If the members of a lineage become dispersed, or it is growing in size, the head may permit an older member in a distant area to take charge of the members there, thus forming a sub-lineage. As this sub-lineage develops, its members may move further away from the original lineage area through marriage, and it may continue to grow in size, until it breaks its original ties to become a separate matrilineage. Thus the process of fission is a continual one, and has apparently not been affected by culture contact.

Most Afikpo farmland is controlled by matrilineages. One of these groupings usually owns land within the farming areas of a number of villages. The allocation of this land for farming is under the control of the lineage head. Seniority, interest, and farming skill all play a role in the determination of shares of matrilineal farmland, much as in the case of patrilineal land. While a man sometimes inherits land from his full brother or his mother's brother, the precise allocation of land is up to the lineage head in terms of the needs of the whole lineage. A person generally receives the same share each time he farms the land, though changes are made by the lineage head to account for deaths, increase in age, and the presence of new farmers in the lineage.[12] The lineage head must be a skillful diplomat to balance the needs of the persons using lineage land.

Formerly, when a lineage member was killed by an Afikpo of another clan it was the responsibility of the dead person's matrilineage to capture the killer and either kill him or sell him into slavery. Failing this they captured someone else from the killer's matrilineage, or that group willingly gave up a member they considered undesirable. It was the responsibility of the murdered person's clan to help catch the killer, and often the murderer's clan helped hide and protect him. There was no revenge for the killing of one clan member by another, though sacrifices were made at the clan shrine to appease its spirit, which was said to be angered by the action. Blood revenge has now been suppressed by the British.

The lineage head, and sometimes the sub-lineage head, takes an extremely active role in matters of inheritance, funerals, divorce proceedings, title ceremonies, and other rituals of its lineage members. Members of the clan as a whole participate in these only if they are important affairs or if the persons involved are influential clan members. The lineage head arbitrates disputes between members, tries to make peace in disagreements between a female member and her husband or her co-wives, and advises members on judicial procedures when they are involved in a legal dispute. The head rarely calls the group together, but meets with them individually as the need arises. He may offer advice on religious matters, but he more commonly refers a lineage member to the clan priest or a diviner, the lineage having no shrine of its own. He acts as a representative of the lineage to the clan and to outside social groups, though other lineage elders also represent the group.

One important aspect of Afikpo social life is the relationship between matrilineal and patrilineal groupings, as expressed in cooperative activities and conflicts between the two types of groups and their individual members. In inheritance the head of the dead man's matrilineage is in charge of the distribution of his goods. All money, palm kernels, and other trade goods go to the matrilineage to be divided equally among its male members,[13] except for a few shillings given to the sons to help perform the burial. The man's clothing and other personal goods, and his yams, are divided among the sons by the matrilineage head and help provide for the dead man's family during the period following his death.[14] In pre-British times there was a tendency, which is increasing under conditions of culture contact, for a son to try to hide or deny the existence of his father's trade goods or money, and to keep them for himself. Many inheritance disputes today center about this type of situation, and the British controlled courts, where such cases are often taken, seem to be more sympathetic toward the sons than the Afikpo elders would be. As a result father-son inheritance seems to be increasing.

Again, in the burial of a male elder, and in the subsequent funeral ceremonies, it is the dead man's eldest son and men of his minor patrilineage who are responsible for the proper performance of the ceremonies. The male members of the deceased's matrilineage and the elders of his matriclan, if he was an influential person, come and take part as well. These matrilineal relatives claim that they are prepared to perform the funeral ceremonies if the son is not, though this rarely occurs.[15]

In marriage ceremonies the patrilineal relatives of the couple play important roles, though members of the girl's matrilineage will witness the presentation of the £5 bride price by the groom's representative to his bride's father, who keeps £3 and gives £2 to his wife. However, the bride price is clearly matrilineal in intent since if the father is dead his matrilineage head divides his share among the male matrilineage members, and if the mother is dead her elder sister or other female elder of her matrilineage divides the money among the women of her matrilineage, though the girl's stepmother, if there was one, may be given a share as well.

When a man establishes certain shrines for his own individual use male elders of his minor patrilineage and his matrilineage play important roles, as they do when he takes titles. For his more important titles his major patrilineage and matriclan take part, and the clans of each of his wives contribute money to help pay the costs, and in return he feasts them.

In the family the descent groups of the members play important roles. A husband looks to his patrilineal relatives for support and advice, his wives more to their matrilineal ones. The children, of course, are members of both types of descent groups. In the case of a son the matrilineal groupings are interested in him because he will help to protect and maintain them and their land. The patrilineal groupings take an active interest in sons because they live within this unit and will play an important role in it as adults. Daughters are mother-centered and seem to be more the concern of matrilineal than of patrilineal groupings. Sons also have strong and friendly relationships with their father's matrilineage, receiving farmland from it, and frequently marrying into it.

A matrilineage which is in need of help in a dispute can turn to its clan but often also seeks outside help from a major patrilineage, sometimes one with which it has close associations through marriage, one in which its clan shrine is located, or one with whom its leaders are on friendly terms. The matrilineage pledges land to such a group in return for money and the active aid of its members in the dispute. There is also, however, a noncooperative aspect to matrilineal-patrilineal land relationships. If the pledged land is not redeemed for many years its original owners may lose track of it, and the patrilineage may keep it permanently. At other times the matrilineage and its clan may be weak and the patrilineage may claim that the land always was theirs, pushing the case successfully through the British courts, though if the case should appear only before the Native Authority Court, or the Afikpo elders, this claim would not be so likely to be upheld. Such land seizures occurred before British contact but seem to be increasing today.

Again, sons have the right to use land belonging to their father's matrilineage, though they cannot pass this land on to their sons. However, a favorite form of marriage for a man is with a father's sister's daughter, thus enabling his sons to continue to use this land, since they will be members of his father's matrilineage. Thus matrilineal land rights can pass from father to son. There is some evidence that this practice continues generation after generation, linking two matrilineages and one patrilineal group.

A person may sometimes be caught in the web of disputes between his patrilineal and matrilineal groupings, or between his patrilineal ones and the matrilineal groupings of his wife or father.[16] Each side will want him to join them, but he generally tries to remain neutral; otherwise he may be threatened with sanctions, particularly the loss of farming privileges from the side he opposes. His position is especially difficult if he is a leader in both groups, and he may resolve the matter by trying to bring about a compromise.[17] On the other hand, a person may derive aid from these same groups in disputes in which he is involved, or in political matters, and the successful Afikpo leader is often one who is able to manipulate these for his own ends.

The distinction between the activities and functions of the matrilineal and patrilineal groupings in Afikpo is not quite as clear as it is among the nearby Yakö.[18] Both types of descent groupings in Afikpo control land, though most is matrilineal, and both play important religious and ritual roles, though there are possibly more of these associated with the patrilineal groupings. The patrilineal organizations are one basis on which the authority of the village is formed, but the matrilineal groupings are not concerned here. In the village-group as a whole, neither type of descent grouping dominates, and both are merged in association groupings, particularly the age grades of the Afikpo elders. Inheritance of certain types of moveable property is matrilineal, though yams and household goods pass to the children. The inheritance of land is within the descent groups controlling it, and depends on the needs of the members of the group.

The influence of direct European culture contact has not radically altered the balance between matrilineal and patrilineal groupings, though it has produced changes in them. The matrilineal clans have been strengthened by their amalgamation under peaceful conditions, but today father-son inheritance and the increasing tendency of patrilineal groupings to attempt to seize matrilineal land is weakening their position. They have also lost the power of blood revenge. On the other hand, the *amade*, those patrilineal groupings which held special judicial power, are no longer influential. Also, the authority of the village, based to some extent on the combined activities of its patrilineal groupings, is being usurped by the development of local government.[19] Culture contact has also tended to emphasize bilateral relationships to the detriment of both types of descent groupings. Persons now sometimes attempt to will their property to specific children, and they are often away from Afikpo for periods of time in trade, work, or for other purposes, frequently with their families. Afikpo people seem to be becoming more concerned with their family groupings and less with the larger descent groupings, though no drastic deterioration of these unilineal organizations has as yet occurred.

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Notes

1. This is a considerably revised version of a paper originally read before a conference on "Stability and Change in African Societies," jointly sponsored by the Social Science Research Council of New York and the National Research Council, at Princeton, New Jersey, October 14 through 16, 1953. The research for this paper was carried out between December, 1951, and February, 1953, with the aid of grants from the Social Science Research Council and the Program of African Studies, Northwestern University.

2. The population in 1953 was 26,305. Cf. Nigeria, Census Superintendent (The Government Statistician), 1953, p. 25.

3. If the founder had but one wife with sons the division is between them, each, or at least the oldest few, being founders of minor lineages.

4. Raffia and palm groves are sometimes also owned by patrilineal groupings.

5. A similar procedure occurs with matrilineal land. Cf. p. 477.

6. Cf. p. 477.

7. The land owned by matrilineal and patrilineal groups is located in village farming areas. The village determines which section of its land will be used in a given year and performs rituals to ensure successful farming.

8. Each clan, *ikwu*, is also known by a specific name, prefixed by the term *ibe* (side or part), for example, *ibe aja isu.*

9. In the event of such a reamalgamation, marriages between members of the clans concerned are dissolved.

10. *Ibe osim* men had to pay bride price on free women, but such women, and their kin, were often reluctant to allow such a marriage. Some patrilineages, compounds, and villages forbade their men to marry slave women.

11. The names of the founder's husband and of children of male members in earlier generations are not usually remembered.

12. The head also allocates shares to husbands of lineage women to farm for their sons or future sons, to agnatic half-brothers of male members as a courtesy, to clan members from different lineages who live in the same area and want land to farm, and to widows, friends, and relatives of lineage members. None of these persons have much authority over the land.

13. Trade goods are often sold and the money divided. Clan elders will be given a share of the inheritance if the man was wealthy.

14. A woman's property is divided, on her death, by her male matrilineal head among her daughters.

15. In the burial of a woman her eldest son plays a leading role, and other members of her matrilineage take part, but her patrilineal relatives are little concerned.

16. These seem to be most frequently over land or inheritance.

17. The situation is similar when a person's matrilineage is in dispute with that of his father or one from which he has taken a wife.

18. Cf. Forde, 1939, 1950.

19. Cf. Livingston Booth, 1955; Ottenberg, 1956. In the 1930's and 1940's the patrilineal groupings were to a certain extent the basis for the selection of members to the Native Authority Court and Council.

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LA NOTION DE MANA DANS LA CULTURE HAÏTIENNE

Emmanuel C. Paul

R. H. Codrington qui a découvert dans la culture mélanésienne, la notion de Mana, la définit ainsi dans son livre *The Melanesians* (1891): "Une puissance ou une influence; elle n'est pas physique; elle est en un certain sens surnaturelle, mais elle se revèle dans la force corporelle ou en toute espèce de force et de capacité possédée par un homme. Ce mana ne tient pas à un objet déterminé; presque n'importe quel objet est susceptible de le véhiculer; des esprits le possèdent et peuvent le communiquer. . . . En fait, toute la religion mélanésienne consiste à acquérir pour soi-même ce mana, ou à faire en sorte qu'il s'applique à notre profit."[1]

Depuis, des anthropologies de diverses tendances ont vérifié la notion dans de nombreuses autres cultures sans écriture ou elle se désigne sous des noms différents. Hubert et Mauss dans "Théorie Générale de la Magie" Année Sociologique 3, p. 22, définissent le mana "la force par excellence, l'efficacité véritable des choses qui corrobore leur action mécanique sans l'annihiler. C'est lui qui fait que le filet prend, que la maison est solide, que le canot tient bien la mer. Dans le champs, il est la fertilité, dans les médecines, il est la vertu salutaire ou mortelle."[2]

A s'en tenir aux observations, le mana est une force qui peut être bénéfique ou maléfique suivant l'usage qu'on en fait. Aussi, certains écrivains en tenant compte de son action le considèrent comme la caractéristique dominante des religions des peuples dits primitifs (manisme), d'autres y voient les éléments essentiels de la magie. Que la notion soit observable parmi les cultures ou religions qui, quoique arriérées, ne sont pas moins différentes, cela dénote qu'elle correspond à une philosophie commune qui la transcende. C'est pourquoi, en dépit des erreurs d'interprétation auxquelles se prêtent les observations relatives au mana, nous ne voyons aucune contradiction à l'intégrer dans la philosophie dynamique des bantous dont le R. P. Placide Tempels s'est fait le vulgarisateur et l'interprétateur dans son livre.[3] Il est inutile de reprendre ici les données de cette philosophie, particulièrement l'ontologie des bantous et leur principe d'interaction des forces pour montrer leur harmonie avec la notion de mana. Cela ne signifie pas, comme le dit le Père Tempels que "les bantous sont dynamistes ou énergétistes." Nous ne voudrions pas reprendre ici certaines mésinterprétations de la notion qui reste, en fait, observable un peu partout. Notre rapprochement a seulement pour but de l'intégrer dans un système philosophique qui permette de la mieux comprendre avant de la vérifier dans la culture haïtienne.

II

Le créole haïtien qui demeure le véhicule le plus perfectionné de la culture populaire se révèle relativement pauvre en matière de métaphysique. C'est ainsi qu'il se sert de la correspondance de "âme," soit même pour désigner une toute autre notion spirituelle. Ainsi mana n'a pas sa correspondance en un seul terme. Le mot qui s'en rapproche le plus, c'est "Ouanga" à condition de le prendre dans son sens courant et non péjoratif. Ainsi entendu, il n'est pas identique au "bwanga" des bantous qui s'applique seulement à la thérapeutie. Cependant, "Ouanga" produit les mêmes effets que "Bwanga" "puisqu'il augmente directement la force vitale ou l'être même." Dans le créole courant, on désigne comme "Ouanga" non seulement tout objet qui en sert de substrat matériel, mais toutes opérations tendant à conférer, à utiliser cette force, cette puissance du nom de mana. Nous avons reconnu que le mana peut être bénéfique ou maléfique suivant l'application qu'on en fait, tout en restant en soi la force par excellence et l'efficacité véritable des choses d'après Hubert et Mauss. Sous ces deux aspects, on peut vérifier l'existence de la notion par deux termes qui englobent en indiquant son application celui de Ouanga. Il s'agit d'après plusieurs informateurs de "monter" pour désigner un mana ou un ouange bénéfique et "ranger" pour désigner un ouange ou un mana maléfique.[4] Par exemple, on "monte" un bras pour que la personne batte du tambour avec brio ou pour qu'il ait la puissance de piper des dés avec des chances de gagner. On "range" un aliment ou une boisson pour qu'il rende malade ou tue celui à qui il est destiné.

Cependant malgré ces particularités, à se baser sur les indications de Codrington, Ouanga n'est pas identique ou similaire à Mana. Les "loas" qu'on appelle improprement et sous l'influence du français, "dieux," sont d'abord les premiers, avec la permission de l'Etre Suprême, à conférer cette puissance dans le vodou. Cela est si vrai que le ouanga ne se fait qu'avec leur assistance, grâce à leur propre puissance qu'ils tiennent de Dieu. Et suivant la philosophie vodou si semblable à la philosophie bantoue, les défunts confèrent également cette force, cette puissance spirituelle. C'est à ce titre qu'ils sont les protecteurs des parents vivants. D'où donc la nécessité de prendre sous reserve l'équivalent ouanga pour mana puisque celui-ci a l'avantage d'être plus synthétique et plus générique.

Il reste néanmoins constant que la notion est vérifiable dans le vodou haïtien.

III

Il faut donc s'attendre à une application la plus diversifiée de ouanga sous ses deux aspects "monter" et "ranger." Toujours son recours répond à un mécanisme de défense soit pour se protéger ou assurer son succès sur un adversaire, et cela dans toutes les branches d'activité. Il serait fastidieux de citer ici tous les cas où l'on s'assure la force conférée par ouanga. Nous nous contenterons de quelques exemples pour en donner une idée.

Dans le domaine économique, hommes, animaux, choses peuvent être "montés" ou "rangés," c'est à dire être chargés de force bénéfique ou maléfique. Un agriculteur "monte" son jardin pour le protéger contre des actes de malveillance, l'immuniser contre les maléfices (ouanga au sens péjoratif) d'un voisin, d'un compétiteur. Il peut aussi "ranger" ce jardin en guise de piège pour qu'un mal quelconque arrive à un voleur. Un artisan "monte" son atelier ou une partie de son corps pour s'attirer des clients. Un marchand "monte" son sac qui lui sert de caisse pour se prévenir contre toute soustraction frauduleuse opérée par des moyens surnaturels. Dans les combats de coqs, on "monte" l'animal pour augmenter son agressivité, sa résistance et ses possibilités de gain. L'usage de "monter" ou de "ranger" est courant dans les compétitions sociales où l'on doit faire preuve de puissance. Un homme peut "ranger" la partie sexuelle de sa femme pour prendre au piège un amant, son adversaire. Voilà donc autant de pratiques et de croyances qui démontrent l'existence du concept et l'utilisation de la force en question: le mana.

Qui la confère? C'est là, la spécialité du prêtre ou de la prêtresse du vodou qui sont les dépositaires de ces connaissances et les manipulateurs qualifiés de la force. Il y a bien des situations ou ces Membres du sacerdoce ne croient pas à l'efficacité de leurs opérations. Par affairisme et pour des raisons de prestige, il est plutôt rare qu'ils refusent leurs services, quand ils sont requis. Ce qui est néanmoins constant, c'est que eux, en dépit de tout, et, davantage leurs clients, croient en l'existence, l'utilisation de cette puissance qui se définit par la notion de mana. Nous croyons avoir suffisamment établi la vérification de celle-ci dans le vodou haïtien, grâce aux nombreuses pratiques, formes du ouanga et la représentation qu'on se fait des Invisibles. Tout ceci s'éclaire d'un jour nouveau à la lumière de la philosophie des bantous qu'on peut relever dans notre héritage culturel.

Haïti.

Notes

1. G. Van der Leeuw. *La Religion dans son essence et ses manifestations*. Payot, Paris, 1948, p. 10.
2. Cité par Dr. H. Aubin. *L'Homme et la Magie*. Paris, 1952. Voir également Mauss. *Manuel d'Ethnographie*. Payot, Paris, 1947.
3. R. P. Placide Tempels. *La Philosophie Bantoue*. Paris, 1949.
4. Les mots "monter" et "ranger" sont les équivalents créoles des mêmes mots français avec, à peu près, les mêmes significations, mais en langage profane. Sans notre texte, ils changent de signification; ils ont un sens rituel.

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THE CALF SACRIFICE OF THE TODAS OF THE NILGIRIS (SOUTH INDIA)

H.R.H. Prince Peter of Greece and Denmark

INTRODUCTION

The Todas live in the Nilgiri Mountains of South India. They are a tribe of approximately five hundred people. They have a pastoral economy; they keep buffaloes which they worship as sacred. The milk of these animals is processed in dairies which constitute the tribe's temples. The Todas are vegetarians like other Indians, but in other respects they are completely aberrant, and differ in very many ways from their neighbors. Their origin is unknown and much speculation has taken place as to where they may have come from. Dr. M. B. Emeneau of the University of California in Berkeley has said that their language is a form of Kanada. I, however, found, during my fieldwork among them in 1949, fourteen apparently Sumerian deities in the *Kwarshms* or dairy prayers of the tribe; Emeneau has contested my findings on the basis of linguistics. (A recording of the Kwarshm of Ön, God of the Underworld, was then played.)

I. THE CALF SACRIFICE

Incidental to my study of polyandry, I chanced upon the very secret calf sacrifice reported by W. H. R. Rivers in his book, "The Todas," 1906. Dr. Rivers was able to photograph the ceremony which had only been mentioned before by Harkness in 1832 and by Muzzy in 1844. Thanks to the kindness of my informants, I was given an opportunity, in 1949, both to film and to sound-record the calf sacrifice of the Todas for what I believed to be the first and only time. I will show a film of this ceremony, but before I do so, I would like to give some explanations as to what is looked upon as the reasons for the sacrifice.

Rivers' informant, Teitnir, said that Kwoto induced the Gods to eat the flesh of a male buffalo calf by throwing a portion of it in the midst of their assembly when he flew over them in the guise of a kite, after having killed the animal with today's ceremonial. To the gods he said: "I have partaken of it and, if your counsels are to be right, you must too." The gods blamed Kwoto for this. Whereupon, he reasoned as follows in typical Toda fashion: "I am not to be blamed; if you blame a man who is not to be blamed, then why do you not eat flesh which is not to be eaten?" After Kwoto had tied down the sun, as Toda myth recounts, and thereby convinced the gods of his supernatural powers, they agreed to partake of the sacrificial calf. Since then the Todas have carried out the ceremony and eaten the victim every year.

My informant, Ujar, in 1939, had already revealed to me the following very secret story: Tögart, god of the monsoon, and his wife, Karj, were eating buffalo flesh at Karjemur on the Kenetoli River (Avalanche). The monster Arkodj discovered them and ate up Tögart as well as a Kota who was going home to his village, and a jungle goat. Arkodj opened his mouth slightly, whereupon the goat jumped out again. Tögart and the Kota were, however, swallowed whole and, while in the monster's stomach, the god asked his companion to give him a knife to cut himself out. This he did and they were both saved in this way; since that day the Kotas have been authorized to make iron tools for the Todas. This is the origin of the calf-sacrifice which is done every year in July (Rivers has October), and not only once but many times (I suspect in order to rid themselves of a surplus of male buffalo calves) to encourage the monsoon by offering meat to Tögart and Karj. Kwoten (of the Pan clan) and Teikuteihi (of the Tarar clan) were the first to do it.

My interpreter, Kanvarsathi, in 1949, told me after I had seen the ceremony that it was held to propitiate the goddess, Tögörsh, who lives on a mountain near Mukurti Peak on the Malabar side of the Nilgiris. He assured me that this goddess of whom I had not heard before had nothing to do with Notirsh (of the clans of Melgarsh, and Kuur; abode: Snowdon) and Kulinkars (of the Karsh clan; abode: Mukurti) as Rivers has it. She must be propitiated for buffaloes to give calves and milk and be kept from ill health and the depredation of tigers.

My wife's informant, Ǒknarsh, told her that the calf was sacrificed to rid the tribe of all evil, that it was in fact a scape-goat—or scape-calf, if you prefer.

II. THE SACRIFICE AS I SAW IT

On Monday, August 24, 1949, I was shown the sacrifice at the Kuur *erngar* (sacrificial spot). Rivers has it that the ceremony takes place on Sunday, Wednesday or Thursday, but I could find no confirmation of this.

A dreadful quarrel took place among the Todas as soon as it was known that I was to be shown the ceremony. Pilkhliud of Kuur, who was to stage the sacrifice for me, was attacked by Mutirshk of Karsh and his sons. They were jealous and afraid that to reward him for his kindness to me, I would arrange for him to be appointed *monegar* (government representative with the tribe); they also said that the sacrifice was secret and should not be shown to outsiders because, also, it might confirm the accusation of the neighboring Bagadas who, quoting the Kotas, said that Todas were also flesh-eaters and, therefore, of low caste. Kanvarsathi was obliged to work on them until 2:00 A.M. that night in order to patch up the quarrel.

The next morning, at 10:00 A.M., we started from Kuur, Pilkhliud, true to his promise, but still very sleepy, leading the party. This consisted of Pershgiar, the *palikartmokh* (or dairy-temple attendant) of Kuur, whose grandfather, Punatvan, had already shown Rivers the ceremony, of Punoi, son of Sadigur (Genealogy No. 56 in Rivers), and of Panghwur, an older man, paternal first-cousin of Pilkhliud. The calf was driven by Pershgiar, and we went some distance into the *shola* (wood) to the *erngar*. (In order to give visual illustration of the calf-sacrifice, a 16 mm., black-and-white film of the ceremony was then projected with the following commentaries:)

1. A fire is first produced by friction, which is an archaic way of doing this but essential for all religious Toda functions. Punoi operates the swivel, old Panghwur assists him, while Pershgiar looks on.
2. Panghwur takes a piece of ignited cloth.
3. Punoi blows on the smouldering bit of cloth in order to light the long logs of the fire; the fire in this ceremony is always made with long pieces of wood disposed side by side.
4. Pershgiar is seen with the calf; he holds some leaves and a log of the *tur* tree, a species of the Nilgiris.

5. A close-up of the buffalo calf.

6. Pershgiar now passes the log and leaves over the calf's back reciting the while Kwarshm appropriate for this occasion: "*Atthkark per ma*" (the prayer of the Kuur mund (village) which means "Kuur to, may there be increase"). My interpreter, Kanvarsathi, explained here that the Kwarshm of the goddess Tögörsh was also said here: "*Kajnerj-Ontkwur*," ending with "*Tögörsh-urshtma*" (meaning: "Stay forever so that it should finally remain with her"). I did not observe any dropping of leaves here as described by Rivers.

7. Old Panghwur holds the calf.

8. Pershgiar kills the calf by clubbing it on the head with the *tur* log.

9. He finishes it off.

10. Panghwur in turn finishes it off. There is no beating here on the testicles as described by Rivers and which he thought to be an Indian way of dispatching a sacrificial animal.

11. The left fore-limb is cut in order to gather blood on a *tur* leaf, which is then burned in the fire. I did not observe any of the passing of the log and leaves over the body of the calf as in Rivers.

12. Punoi starts cutting around the neck to detach the skin from the body. He makes no incision down the belly.

13. The left hind leg is next operated upon.

14. The animal is shown skinned, lying on its skin. The latter is then removed from under it and hung on a tree, not laid on the ground for soaking bits of flesh in the blood as in Rivers.

15. The hind-quarters are cut off.

16. The liver is taken out.

17. The stomach is removed; if there is milk in it, this is considered very auspicious. The bowels are taken out and thrown away; they are not spiked as Rivers has described it.

18. Punoi starts fixing various pieces on wooden spikes called *ko* (there are not fifty of these as in Rivers); Pershgiar comes and helps, while Pilkhliud looks on. (The names of the various pieces are given in Rivers, pages 281–2: shoulder, trachea and larynx, upper part of hind limb, gall bladder, bladder, pelvis, sternum and part of ribs, heart, lungs, spleen (this was burned and not given to a cat as it should properly be, since Pilkhliud admitted that he had forgotten to bring the cat along), lower backbone and ribs, upper part of backbone and ribs).

19. The head is next spiked through the left nostril by Pershgiar.

20. He places the pieces around the fire to roast.

21. He throws small additional pieces of meat into the fire, calling out: "*Ŏn*" and "*mar-mar*" (the latter exclamation supposedly that of a calf calling out). Rivers has it that the words spoken here are: "*Notirsh per ma, man,*" and he also says that additional coals are thrown on the fire at the same time, but I found no indication of this, nor that these things took place in the order described by him.

22. The head of the calf is placed on its spike at the head of the fire and it was explained to me that it should not call out as it supposedly did once, which would be a very bad omen.

23. The *palikartmokh*, Pershgiar, clad in the black loin cloth which is that of his office, circles the fire anti-clockwise, throwing in more pieces of meat and calling out "Ŏn" again.

24. The head is singed in the fire as it should be and then thrown away in

the bushes. Finding this a waste of good calf's head, I retrieved it later and took it home to eat it French-style "vinaigrette."

25. The roast pieces being ready are taken to the *kirpali* (village dairy temple) of Kuur. They are not eaten on the spot as Rivers says.

26. Pershgiar is seen taking them in through the narrow door of the *kirpali*, where they will be eaten by him and by other men, but never by women. Rivers says that other parts are taken to the huts to be eaten by all, but I could find no confirmation of this.

There follow views of the Kuur mund, of Pilkhliud and Punoi, of their children and of their huts.

A colored view of the sacrifice is shown; the colors are very vivid, and it is perhaps preferable that the film shown was a black-and-white one, as otherwise it would have been too gruesome.

III. ADDITIONAL COMMENTARY

Pilkhliud, some days later, was attacked at a Toda funeral which I attended. He was accused of having unlawfully shown me the calf sacrifice, and my wife was obliged to intervene to save him from a severe beating.

My interpreter, Kanvarsathi, called the ceremony *ervan* (from *er*, male buffalo calf, and *pan*, ceremony), not *erkumpthpimi* as in Rivers.

My informant, Ujar, told me that Kotas can attend the calf sacrifice, but that they are not allowed to touch anything.

Another informant of mine, Wunkwur, told me that the Badagas have a similar calf ceremony (possibly Breeks' *kona shastra*, described by him in 1873) which takes place at the end of August. They hold it in honor of their gods, Talsurunar and Talsururen, who live in a cave below the Norsh mund. The calf is supplied by the people of Norsh. The calf is killed and its blood drunk by the Kurumbas today, for the Badagas do not want any longer to do this.

The question arises as to the origin of the calf sacrifice of the Todas. Is it their own or is it Indian? Is it a Semitic *Korbon* or is it Sumerian? The Toda gods are very hazy and a study would be well worth while. The Badaga parallel described above would seem to confirm that the sacrifice is Indian, as Emeneau would no doubt like it to be. But then, cannot it equally be said that it is the Badagas who have borrowed it from the Todas rather than the latter from them?

Rivers' explanation, as we have seen, attributes the ceremony to Kwoto. But the latter is a man-god hero of the Todas to whom very much is attributed anyhow. Tögart and Karj seem to be very little-known Toda deities, as is the monster, Arkodj, too. Kwaten and Teikuteidi are other men-god Toda heroes alleged originators of many present-day customs. Tögörsh is not mentioned anywhere, but I would say that she is probably the Toda equivalent of the Indian goddess, Kali, whose worship is so prevalent in neighboring Malabar. No Toda mentioned Notirsh and Kulinkarsh to me in connection with the calf sacrifice, and I could only find them in Rivers. The mention of Ŏn seemed to me new and significant.

CONCLUSION

My impression, although this is purely a guess, is that the Toda calf sacrifice is an Indian-like propitiatory ceremony to appease a fierce Kali-type deity, or Ŏn, god of the Toda Underworld.

But it may, of course, be built up on something very much older.

Thus, the *Teututushchi* ceremony, in which the Todas light a fire at the foot of the Nilgiri range of mountains in October in order to make grass and honey plentiful (described by Rivers, page 290) appears to be very ancient in origin. In the Kwarshm recited at this occasion I found the name of the Sumerian goddess of the mountains, *Ninkurshag*, mentioned as follows: "*Kirjen-Pirjen Ördh, Terkish-Tishkt Ördh, NINKURSHAG Ördh An, Puth Ördh An, cet.*"

(A recording of this Kwarshm was then played.)

The Toda informants from whom I obtained this denied all knowledge of what the meaning of Ninkurshag was and were very impressed when I told them that it was the name of a deity five thousand years old. They said that they only repeated this word because it had been taught to them by their predecessors.

There is much mystery still in Toda religion, and investigation of the possible antique link between South India and ancient Sumeria should, I think, be followed up, perhaps on the lines of the latest finds of the Danish expedition to the island of Bahrein, leader Professor Glob.

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POLYGAMIE ET SES PARTICULARITÉS

Sliman Rahmani

Il y a quatre ans, au IVème Congrès International des Sciences Anthropologiques et Ethnologiques de Vienne, j'ai résumé une étude sur le Mariage des Autochtones d'Algérie,—des Kabyles en particulier.

Avant d'aborder le sujet de ma communication sur "La Polygamie et ses particularités," je donnerai tout d'abord un petit aperçu sur la population berbère de l'Algérie, sans parler toutefois des berbères marocains et tunisiens.

Les premiers habitants de l'Algérie étaient berbères. Quant à leur origine elle se perd dans la nuit des temps.

Cette contrée de l'Afrique du Nord a été comme tous les pays du monde, envahie à diffèrentes reprises. Les Romains y restèrent assez longtemps.

Au VIIème siècle, les Arabes, venus d'Arabie, de Syrie, conquirent l'Afrique en commençant par la Tunisie.

Ils islamisèrent, non sans difficulté, la population berbère. Aussi, devant la poussée arabe, le parler berbère subit des altérations, et fit place à la langue arabe qui gagna de proche en proche le Sud, les Hauts-Plateaux et les villes, à tel point qu'un grand nombre de berbères arabisés renient leur origine et se disent arabes. Certaines régions, quoique islamisées, conservent encore leur parler, voire même leurs mœurs et coutumes.

Parmi ces régions conservatrices, citons les Kabyles, habitants des deux Kabylies: la Grande Kabylie située dans le département d'Alger, et sa voisine, la Petite Kabylie dans le département de Constantine.

Ces deux régions sont montagneuses et d'un accès difficile.

Les Kabyles, belliqueux (ils aiment faire parler la poudre), et fier de leur indépendance qui dura des siècles, se sont retranchés sur les pitons et les flancs des montagnes pour mieux se défendre contre l'ennemi.

C'est une population très dense, laborieuse qui s'expatrie facilement à la recherche du gain.

Il y a aussi les chaouïas de l'Aurès dans le Constantinois; c'est une vaste région montagneuse.

Les Mozabites, dans le Mzab (sud algérois), dont les origines sont assez curieuses et assez longues à expliquer, sont berbères.

Les Touaregs dans le désert sont les seuls berbères qui conservent encore de nos jours un mode d'écriture en caractères "tifinar." Les hommes sont voilés tandis que les femmes ne le sont pas.

Il y a aussi d'autres tribus berbères disséminées dans les trois départements algériens.

Ces berbères islamisés conservent, malgré la loi coranique et la loi française, leurs mœurs et coutumes, et chez les montagnards les plus reculés, leurs kanouns ou droit coutumier. Ils sont sunnites mais avec quelque entorse au droit musulman.

RAISONS QUI MOTIVÈRENT L'INSTITUTION DE LA POLYGAMIE CHEZ LES MUSULMANS EN GÉNÉRAL

Le Prophète Muhammed (que les prières et le salut soient sur lui), humaniste, féministe et grand psychologue connaissait les penchants et les travers d'autrui,

. . . . des pays chauds surtout. Il toléra donc la polygamie mais avec bien des réserves. La polygamie est une tolérance et non une obligation.

En instituant la bigamie et la polygamie limitée à quatre femmes légitimes, le Prophète a voulu éviter à ses fidèles tout dérèglement dans les mœurs et leur permettre d'avoir une nombreuse descendance. Les femmes, d'autre part, toujours en surnombre par suite des guerres qui décimaient et déciment encore les guerriers, pouvaient se marier ou se remarier plus aisément. L'Envoyé de Dieu a voulu également réduire le nombre des divorces qui séparent les enfants de leur mère. Remarquons en passant que la garde des enfants une fois sevrés est confiée au père et non à la mère.

Pour mémoire, rappelons qu'avant l'Islam les Arabes idolâtres ou polythéistes avaient des mœurs que nous réprouvons aujourd'hui. Prenons pour exemple les parents qui enterraient vivantes leurs filles même après plusieurs années d'existence pour les soustraire, disaient-ils, à la misère et à la honte. Cela se passait surtout dans les familles nombreuses et nécessiteuses.

L'inceste et la polyandrie étaient chose courante. La femme du frère aîné servait d'épouse à ses beaux-frères. Comme dans la Bible, il suffisait que l'occupant du moment déposât son bâton devant la porte pour prévenir et en interdire l'entrée.

Pour mettre fin à la barbarie des parents, aux mœurs dissolues des gens, le Prophète prit des mesures sévères, supprimant ou atténuant selon les circonstances, telle ou telle pratique ancienne.

Pour revenir à la polygamie, un sage, partisan de celle-ci, aurait déclaré: Voyons! Prenons pour exemple les quatre femmes légitimes d'un mari dont la première a ses règles, la seconde en couches, et la troisième malade! Dans les trois cas, l'homme doit s'abstenir de tout rapport intime avec elles: la première, tant que le flux sanguin n'aura pas cessé et qu'elle n'ait pris un bain purificateur, et la seconde durant les quarante jours qui précèdent la cérémonie des relevailles. Quant à la troisième, son état de santé ne lui permet pas de recevoir son époux. Seule, la quatrième, valide, pourra s'occuper de la maison et de son mari.

Pour expliquer le cas des deux premières épouses reportons-nous à la tradition musulmane ou sunna. Celle-ci considère comme étant impures la parturiente et celle qui a ses règles. Il leur est interdit pendant cette période de faire la prière, dejeûner pendant le Ramadhan, de ne pas approcher leur mari.

Dans certaines familles, la femme en état d'impureté ne doit pas préparer les aliments, ceux-ci paraissent insipides.

Par ailleurs, on prétend que l'époux qui, malgré cette interdiction, a des rapports sexuels avec son épouse en état d'impureté risque, en cas de conception de celle-ci, d'avoir un enfant vicieux (mis leḥram,—en arabe: uld ḥram—"l'enfant du péché").

Autre problème.—La femme stérile risque de provoquer chez son mari l'envie de prendre une seconde épouse dans le but d'avoir des enfants mâles surtout. En effet, lorsqu'au bout d'un certain temps de mariage on s'aperçoit que la femme n'enfante pas, toute la famille, les voisins et les amis n'ont de cesse d'inciter le mari à se remarier. Deux cas de conscience se présentent à celui-ci: divorcer et fonder un nouveau foyer ou garder sa première femme pour en prendre une autre, puis, si besoin est, une troisième et même une quatrième.

Dans bien des cas, la femme stérile ou n'ayant eu que des filles recommande à son mari de se remarier dans l'espoir de le voir devenir père d'un ou plusieurs garçons qui perpétueront la famille. Et combien y en a-t-il qui ont élevé avec tendresse les enfants donnés par la co-épouse (takna plur takniwin).

Dans les temps bibliques, Sarah qui se lamentait de ne pouvoir donner un fils à son vénérable époux, le patriarche Abraham, ne finit-elle pas par le convaincre et le décider à épouser son esclave Agar! Vous devinez la suite, car chez la femme, même vieille, la jalousie ne perd pas ses droits. En effet, un peu plus tard, Sarah, devenue jalouse, mit en demeure Abraham de choisir entre elle et la co-épouse Agar et son fils Ismaël. Abraham, par respect pour Sarah, et pour obéir à la volonté divine, finit, après bien des protestations, par abandonner dans le désert d'Arabie sa seconde femme et son fils.—C'est la Mecque actuelle, le lieu saint par excellence des Musulmans.

Enfin, chez la plupart des bigames une seconde femme est nécessaire parce que la première est infirme ou incapable de diriger la maison.

Pour les riches, la polygamie est due parfois à l'orgueil qui les pousse à étaler leur opulence, ou simplement à des raisons politiques et d'influence. A noter qu'en Algérie, avant l'arrivée des Français, les tribus étaient souvent en guerre. C'est une des raisons pour laquelle les grandes familles recherchaient des alliances pour repousser l'ennemi et maintenir leur prestige.

Sur un autre plan, suivant le Dr. mexicain Anselmo Numez, la majorité des hommes doivent avoir plusieurs femmes pour ne pas vieillir.

"Se marier à une seule, assure-t-il, est contraire à la loi naturelle organique. La monogamie pour les hommes mène notamment à l'hypertension, à l'artériosclérose, à la névrose et à la folie.

"Les femmes, ajoute-t-il, n'ont, par contre, besoin que d'un seul homme." (1954.)

Nos vieux arabes n'ont pas attendu que le Dr. Anselmo prévienne les hommes monogames de ce qui les attend pour prendre les devants, et cela depuis fort longtemps. J'ai connu des hommes qui, malgré leur âge avancé, 70, 80 et 90 ans, plusieurs fois arrières- grands-pères, renouveler leur couche en se remariant avec des jeunes filles de 15, 16 et 18 ans, et avoir des enfants avec elles. Ceux du Sud notamment battent ce record.

DIFFICULTÉS ET INCONVÉNIENTS DE LA POLYGAMIE

La religion musulmane recommande de se contenter d'une seule épouse surtout lorsqu'un mari n'a pas les moyens d'entretenir convenablement ses femmes légitimes en assignant à chacune d'elles un appartement particulier et des servantes, et quand il n'est pas sûr d'être juste à leur égard.

Voici ce que dit à ce sujet le Coran (Les Femmes, chap. IV, v. 3): "Si vous craignez de n'être pas équitables envers les orphelins n'épousez, parmi les femmes qui vous plaisent, que deux, trois ou quatre. Si vous craignez encore d'être injustes, n'en épousez qu'une seule ou une esclave. Cette conduite vous aidera à ne pas être injustes."

Le mari ne doit pas avoir de préférence au point de négliger une épouse pour une autre. Les conditions imposées à un polygame sont tellement rigoureuses que beaucoup y renonceraient s'ils devaient appliquer à la lettre les prescriptions du Coran et des ḥadiṯs.

Sur ce point, le polygame aisé, lettré et craignant Dieu, peut à la rigueur se montrer équitable envers ses femmes en leur fournissant des logements à part et des servantes afin d'éviter le contact permanent entre elles et les scènes de ménage. Quant à la jalousie n'en parlons pas! Combien y en a-t-il qui sont ignorants et de condition modeste? Les co-épouses sont là dans l'unique pièce de la maison ou de la tente, mêlées aux enfants des unes et des autres! Il est vrai que le commandement intérieur revient à la belle-mère ou à défaut à la plus ancienne, et qu'elle peut les mener à la baguette! Mais, malgré cela, que de scènes, et que ne subit pas le pauvre mari que chacune de ses femmes cherche à accaparer, à le détacher de ses autres épouses en recourant aux amulettes des talebs (hommes lettrés en arabe), à des sorcières et à des filtres de toutes sortes! . . .

Des auteurs orientalistes bien connus tels que Perceval, Barthélemy-Saint-Hilaire, le Play, Gustave Le Bon, pour ne citer que ceux-là, ne désapprouvent pas la polygamie; ils la trouvent plus logique et plus saine que ces ménages à deux avec à côté une ou plusieurs maîtresses. Des rois de France n'imposaient-ils pas à la reine, à la cour et au public une favorite en titre?

En remontant bien loin, les Vandales, les Goths, les Germains, les Francs, les Normands, les Saxons, étaient polygames et le sont restés bien longtemps après leur conversion au Christianisme.

Charlemagne avait huit femmes dans ses huttes en terre d'Aix-la-Chapelle, et les chroniques du temps nous ont conservé le souvenir de scènes de pugilat, où, malgré son gantelet de fer, le Grand Empereur n'avait pas toujours le dessus.

De toute façon la polygamie a été de tout temps très peu pratiquée en Kabylie. Le Kabyle, pointilleux, place son amour-propre au-dessus de tout. Aussi, préfère-t-il répudier la femme stérile ou celle qui ne lui convient pas plutôt que d'avoir plusieurs épouses. Il évite ainsi toutes sortes de discussions dans le ménage, et un entretien trop coûteux.

Un dicton kabyle dit: "Le bigame est épuisé par les soucis." (bu snaṯ, hemm ifna-t).

D'autre part il met sa santé à l'abri des entreprises des co-épouses jalouses qui, pour se faire aimer exclusivement du mari, lui font absorber chacune de son côté, des filtres composés d'ingrédients plus ou moins nuisibles à l'organisme. . . . Il ne faut pas croire qu'il n'y a pas du tout de polygames parmi les Kabyles, il y en a mais très peu en comparaison avec les Arabes.

Notons que l'émancipation des femmes musulmanes dans la haute société surtout se développe de plus en plus. Aussi, depuis quelque temps, les Pakistanaises, les Egyptiennes par exemple repoussent le port du voile et partent en guerre contre la polygamie. Quant à la Turquie c'est chose faite!

Le concubinage est inexistant, il n'est pas toléré par la coutume kabyle.

Les Touaregs de l'ouest, nobles, Imrad, Iradjenaten, sont monogames. "Un homme qui épouserait deux femmes à la fois, disent-ils, attirerait la mort sur sa tente" (Les Touaregs de l'Ouest, par le cap. H. Bissuel—Alger. 1888. p. 107).

Par ailleurs, et d'après Louis Milliot (La Femme musulmane au Maghreb, p. 119), "Une femme targuie ne consent jamais à un partage des faveurs maritales, même avec une concubine négresse."

Il y a un fait certain, c'est qu'actuellement la bigamie et la polygamie sont en nette régression en Kabylie et dans le reste du pays algérien. Les principales causes de celle-ci sont dues à l'évolution progressive de la population, au contact fréquent des Européens et à la cherté de vie toujours croissante.

"Le dénombrement de 1948 indique, sur 1,281,399 hommes mariés, il y a 37,000 bigames, soit 2,89%, et 1,806 polygames, soit 0,14%.

"Les statistiques montrent que la polygamie est en voie de disparition: de

1886 à 1948, alors que la population algérienne passait de 3 millions à 7 millions ½, le nombre de polygames tombait de 89,000 à 38,900." (Voir Pierre Guiho. "Les conflits entre la loi française et le statut personnel des musulmans algériens en matière de mariage." In Annales juridiques, politiques, économiques et sociales publiées par la Revue Algérienne—Rev. de la Faculté de Droit d'Alger—1 année nos. 3–4. p. 161. no. 37—Libr. Ferraris, Alger.)

Alger, Algeria.

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PRODUCTION, DISTRIBUTION AND POWER IN A PRIMITIVE SOCIETY

Marshall D. Sahlins

This paper proposes to examine the relationship between certain features of production, goods distribution and political power in a kin-organized society, that of the island of Moala in the southeast of Fiji. The ethnographic materials to be utilized were collected by the author during more than ten months of field work in 1954 and 1955.[1] Moala is an island 24 square miles in area with a population of approximately 1200 (all Fijian), settled in eight villages. The major units of social organization are patrilineal descent groups, various segments of which are localized in the villages. Leaders of certain descent-group segments operate as territorial chiefs within the unified, island-wide polity. Principal subsistence activities are growing root crops by swidden agriculture and raising wet taro. The culture has been subject to European influences. Involvement in the Colony's monetary economy has been increasing of late. But thanks to British indirect rule, the native political and economic organizations are still operative, and their inter-relationship is clear.

It will be shown that in Moala the ability of chiefs to exercise authority depends markedly on their ability to utilize prevailing distributive forms in order to dispense goods liberally in the community. Since strict reciprocity is not characteristic of these forms, those who play dominant roles in distribution are thereby impoverished rather than enriched. Furthermore, the possession and consumption of vital goods tends to be equalized rather than unequalized. While this result is paradoxical from a modern viewpoint, in Moala it is adaptive and advantageous. For production in the local environment regularly and expectably engenders large and critical differences in the quantities of such essentials as foods acquired by individuals, families and villages. Thus at the same time the system of distribution is a basis of political authority, it also constitutes an adjustment to local ecological circumstances.

Outside of the simple pooling of resources within households, the predominant method of goods distribution in Moala is known as *kerekere*. To kerekere is to solicit an item from a relative. Almost any type of goods, *viz.* food, money, mats, tools, clothes, tobacco, canoes and so forth, can be requested by kerekere. One may also so solicit labor services, such as help in garden work, or the services of the several types of part-time specialists. It is possible to kerekere usufructory rights to land from relatives in patrilineal descent groups (the land-holding units) other than one's own.

There are two critical characteristics of kerekere. The first is that the person making the request and the potential donor should be kinsmen. This does not actually restrict the operation of kerekere, for the existing classificatory kin terminology permits very widespread extension of kin ties. The avenues of kerekere open to any individual encompass just about everyone he meets. But always the ethic of mutual aid and solidarity inherent in kin relations rationalizes its practice. The second critical characteristic is that one should solicit a good or service only if there is a genuine need. One should not and usually does not ask for something in pursuit of greedy desires (*kothokotho*). By the same token, the only legitimate reason for refusing a request—except lack of the solicited item—is that the donor, by giving, would place himself in need. Even in that case a refusal is sometimes difficult and is usually awkward.

In a few forms of kerekere there is direct reciprocation for solicited items. This occurs primarily by way of recompense for solicited services rendered, or where goods have been specifically requested for short-time use only and then returned. But in most instances of kerekere, especially of goods, there is not even an implicit understanding that the recipient will return the goods or their equivalent by his own initiative. It is only implied that said recipient is bound (in the vague future) to honor any request made by the donor if the latter chooses to kerekere in return. An obligation to reciprocate merely exists insofar as the recipient of an item is made more accessible than otherwise to a future request by the donor. Should the donor kerekere in return, he will not ask for things equivalent to what he gave—only for that of which he is in need. The obligation to reciprocate is fulfilled whether or not the items secured by the original donor in a return kerekere are equal, greater or less in value to that which he gave in the first instance.

Reciprocity may not occur at all. The donor of a thing might choose not to kerekere from the person to whom he has given something. Strict reciprocity is in fact not encouraged by an implication of prestige which attends the giving of a thing and an implication of inferiority attending soliciting a thing. To give is to indicate "strength," productive ability, while to ask is to admit "weakness," inability to produce sufficient for one's needs. A man usually humbles himself before a donor in making a request, while the latter assumes a dominant, patronizing posture. Moreover, it is not necessary for a man who has received a good from another to reciprocate (through the donor's kerekere) before requesting something else from the same person.[2] There may be a continuous series of one-way transactions between two people which tends to differentiate them in terms of social superiority. This, as will be shortly described, is the aspect of kerekere which lends itself to political manipulation. At this point, I would note that being based on considerations of need and allowing for a one-way goods flow, kerekere functions to equalize consumption in the face of possible differences in production.

Goods distribution has a specific relationship to chieftainship, the nature of which tends to strengthen chiefly authority. The greatest moral obligation incumbent on a chief is that of giving material aid to his people. Chiefs of villages and particularly the island paramount in the ranking village, Naroi, therefore become subject to a great deal of kerekere. But there is a critical difference between kerekere from a chief and kerekere from others. For a chief almost never solicits goods in the way of return for goods given by him; it would be a sign of "weakness" on the chief's part. How then is a chief able to satisfy the numerous requests which come from his inferiors? The answer is, primarily through the productive labor of himself and members of his extended family. The chief's family must work mightily to insure that the reserve on hand is sufficient to allow the chief to function adequately in his distributive capacity. If these resources should fail, the chief may send to a subordinate chief or to close relatives for the items required to satisfy a kerekere. Although identical in effect, chiefly demand is not considered kerekere—nor is it delivered as such—rather it is a request for what is his inherent due. So from household resources and occasionally from goods procured outside the household, chiefs honor the requests of those who come to them when in need. In return, chiefs reap the rewards of giving: the prestige, the acknowledged superiority, and the loyalty and continued support of the people. Thus political position is reinforced by a measure of economic control.

Succession to office may be influenced by the distributive role which a chief should perform. The primary qualification for succession to a chieftainship is high rank according to seniority of descent within the patrilineal group traditionally filling the position. But senior men descending in the line of first sons of former chiefs are sometimes passed over in selecting a successor because they lack productive ability or lack a large household—in short, because there is doubt that they could fulfill the economic obligations of chieftainship. A man of junior status having these qualifications may accede. Thereby is born a conflict between high rank and high office. Rivalry for power grows between the descendants of the ousted senior line and the offspring of a chief of junior rank, and this rivalry may continue over many generations. Every high chieftainship in the island polity is the center of a controversy of this sort. The rivalry is only intensified if the higher-ranking group manages to recapture leadership. The group out of power at the moment will repeatedly fail to comply with chiefly wishes and neglect to give the chief the proper forms of respect. At the same time, the chief and his nearest relatives hesitate to take suppressive action. In fact to take any measures that may be construed as oppressive would merely serve to arouse, possibly to violent action, those around the chief who are hostile to his rule.

An even more direct debilitating influence on chiefly power arises from the character of the distributive system. It is critical to note that distributing goods widely by kerekere is not a chiefly prerogative; anyone capable of doing it can and will.[3] Non-chiefly families, especially large ones, are often able to accumulate ample reserves of vital goods. They become subject to kerekere by less fortunate relatives, but with large reserves it is unnecessary to kerekere in return. The leader of such a family gradually accumulates prestige through his distributive activities. Moreover, he weans loyalties and support away from the local chief, since to some degree the chief is not given the opportunity to distribute goods. The chief's authority is challenged and weakened, for his distributive function has been usurped. Again power rivalries arise, and they are just as intense as those created by a succession of a person of inferior status to a chieftainship—in fact, they may lead to such a succession. There is mutual hostility between chiefs and those who challenge their distributive position, accompanied by widespread disinclination to acknowledge the status or even the rule of the chief. The degree of autocratic control that the chief might otherwise exercise is thereby checked.

We have seen the ways by which the system of goods distribution may both reinforce and limit political power. I shall now deal with an aspect of the cultural ecology which helps to account for the predominance of kerekere as a distributive mechanism. It is not implied that features of vital production in the local environment come first in an historical sense and the method of distribution developed in Moala as a response thereto. Certainly social, ideological and other factors—even the distributive system itself—influence the manner of adaptation to the environment. It is irrelevant, however, in this context how one accounts for the methods of technological activity. Given the environment and the way in which it is exploited, certain characteristics of production result to which the remaining aspects of culture are and must be adjusted.

An outstanding aspect of food production in Moala is that there are large differences in the quantities of food raised by different individuals and families. *In a given village these variations amount to the difference between surpluses in some or all foods in certain households and food scarcities in others.* I am unable to state with precision the degree to which surpluses and scarcities exist, but the accompanying table of quantity differences in the major foods planted in the gardens of nuclear heads in Naroi in 1954 will give some indication. (The figures are not exact. Most men keep close track of the amount of food they plant, at least in round numbers, but I was able to check only a few of the numbers reported to me. Therefore, I would be unwilling to use these figures more than to indicate in crude terms the range of variation in planting. The figures are based in most cases on a sample of 30 of the nearly 50 nuclear family heads in the village. The sweet potato and dry taro amounts are based on a sample of 29.)

TABLE 1

QUANTITIES OF MAJOR CROPS PLANTED IN GARDENS OF FAMILY HEADS, NAROI, 1954

	Yams (Mounds planted)	*Wet Taro* (Corms)	*Cassava* (Mounds)	*Xanthosoma* (Corms)	*Sweet Potato* (Mounds)	*Dry Taro* (Corms)
Total range in plantings per man	200–3400	0–4000	0–3000	0–1005	0–500	0–600
Interquartile range (Middle 50% of cases)	400–700	400–1300	150–500	150–500	130–300	0–200
Mean planted per man	734	1000	428	369	213	165

A variety of factors account for these differences in production. First, it should be noted that despite relatively dense population (50 per square mile), land available for swidden agriculture is not cultivated to its potential extent, and large families are easily able to expand their cultivations. The higher land of the island, relatively distant from the coastally-located settlements, is usually covered with long reverted forests. Yet this is fertile land, and many are able to clear and plant there. Moreover, soil analyses undertaken by the Fiji Agricultural Department with samples I collected shows that, even in areas closer to the villages, the usual period of reversion after cultivation is much longer than the minimum period necessary for soil recuperation. The land tenure rules are of sufficient flexibility to accommodate productive expansion. Lands at higher elevation are free to all comers. And if land nearer the village is insufficient for a large family or undesirable, it is an easy matter to kerekere usufructory rights from relatives.

As just implied, the available man-power in a family is a significant determinant of total household reserves. Some of the differences in Table I are due to the fact that certain of the men have mature sons whose labors considerably increase familial output. (Women do not take part in agriculture.) As a general rule, families with many male workers, few females and immature children are able to accumulate larger reserves than families with fewer workers and more non-workers—for in the latter case less is produced and more of that is consumed. Other factors contributing to differences in production include: variable fertility of planting sites due to soil and climatic conditions, differential occurrence of incapacitating illnesses, and differences in age, strength, agricultural skill and personal motivation. (Such motivation, incidentally, derives partially from the rewards offered by kerekere—either the reward through dispensing aid or that of being able to garner a food supply through solicitation rather than production.)

A second feature of production to which I call attention is regional variation in the quantities of various types of food produced. In different locales, variations in rainfall and sunlight, and of waters and topography necessary for irrigation, produce different potentials for growing the several types of crops. The Moalans are aware of the conditions required by their crops and plant accordingly. As a result, different villages tend to produce more of certain crops, one village more yams, one more wet taro and so forth. The accompanying table indicating averages of the planting per family heads in three different villages describes the nature of regional variation in production. The villages are Naroi in the extreme northeast of the island, Keteira on a deep bay in the eastern or windward coast, and Nuku on the westernmost point of Moala.

TABLE II

AVERAGE PLANTINGS IN 1954 OF FAMILY HEADS IN THREE VILLAGES

	Naroi (sample as above)	*Nuku* (N = 13, all family heads)	*Keteira* (N = 16, all but two family heads)
Yams (mounds)	734	371	371
Wet Taro (corms)	1000	3200	1875
Cassava (mounds)	428	219	354
Xanthosoma (corms)	369	175	47
Sweet Potato (mounds)	213	19*	52
Dry Taro (corms)	165	117	293

* Time usually given to planting occupied by building new village water-supply outlets in 1954.

Regional, individual and familial variations in quantity of food production are key characteristics of the Moalan economy which help account for the predominance of kerekere as a distributive mechanism. Food scarcity is a daily possibility in any number of houses in a village, while at the same time other families may have a surfeit of food. Moreover, a family abundantly supplied one day may run short on the next. But kerekere, being predicated on need and operating through widespread networks of kin relationship, is an effective means for alleviating these innumerable crises and potential crises. Kerekere may easily be invoked on a moment's notice to procure food (or other vital goods) from any available source. Similarly, kerekere as it is invoked between members of different villages spreads the consumption of regionally diverse produce. Through kerekere individual and local differences in production become contributions to the general welfare in a material sense, while large-scale producers derive non-material compensations for their efforts. Kerekere is a simple, efficient and advantageous means of goods distribution when one takes into account the method and results of vital production in the local environment.

In summary, production, distribution and political power comprise a functionally related system. The predominant form of distribution, kerekere, is an effective mechanism for adjusting inequalities occasioned in the productive exploitation of the environment. Dispensing goods widely by kerekere contributes markedly to a person's prestige; hence, chiefs attempt to engage in the system on a large scale as a means of reinforcing their standing and extending their authority. But because such manipulation of the distributive system cannot be solely restricted to chiefs, kerekere may foster a type of rivalry which debilitates the power structure and produces an atmosphere of militant egalitarianism.

If there is one outstanding implication of this study, it is that a crucial investigation is yet to be made into the types of distributive systems of the primitive world, their bases in production, and the measure of economic control they bring to the prevailing power structure. This analysis provides the hope that such an investigation might permit formulation of broad propositions relating economic and political development in kin-organized societies.

ADDENDUM

In conversation subsequent to the presentation of this paper, Professor Raymond Firth suggested that explicit mention be made of the problems raised by kerekere in conjunction with the introduction of a monetary economy. In concurrence with the suggestion, I would point out that kerekere, while adaptive under traditional conditions, is an impediment to the transition to a monetary economy. The difficulty is occasioned by the multitudinous claims laid through kerekere on the hard-earned goods procured by individuals through copra production. Accumulation of capital is thereby discouraged despite many economic and social pressures, having the opposite effect.

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Notes

1. The author wishes gratefully to acknowledge the Research Training Fellowship granted by the Social Science Research Council of Washington, D.C., which made the field study possible. Preparation of field materials for publication is being aided by a grant from the Columbia University Council for Research in the Social Sciences.

2. The system of kerekere may thus be termed an "optionally reciprocal" system. As such it is a variant of Professor Polanyi's "reciprocal form of economic integration" (Karl Polanyi, *The Great Transformation*. New York, 1944. Cf. Chap. IV).

3. As such kerekere may be contrasted with the redistributive mechanisms in Polynesia where control of the distributive system is an inherent right of chieftainship. Cf. Marshall Sahlins, *Social Stratification in Polynesia*. University of Michigan Microfilms, 1954. Ann Arbor. (Columbia University Ph.D. Dissertation.)

TIPI DI CULTURA NEL PARAGUAY

Pietro Scotti

Ho avuto occasione di interessarmi allo studio delle culture del Paraguay studiando la Collezione Boggiani del Museo Pigorini (Roma) e gli scritti, anche inediti, dello stesso esploratore ed etnologo italiano, nonchè esaminando la Collezione dei Missionari Salesiani del Gran Chaco e le notizie da loro raccolte sul luogo.

Dallo studio di tali elementi e dai lavori di vari A.A. antichi e moderni è sorto in me il desiderio di configurare in un quadro generale i tipi di cultura del Paraguay, particolarmente vedendoli nella luce dei concetti e delle classificazioni etnologiche storico-culturali. Debbo subito dire che—come del resto mi accadde per altri settori culturali esaminati—ho subito veduto una gran complessità in questi tipi di cultura. Indubbiamente gli elementi tipici dei vari cicli culturali sono spesso chiaramente manifesti, ma in concreto le singole culture sono praticamente quasi tutte compòsite, e talora assai compòsite. Ciò risulta, oltrechè da molti lavori, in modo speciale dai moderni lavori di Nordenskiöld e di Métraux (V. in *Bibliografia*).

Prescindo, nel mio esame, dalla recentissima cultura europea. Però, in un certo senso, bisogna tenerne conto, come di un importante fenomeno di acculturazione. E anche in altro senso: perchè gli Europei hanno contribuito a portare nel Paraguay elementi culturali esotici non europei. Cito un solo esempio, la introduzione dell'arco a palla, di origine asiatica, portato nel Sudamerica dai Portoghesi.

Ma, pur generalmente prescindendo dagli elementi europei, le culture del Paraguay si mostrano ugualmente assai complesse. E quindi non sarebbe oggi accettabile la carta etnologica data da W. Schmidt nel suo noto generale lavoro sui cicli culturali del Sudamerica (anno 1913) in cui l'A. dava per il Paraguay la presenza di due soli tipi di cultura: Urkulturen; Frei-mutterretliche Kultur (Bogen-Kultur); come a dire: una cultura così detta "australiana" e la cultura agraria, collegata più o meno ai Guaraní e agli Aruachi (oggi Métraux ritiene che in parte l'attività agraria nel Paraguay possa anche collegarsi a un influsso andino).

E' ormai evidente che una schematizzazione tipo Schmidt (spiegabile per la relativa insufficienza delle indagini nel 1913, ed anche per il carattere assai generale e sintetico del suo lavoro) non dà un quadro esauriente delle culture paraguayane.

Prima di parlare, propriamente, delle culture ricordo qui la ripartizione degli Amerindiani del Paraguay dal punto di vista linguistico. Sono qui presenti i Guaraní (fra i quali particolarmente interessanti i Guayakí), i Maskoi (cioè Lengua, Angaité, Sanapaná, Sapukí, Kaiotogui), i Samuku (antichi Samuku, Ciamacoco, Morotoco), e i Guaykurú (Toba, Mokoví, Abiponi, Payaguá, Kaduvéo). Alcuni di questi gruppi sono estinti o in via di estinzione; il gruppo Kaduvéo da parecchi decenni non è più nel Paraguay; già sulla fine del sec. XIX era passato nel Brasile meridionale.

Non sempre vi è corrispondenza fra il tipo linguistico e il tipo culturale; tanto per citare un esempio, i Guayakí (che parlano una lingua guaraní) sono di tipo culturale assai diverso, assai primitivo, rispetto ai Guaraní propriamente detti, i quali poi oggi sono molto europeizzati, nel Paraguay.

Venendo ora particolarmente all'esame dei vari tipi culturali, si può fare una grande triplice distinzione fra i tipi culturali del Gran Chaco (ad occidente), quello dei Guayakí (ad oriente), e il tipo culturale evoluto dei Guaraní (nella porzione intermedia, cioè nell'area ad oriente del fiume Paraguay).

Al tempo della conquista spagnola e per vari secoli successivi l'area posta ad oriente del fiume Paraguay presentava popolazioni guaraní piuttosto evolute (cultura agraria) ma resistente agli influssi europei, anche perchè in detta area vi erano popolazioni non guaraní, come i Payaguá e i Kaduvéo, molto energiche nella lotta contro gli Spagnoli. Non sto qui a delineare minutamente i caratteri di questa antica cultura guaraní perchè sono assai noti.[1]

La cultura dei Guayakí presenta, come ho detto, caratteri di alta primitività. Ho avuto occasione di illustrare una notevole collezione guayakí raccolta da Boggiani per il Museo Pigorini (Roma) e l'alta primitività di detta cultura appare evidente attraverso l'estrema semplicità degli strumenti facenti parte della collezione stessa.[2] Essi vivono di raccolta, caccia e pesca. Hanno scalpelli per lavorare il legno, costituiti da denti di roditori montati su tibie di animali; recipienti da acqua fatti con una graminacea fittamente intrecciata, e rivestiti poi all'esterno con cera; rozze gerle di foglie di palma usate nei loro viaggi. Il tutto fa ascrivere indubbiamente i Guayakí a una cultura molto primitiva, con riscontro a quelle culture semplici che si trovano in Fuegia, e in varie tribù del Brasile e del Paraguay, Spesso si parla di "tipo australiano." I Guayakí parlano un dialetto guaraní e, in parte, sono anche guaranizzati; si tratta però di fatti relativamente recenti, e limitati all porzione periferica dei Guayakí, più vicina al mondo civile.

Di interesse tutto particolare sono le varie poplazioni dimoranti nel Gran Chaco (Paraguay occidentale). Generalmente le culture ciachensi presentano un fondo di alta primitività, spesso però integrato con elementi vari, di carattere superiore (agrario-matriarcale, ad esempio) e perfino europeo. Per alcuni catteri queste culture sono state ricondotte al "tipo australiano." Però alcuni elementi, anche fra quelli arcaici e idiomatici, sembrano alquanto diversi. Prendiamo, ad esempio, l'abitazione. Esiste, è vero, la tipica "capanna ad alveare,"[3] ma assai spesso essa presenta altra forma, di tettoia a fianchi aperti, con tetto piano poggiante su quattro pali terminanti a forchetta. Ed è interessante, a questo proposito, notare ciò che osservò Boggiani, che cioè i Kaduvéo quando si recavano alla caccia adottavano proprio questo tipo di abitazione (sia pure un po' meglio chiuso, con qualche parete) mentre quando si dedicavano alle attività agrarie abitavano in grandi capanne a due pioventi, dimore collettive per più famiglie, tipiche del mondo agrario-matriarcale, e senz'altro da collegarsi con influssi aruachi, sia pure attraverso i Guaraní. Sarebbe troppo lungo, in un lavoro sintetico come intende essere il presente, analizzare le singole culture delle singole tribù ciachensi. Mi limito pertanto ad alcune indicazioni più importanti, atte a mostrare la complessità delle culture stesse.

Nordenskiöld e Métraux con molta cura han messo in evidenza elementi andini esistenti nelle culture del Gran Chaco; io stesso, illustrando la citata Collezione Boggiani, ne ho messo in evidenza alcuni: cucchiai di legno, vasi di terracotta sostenuti con corde alla fronte, zufolo lungo di legno, alcune tecniche tessili, alcuni tipi di pettine. Io ho riscontrato nella Collezione illustrata anche un poncio. Boggiani aveva intuito elementi andini nelle ricche decorazioni dei vasi caduvéi; le analisi minute e recenti di D. Ribeiro hanno confermato tale intuizione (anche se altri ornamenti hanno altra origine, anche europea). Fra i Ciamacoco, i Kaduvéo e altre popolazioni sono stati anche riscontrati elementi totemici o dubbiosamente totemici. E' questo un nuovo elemento da tener presente. Da un punto di vista storico si può discutere il significato di questi elementi, tanto più che la distribuzione geografica del totemismo in Sudamerica appare piuttosto dislocata a nordovest. Questi elementi totemici sono un acquisto recente da popoli viciniori o sono invece antichi, attribuibili a uno strato di grandi cacciatori prima qui più potente? Non dimentichiamo che i grandi cacciatori si trovano anche più a sud in Patagonia e che i Bororo (situati a nordovest del Paraguay) presentarono un totemismo assai sviluppato (unito d'altra parte a una ricca cultura agrario-matriarcale). A proposito di correlazioni culturali con i Bororo è notevole che il tempo intercorrente fra la prima e la seconda sepoltura, per i Bororo e per i Kaduvéo, è esattamente identico. Significato totemico possono avere anche certi dischi pettorali fatti di conchiglia (genere *Unio*) usati dai Ciamacoco. Presso questa popolazione Belajeff ha messo in evidenza l'esistenza di clans collegati a vari animali, fra i quali figura la tartaruga.[4] La tartaruga è anche spesso raffigurata dai Kaduvéo, specialmente nelle pipe di legno scolpite. Pure fra i Kaduvéo figurano (nella Collezione Boggiani da me illustrata) alcuni ventagli di palma, ad intreccio; che potrebbero far pensare ad una, almeno indiretta, origine polinesiana. A questo riguardo devo dire che questo reperto non contrasta sostanzialmente con i limiti dati da W. Schmidt, nella cartina citata (anno 1913), all'area di influsso austronesiano (*Die austronesischen Kulturgebiete*). Non ignoro che la questione dei rapporti culturali fra Sudamerica e Oceania è assai complessa. D'altra parte, nel caso, si potrebbe anche pensare anche a un semplice fenomeno di convergenza.

I cenni qui esposti, assai sommari, sono sufficienti, credo, a mostrare la complessità delle culture del Paraguay, anche di quelle realmente più semplici. Questa constatazione potrebbe parere contraria alle costruzioni ciclo-culturali classiche (Graebner, Ankermann, Schmidt e molti altri A.A.). Io però non credo che tali grandi quadri classificatorii siano da abbandonare, con leggerezza. In fondo, è proprio per mezzo di quei quadri (sia pure un po' astratti) che noi possiamo analizzare le varie singole culture, così come un medico studia la malattia in concreto, in un singolo paziente, tenendo conto dei grandi, classici quadri clinici. Non bisogna però credere di trovare, o almeno di trovare sempre, in concreto, quei semplici schematici quadri. Ogni cultura è una cultura; in essa entrano elementi provenienti dal di fuori, in essa si riscontra un fondo antico comune ad altre culture, ma poi c'è assai spesso un elemento proprio, idiomatico, nuovo. E questa "novità" è, culturalmente, la cosa più importante perchè sta ad indicare un'attività creatrice dello spirito in un popolo.

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Notes

1. Lothrop, S. K., The Guaraní, *in* Steward, J. H., *Handbook of South American Indians*. Vol. I. Washington, 1946, p. 179 e segg., e i lavori di Bertoni e Métraux in *Bibliografia*.
2. Scotti, P., La collezione etnografica sudamericana di Guido Boggiani, *Rivista di biologia coloniale*, 7 : (1). Roma, 1946.

3. Vedi, a questo riguardo, un tipico disegno di Boggiani (armatura di capanna provvisoria dei Sanapanà) in Scotti, P., *I contributi americanistici di Guido Boggiani*, Genova, 1955, p. 173.

4. Cfr. Belajeff in *Rev. Soc. Cient. del Paraguay*, Asunción, 1936, p. 192.

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THE MAINTENANCE OF UNITY AND DISTINCTIVENESS BY A PHILIPPINE PEASANT VILLAGE[1]

Willis E. Sibley

For tribal societies, long the major focus for anthropological research, no great problems are presented in the maintenance of the physical and psychological boundaries of the local group. While many instances may be cited of intertribal trade, warfare, and other contacts between such tribal units, such relations are generally intermittent and occupy little time. Thus tribal societal and cultural systems may be treated as isolable, whole structures.

For peasant societies, however, the very ability to continue in existence is seen to be dependent upon persisting relations of various kinds between peasant communities, and persons and institutions on other levels of a national life of which they are a part[2] (Redfield 1956: 37). In peasant villages in which the maintenance of quite clear-cut social boundaries is valued and functional for the persistence of the group,[3] there arises a problem of adjustment between the need for relationships with external social units, persons and systems, and the continuation of marked social boundaries between the village and its external world.

Manalad, a village in the municipality of Ilog, Negros Occidental, Philippines, is such a community. Lying in the flat southwestern coastal plain of Negros Island, the village economy is based today mainly upon tenant rice farming and wage labor in nearby sugar haciendas.[4] The 400 villagers, most of whom are Catholics, form a homogeneous social group which resembles closely the corporate peasant community type outlined in a recent paper by Eric Wolf (Wolf: 1955). About the corporate community type, Wolf writes that (Wolf 1955: 456):

> The distinctive characteristic of the corporate peasant community is that it represents a bounded social system with clear-cut limits, in relation to both outsiders and insiders. It has structural identity over time. Seen from the outside, the community as a whole carries on a series of activities and it defines the rights and duties of its members and prescribes large segments of their behavior.

In Manalad, as in the Latin American communities of which Wolf writes, we find present bilateral kindreds. In Manalad, we find village social boundaries established largely around a series of these deeply interlocking kindreds, while in Latin America Wolf finds the corporate holding of land to be a more important determinant of community solidarity, at least historically.

In Manalad additional internal ties of *compadrazgo* relations link with a series of right and obligations many villagers not already joined affinally or consanguineally. A high rate of village endogamy, running at about 50 per cent in 1955, contributes to the depth of intra-village ties.

In this paper we begin a preliminary examination of the problem: How is village unity and distinctiveness maintained, despite the existence of numerous persisting external ties? In this paper are examined some of the consequences of various external relations for the sense of identification of the villager, for group membership, and for cultural and societal change. We try to learn how the external bonds are balanced in favor of the result we see—a village with truly distinctive social boundaries marking it off from other similar villages.

External relations for Manalad may be classed roughly into the following categories: political, commercial, religious, occupational, and real and ritual kinship ties (*compadrazgo*). With the exception of the latter category, that of real and ritual kin, they may be characterized as more impersonal than those within the village, less frequent, and often organized into superordination-subordination relations. Geographically, external relations for Manalad do not form radially congruent patterns such as those found by Dr. Starr in her study of levels of communal relations in Veracruz, Mexico (Starr 1954). Instead, widely varying patterns of interaction over space can be mapped for different kinds of external relations. Let us now turn to an examination of some external relations, and their consequences for the maintenance and defence of the village's community boundaries.

About 10 per cent of the children of parents still living in Manalad reside today in other villages, towns and cities, mostly in the same province. Considered as members of social groups outside the village, these children who have left Manalad provide the basis for new bonds of allegiance for their parents —counter-unities endangering the distinct social boundaries for the village and the parents' allegiance to it.

On the other hand, in the children's attempt to return to Manalad at least annually to participate in the fiesta, the village's biggest annual collective effort in honor of the patron saint and to aggrandize the village before other villages, they help to reaffirm strongly that Manalad is a bounded community, sharply to be distinguished from other similar villages. By virtue of visits back and forth between parents and children, the influence of the latter upon Manalad is not broken with their leaving, and through their new experiences, the children may initiate changes when they return.

Although about 50 per cent of the marriage ties in Manalad in 1955 were endogamous, both male and female marriage partners are continually brought into new membership in the village. The bringing in of spouses establishes new bonds of obligation with other villages, and thus blurs the distinct social boundaries which the village strives to maintain. However, the spouses newly brought in quickly tend to spend most of their time in Manalad, and their network of rights and duties soon is much greater in Manalad than in the villages of origin. The fact that a large number of persons within Manalad must approve prospective spouses from other villages before marriage negotiations are begun allows the village to limit its new members to those persons sharing values prized in Manalad.

During the dry season, from about January through June, visits are frequently made to fiestas outside Manalad. Almost invariably one visits only those fiestas in localities where relatives reside. In the course of such visits, kinship ties with collateral and other relatives are reaffirmed. Such activity would tend theoretically to break down the unity of Manalad as a bounded community, but seems not to do so. A partial explanation for this failure lies, apparently, in the small proportion of time which an individual spends in this activity, as compared with the amount of time spent in activities with Manalad as the most important "nexus of activity"[5] and identification.

In the process of helping another village to celebrate its patron and aggrandize itself, the visitor from Manalad helps that village to reaffirm its solidarity and distinctiveness from other villages, just as the opposite behavior occurs when Manalad holds its annual celebration. This practice, then, seems to help all such villages maintain their social bounds intact.

Visits regularly made by a few devout Manalad villagers to religious services in several neighboring villages and towns have little effect in changing the nature of Manalad as a locus of identification. Despite the visits by the most devout to outside churches, the locally elaborated variety of Catholicism seems most important. It is almost as if Catholicism is re-created in every village. Along with a highly personalistic orientation on the part of the Manalad villagers to the village patron *Sto. Niño*—honored usually by Catholics and by the Protestant minority alike—Manalad also has as part of its religious calendar the performance of such folk rituals as the "Feeding of Jesus, Mary and Joseph," and the "Feeding of the Father, the Son and the Holy Ghost." These rituals, while organized by the same few families in Manalad each year, require the participation of many other families as an audience to give them social meaning, and hence become collective elaborations unique to the village. The fact that other villages also have elaborations but of different kinds is significant and provides for Manalad and other villages a further means for the maintenance of the community and its social perimeter.

In the growing of rice, tenant rice farmers in Manalad and their landlords play a series of interdependent roles. The local landlords, some of whom were born in Manalad and only later moved to the town, tend to center their interests about the steady annual income which their shares of the rice crop ensure. They were never observed to attempt to introduce changes in the village by virtue of their power as landlords. Rice-share agreements are traditional and seldom give rise to tension except in isolated cases.

During April and May, and again October, many villagers renew their ties with kin in nearby mountain hamlets, going first to help plant and later to harvest upland dry rice in return for shares in the product. As in the case of fiesta visits, counter-links are established which might tend to reduce a villager's allegiance to Manalad. However, since these links with the mountain hamlets are of short duration even though regular and annual, their effect upon the total obligation pattern of the average villager seems to be slight.

About 80 per cent of the adult males in Manalad gain their primary annual incomes through piecework wage labor on nearby sugar haciendas during six months of each year, although a few of these are also tenant rice farmers. The hacienda field managers follow traditional practices in organizing work by presenting a work structure and procedures which fit, rather than conflict with, customary traditional procedures in the community (cf. Nash, 1956). Labor gang supervisors from Manalad tend strongly to utilize kinsmen in their work crews—the same practices which are followed in those portions of rice culture which require cooperative labor of a group of men or women.

The system of paying wages for labor, however, has had consequences in the slow shift in traditional rice culture from the older practice of *dagyaw* or cooperative labor, in return for feasting and reciprocal labor obligations, to piece work rates for some phases of rice cycle labor. A possible but not surely emergent result of the introduction of cash payments throughout the village economy seems to be an increase in individualization to the detriment of the functioning of the nuclear family as a cooperative economic unit.

Regular visits are made by villagers to at least nine weekly markets in surrounding towns, although the Saturday market in Kabankalan and Tuesday market in nearby Galicia are markets most frequently visited. The effect of the market in changing village life seems not to have been striking. It does serve as a center for the exchange for gossip, but as a source for the dissemination of new ideas it seems less successful. Generally, villager meets villager in the market, and except for transitory association with salesmen from the city and from other towns, the market is most important simply as a source for goods one requires. The availability of cheap cotton cloth, primarily from Japan and the United States, is an exception to what we have said before, for it has had an important consequence in its destruction of the local abaca-cloth weaving formerly carried on within nearly every village in the area.

Manalad is perhaps less moved by market association than those villages which depend heavily upon markets as places in which significant portions of the village income are acquired through the sale of goods. Aside from occasional sales of rice in return for emergency cash, Manalad regularly produces no commodities for the market except a little copra.

Current relations between Manalad and the local or municipal government seem to be of little consequence as possible threats to community solidarity. Obligations of the low-income villagers of Manalad are limited primarily to annoying payments for birth registrations, marriage, and death licenses—none of them have enough income to pay taxes which are collected by the municipalities for the national government. The municipality provides a small amount of medical services to the village, either to those walking several kilometers to the town, or by occasional visits to the village by the municipal health inspector, who takes that opportunity to innoculate children against cholera, dysentery and typhoid and to perform other minor services. Also at the municipal level there is the municipal agriculturist, who visits the village periodically to give lectures on agriculture and animal care, and to innoculate animals against disease. Potentially an agent of village change, the individual who visited Manalad during the writer's residence there appeared to be ineffective. He was not felt to practice what he preached, and indeed some of his instruction, particularly about crop rotation, was manifestly impossible to follow in land-short Manalad.

Relations at the provincial level are largely political in nature, creating bonds of obligation between villagers and politicians in the provincial capital. Such obligations are created by the distribution of work on local public works projects, and allocation of money, as in 1955 for the repair of the village road, and so on. The politicians are interested more in receiving votes in return for their favors than in changing the village, and have generally not been threats to the corporate nature of the village except possibly during short pre-election periods. At this time the villagers' external ties with politicians may create internal disputes, but these tensions are transitory and political interest resumes its normally dormant state soon after an election is over.

Links with the highly centralized national government include social welfare aid agents, agricultural loan agencies, the veterans' administration and the school system. Aside from the latter, these relations are in the nature of intermittent, brief associations without lasting effects on the village beyond the increment to village income which the few veterans' payments and less frequent social welfare aid payments provide.

So far we have not talked about the relations of Manalad and external agencies specifically interested in altering the cultural and societal arrangements in the village. They are few, but the village elementary schools and its teachers are such an agency. Through the school system the national government reaches the village, and has in recent years attempted improvement in village sanitation, gardening, animal husbandry, and other projects through adult education in a program called the Community School. These attempts have failed in Manalad except under the direct and continuous supervision of the teachers. We offer two reasons for the failure. First, while the teachers are assigned a high status position because of their occupations, they are not considered able to give meaningful advice about agriculture and animal husbandry because they do not themselves plant rice, nor grow pigs and chickens. Socially, they do not participate in the gossip and fun sessions at the coconut wine stands after the day's work is finished. They stand apart. Secondly, the teachers have not successfully tapped the internal structure of leadership and power which exists in Manalad. In their most recent attempt at directed change, they organized cooperative units linking individuals not customarily in work relationships to one another, thereby failing to take a first needed step to successful change (cf. Nash, 1956).

A unified national government effort begun in 1956 to effect culture change and rural amelioration in the Philippines will soon provide intensely interesting data for the theorist of culture change as well as for the action anthropologist, but has not yet had time to make its effect felt in Manalad.

We have examined very briefly the consequences for Manalad of some of its persisting essential external contacts. We have seen that some of the external relations establish a series of unities or systems of obligation which operate counter to Manalad's efforts to maintain distinct social boundaries. At the same time, we see Manalad to have a kind of balance between its external relations and internal social systems, with village solidarity maintained. It appears to lie at some point between imagined polar positions of the perfectly *extensive* integration of social relations over space of a super-urban center, and the *intensive* inwardly directed integration of social relations in a tribal society. That village solidarity is maintained may seem hard to believe, particularly in view of the numerous bonds with the outside world, especially those with relatives. Indeed, only a partial explanation of this balance and maintenance of village distinctiveness is possible by noting the intermittent nature of external relations, and the fact that many of them tend to be impersonal, holding the outside world at "arm's length."

The rest of the solution to our problem, that is, how does the community remain distinct despite external ties, must come from an examination of the internal systems in the village, among them the patterns of work, residence, kinship, the fiesta and other collective representations. Statistically, it might be shown that for every role the resident of Manalad plays *vis à vis* an external agency, he has several to play within the village. The psychological concomitants of this statistical datum are important, and provide the basis for the village's ability to remain the central locus of identification and membership for the villager.

This general problem of balance between external and internal systems is basic to the whole question of peasant society and culture, and raises questions which can be answered only through intensive comparative field research. For example, how far can the balance of relationships be pushed toward the outside without losing the peasant community as we see it? Or, on the other hand, how far can the balance be pushed toward the inside and still have a peasant society rather than merely a tribal, isolable societal unit?

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Notes

1. This paper is based upon field work carried on in the Philippine Islands in 1954–1955, supported by the United States Educational Foundation in the Philippines (Fulbright), and by the Philippine Studies Program, University of Chicago. Drafts of this paper have been read by Robert Redfield, Fred Eggan, and Sol Tax. The writer is grateful for their aid and suggestions.

2. A number of anthropologists who have published material concerning peasant society concepts are Foster (1955), Mintz (1952), Redfield (1954, 1955, 1956), Starr (1954), Steward (1951), Wagley and Harris (1955) and Wolf (1955).

3. There seem also to be groups for which the opposite is functional. See, for example, Wolf's open community type (Wolf 1955 : 463 ff.).

4. A reading of Mintz's (1952) paper on rural proletarian class segments might suggest that Manalad, with a high proportion of its men working in the sugar fields, might fit this type of organizational type better than that of the corporate peasant community that we utilize in this paper. A further examination of the data, however, showed the primary orientations still to be toward the traditional village economy based on rice, although it may be that Manalad is moving toward a societal organization similar to that found by Mintz in Puerto Rico.

5. This useful term is suggested by McKim Marriott in a recent paper (1955) on the Indian village of Kishan Garhi.

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CONTRASTING PATTERNS OF CAROLINIAN POPULATION DISTRIBUTION IN THE MARIANAS

Robert R. Solenberger

I

This paper will summarize the spatial location of Caroline Islanders entering the Marianas within historic times, with some explanation of the reasons for their distribution. The migrations and settlement patterns involved will be presented in terms of social and cultural dynamics, with attention to ecological factors.

The Marianas Islands form an arcuate chain, including the large islands of Guam, Rota, Tinian and Saipan on the south, and tapering off with a number of small volcanic islands—now largely uninhabited—several hundred miles to the north. For more than a century, two distinct ethnic and linguistic groups have lived interspersed throughout this entire area—the Chamorro majority and a sizeable Carolinian minority.

Both the Marianas and the neighboring archipelago to the south, the Caroline Islands, extending widely from east to west just north of the Equator, are usually grouped together with the Marshalls and Gilberts in the same ethnographic province of Micronesia. Inclusion of the Marianas in Micronesia can be justified only for the pre-Spanish stone-age period. By 1700, however, the entire Chamorro population of the Marianas was concentrated around Jesuit missions, policed by Spanish soldiers, on the two islands of Guam and Rota. During the 18th century, the Chamorros absorbed both immigrant population and culture from the Philippines, Mexico and Spain. In both language and culture, the Chamorros are now hispanicized Indonesians.

The Caroline Islands received very little attention from Spanish officials or Catholic missionaries during the long period of Spanish rule. Even the Germans did little to modify native Carolinian culture on most islands. The Caroline Islanders today are closer culturally to Polynesians than they are to the Chamorros of the Marianas.

There are also some physical distinctions observable when Chamorros and Carolinians are seen in large groups, the Carolinians usually being somewhat darker and having curlier hair. Motor habits and temperament seem even more noticeable in distinguishing the relaxed, informal Carolinians from the self-conscious, dignified Chamorros. Persistence in using their own language among themselves, even after generations of residence among Chamorros, is another factor tending to keep the Carolinians of the Marianas visible as a minority and culturally isolated. Until World War II, costume was an important point of difference between the two groups.

II

The small islands between Yap on the west and Truk on the east constitute a poor, sparse environment physically, and a limited one culturally. In aboriginal times the Caroline Islanders from this area probably found as much to attract them on the larger island of Babelthaup, or on imperial Yap, as they did on Guam. Nevertheless, some of the inhabitants of Lamotrek, Ulithi, etc., only about 400 miles south of Guam, are believed to have been accustomed to travel thither by canoe in ancient times. These trading voyages were suspended during the prolonged wars and epidemics marking the Spanish conquest of the Marianas. Stabilization of the Spanish colony, with its capital city on Guam, made that island attractive to the neighboring Carolinians, not only as a source of iron goods, but perhaps more importantly, as a center of urbanization and cultural stimulation.

In 1788, a fleet of canoes from Lamotrek, piloted by one Luito, rediscovered Guam. Annual trading voyages from the nearer Western Carolines to Guam were resumed in 1805 and continued through the nineteenth century. The Spaniards on Guam, having access to few other means of inter-island transportation, welcomed this contact with distant portions of their colony. Within a few years canoes built and manned by Carolinians were travelling to the uninhabited islands north of Guam, especially to bring back dried, salted meat from Tinian.

III

Population pressure relative to limited land resources on the small Caroline islands and atolls may be assumed as the basic reason for the migration of Caroline Islanders into the Marianas as permanent settlers.

In 1815 about 200 refugees from a typhoon on Lamotrek were permitted to settle on Saipan, where they founded the village of Garapan on the lagoon, the first permanent re-settlement in the Marianas north of Rota since the 17th century. Yet many Chamorros persist in speaking of the Carolinian minority in the Marianas as intruders in the ancient Chamorro homeland. In 1865, Garapan numbered 424 Carolinian inhabitants and only nine Chamorros, who established their own quarter at the north end of the town. By 1886, out of a population of 849, Garapan was still approximately two-thirds Carolinian.

By 1816 there was a settlement of Carolinians near Agaña harbor on Guam, presumably serving as a base for Carolinian seafarers in the Spanish service. This Carolinian colony on Guam persisted until 1901, when it moved to Saipan under pressure of American officials who were scandalized by the scanty costumes of these islanders.

An important Carolinian migration from the atolls south of Guam took place in 1865, when approximately 1000 islanders from that area arrived to work in the coconut plantations of Guam, Saipan and Pagan. In 1869, a family from Ireland acquired a ranch on Tinian and brought in to work it 250 Carolinians from Piserat Island in Namonuito Atoll, the first re-settlement of Tinian since the 17th century. About 20 years later, these people moved to Saipan, settling Tanápag village, where they remain today as a group separate socially and even linguistically from the other Saipanese Carolinians.

Carolinians migrating to the Marianas have tended to follow the Spanish-Chamorro pattern of settlement in large village centers near a Catholic church. But these urbanized Carolinian settlements have invariably been made where there was a lagoon. Carolinians have fewer land holdings in the interior of large islands than do Chamorros. Few Carolinian men follow the Chamorro custom of travelling several miles inland to farm, the Carolinian pattern of horticulture being concentrated in gardens near the beach. By settling along the lagoons on Guam and Saipan, these people from the small atolls could still feel at home with their distinctive combination of gardening as a female occupation and fishing as the main male occupation.

IV

In 1898 the Americans took Guam, and in 1899 the Northern Marianas, along with the rest of Spanish Micronesia, were purchased by the Germans. The boundary, however, remained open. During the prosperous German period, large numbers of Guamanian Chamorros migrated to Saipan, until they outnumbered the Carolinians. Typhoons in the Carolines in 1905 and 1907 caused the German government to bring a number of refugees by ship to Saipan. Some came from islands already represented among the Marianas-Carolinian population, but a distinctive group was brought from the Mortlock Islands, some hundreds of miles farther to the southeast than the origin-points of most of the previous migrants. Later some of these Mortlockese were transferred from Saipan to Ponape—whence a few later returned to Saipan or Guam.

Thus the advent of schooner and steamer transportation increased the scope and extent of migration between the Marianas and Carolines.

The German period saw further development of the copra industry. A Chamorro-owned company, the Pagan-Gesellschaft, operated coconut plantations on several smaller islands north of Saipan. Many of the copra workers who made temporary homes away from Saipan were Carolinians, living interspersed with Chamorros. On one island, Agrihan, the entire plantation was operated by Carolinians from Saipan, as it is today.

V

In 1914 the Japanese seized the German-held islands in Micronesia, including the Northern Marianas. There seems to have been little population movement into or out of the mandated area, except the overwhelming influx of Japanese and Okinawans.

VI

Japanese rule in Micronesia ended in 1944. American control was resumed in Guam. In 1947, the former Japanese Mandate became a United Nations trusteeship. Although an international boundary still exists between Guam and the Trust Territory of the Pacific Islands, the U.S. flag flies on both sides of the line. Between 1944 and 1952, the movement of native population was controlled by regulations formulated by the Navy while it governed both areas, and continued in force by its successor, the Interior Department. Under this system any Trust Territory national who could locate a sponsor, such as a relative or a potential employer, on Guam had little difficulty in arranging to enter. On 16 June 1952, enforcement of the McCarran-Walter Immigration Act was begun, virtually suspending inter-island travel by natives via Guam. Few Micronesians can be expected to wish to enter Guam as quota immigrants, as they consider it a place of temporary residence only.

VII

Beginning in 1946, a number of Caroline Islanders entered Guam as contract laborers. In this way a number of young men and women, mainly from Palau and Ponape, remained temporarily in Guam, in most cases living in the homes of American officers and officials as domestic servants. This type of arrangement was characteristic of the period immediately following the close of hostilities, when little population movement was allowed on the part of Micronesians on any other basis. In addition to domestic service, some Caroline Islanders worked under contract in Government-controlled warehouses. This early opportunity for a few Carolinians to live and work on Guam, for the first time since 1901, has proved of great importance in succeeding years, in proportion as the contract workers cultivated friendly contacts with business men, Government officials, land owners, etc.

Of all the Carolinians, the Palauans have been the most successful in developing a chain of friendships and contacts to which they are continually adding new links. Thus, when the contract system of immigration was terminated in the late 1940's, the Palauans were able to obtain advance sponsorship to enable other Palauans to enter Guam, with a guarantee of subsistence and return to the home island if necessary. This second type of sponsorship agreement, which sufficed until 1952 to enable natives of the Trust Territory to enter Guam, did not specify that the immigrant must work for anyone. In practice, Trust Territory people usually have a job lined up before they reach Guam.

Thus some Palauans continue to be found living in groups on the premises of warehouse operators, housing contractors or other employers on Guam. This is also true of a few Yapese, who number less than ten on the whole island. But with greater familiarity on Guam, most of the approximately 100 Palauans there have preferred to rent their own housing. One run-down, low-rent section of Tamuning Village houses the only concentrated residential group of Palauans on Guam, including several young men working their way through George Washington High School, as well as a few married couples from Palau. About six Yapese and two Ponapean men also live in this sub-standard area. Some other Palauans, however, have sufficient acquaintanceship, and, in a few cases, relationship by blood or marriage, so that they are able to rent or live with sponsors in predominantly Guamanian villages, or in rural areas.

Any large-scale pressure of Palauans to migrate to the Marianas seems to have started only after the American destruction of the Japanese capital city at Koror. Palauans resident there missed the urban life they had learned to enjoy. These migrants, had they been content to settle down to subsistence activities, could easily have made a living in their home villages, or on vacant land on the 50-mile long island of Babelthaup. Thus the Palauans who come to Guam are not looking for opportunities to farm or fish, and are content to live in the bohemian atmosphere of the Tamuning quonset colony, or scattered in the homes of sponsors or employers.

A number of persons of Carolinian descent are concentrated in the municipality of Agat, on the west coast of Guam several miles south of the main centers of population and economic activity. Unlike Tamuning, the villages constituting the Agat community have space for gardens. A factor in making immigrants from the Trust Territory feel at home in Agat is its location on a peaceful lagoon, where some of them set fish traps—an activity abandoned by the prosperous Guamanians—and enjoy spear-fishing. The sector of Agat nearest the lagoon is called War Agat because it was built as temporary civilian housing during World War II. These temporary houses, often poorly roofed with thatch or canvas, like the rusted quonsets of Tamuning, are available for rent to Trust Territory people because Guamanians can now build better homes of their own. The persons of Carolinian descent in War Agat live interspersed with Chamorro immigrants, also from Saipan. In fact, since these Saipanese Carolinians all speak Chamorro, and may be of part-Chamorro descent, most Guamanians are not aware of their identity as Carolinians.

In a more rural section of Agat Municipality is a colony of Truk-Mortlock Carolinians, in part direct recent migrants from Truk District, and in part Saipanese Carolinians descended mainly from the typhoon refugees of 1908, but sufficiently intermarried with Chamorros to have Guamanian relatives in Agat.

Saipanese Carolinians from Tanápag Village, who speak the Woleai-Ulithi dialect, tend to settle with Guamanian relatives in Barrigada, a large but unfashionable post-war housing project. In contrast, neither Guamanians nor the Carolinians themselves seem aware of the presence of several Caroline Islanders in more-fashionable Sinajaña—perhaps because the latter come from different parts of the Carolines, and do not form a social unit. Carolinians living scattered in other areas of Guam are usually fairly well accepted by their neighbors, although those having few Guamanian connections do not participate in many local social activities.

Since World War II, most Caroline Islanders on Guam have been wage earners buying everything for cash. Thus location away from opportunities for fishing or gardening causes little inconvenience. Guam has few good fishing lagoons on the leeward side. Legal restrictions against land purchase by non-Guamanians, and the housing shortage, further reduce the opportunities for a Caroline Islander to reproduce his traditional environment on present-day Guam. In view of post-war "boom" conditions, he is fortunate to be able to rent, or live with his sponsor or employer, anywhere on Guam.

VIII

As soon as the civilians on Saipan began to surrender to the American forces in 1944, three separate camps were set up for (a) Japanese Nationals, (b) Chamorros and (c) Carolinians. Chamorros and Carolinians evacuated from the smaller islands of the Northern Marianas were also brought to these camps. In 1946, the Japanese were repatriated. At about the same time, the former inhabitants of Tanápag were allowed to settle again on or near the site they had occupied since Spanish times, north of the main harbor, on the beach. One factor in their wishing to return to their old location was their sentimental regard for the site of their former Catholic church—one of two on Saipan having a parish organization. An important difference between the Tanápag Carolinians and all others on Saipan is that the former have long intermarried with Chamorros and inhabit a village where many Chamorro families live interspersed with their Carolinian neighbors.

The same Saipanese Carolinian families which occupied part of Garapan have settled permanently in the former Japanese sugar company town of Chalan Kanoa, several miles south of the ruins of their former homes. The Chalan Kanoa Carolinians form a solid bloc, occupying the part of the town nearest the lagoon, where they enjoy bathing and fishing. A few of the former Garapan

Carolinians, under a progressive young leader, have formed a small outlying village at Chalan Pupulo, somewhat east and north of Chalan Kanoa, slightly further from the lagoon than is usual for Carolinians. The interest of their leader in commercial crops, especially coffee, seems to have drawn these people away from the beach towards the coffee groves on the slopes of Mt. Tagpochau.

In addition to the 100 per cent Carolinian island of Agrihan, Anatahan Island, just north of Saipan, is (I understand) being re-settled by Saipanese Carolinians, organized under the leadership of the man mentioned as the head of the Chalan Pupulo group. The purpose of this colony on an island abandoned since World War II is to exploit the wild goat population and grow coffee. A few interspersed Carolinians live on Pagan, an island inhabited mainly by Chamorros. Resettlement of even small copra islands tends to follow patterns of nationality distribution set in German times or before.

In Chalan Kanoa, as in the Marianas in general, Carolinian women who marry Chamorro men usually go to live in the Chamorro quarter, and their children are considered as Chamorros—whose traditional prejudice against Carolinians is cultural, rather than racial. Carolinians, who preserve a stronger tendency to revert to matrilineal reckoning, continue to accept and trust such mixed offspring, one of whom was an outstanding Mayor of Saipan, gaining the support of both groups.

IX

We now revert to one of the basic motives for the whole Carolinian movement to the Marianas, namely cultural enrichment. Guam today is the only spot available to Caroline Islanders even approaching the type of urbanization they have learned to know in movies and magazines. To many young Carolinians, bright lights are not Guam's only drawing cards. Palauans, especially, believe in education as the key to job opportunities in their home islands. Although Trust Territory schools are relatively good, Carolinian youths appreciate the advantage of completing their schooling in Guam, where English is widely spoken and is the only language of instruction. Such students usually make excellent academic records.

A few other special trainees have also been brought to Guam from the Caroline Islands to learn to be seamen, nurses, dentists, etc. Except for a few residential students in Fr. Dueñas Memorial School, and the trainee groups at various Government installations, most students live in scattered homes or rented quarters.

Perhaps, then, the strongest argument for keeping the frontier open between the various Trust Territory islands and Guam is cultural, rather than economic, in long-range terms.

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*

THE BIRHOR (THE LITTLE NOMADIC TRIBE OF INDIA)

L. P. Vidyarthi

Of the 357 million total population of India, only 17 per cent live in the 3018 cities and towns while the remaining 83 per cent live in the 558,089 villages.[1] A part of village India which consists of about 25 million population has been classified as Tribes. This tribal population is mostly concentrated in the hilly, forested, and inhospitable areas of northeastern, middle, and southern India. These tribesmen, though in various levels of economic gradings, are in general economically very backward, educationally almost illiterate, and socially almost cut off from the rest of Indian society.

Of such a tribal population, the highest concentration is found in the State of Bihar (Chota Nagpur and Santhal Paragana) which has been a great exciting laboratory for ethnographers, linguists, and prehistorians. With 45 large and small tribes, numbering about five million people,[2] of different economic, linguistic, racial and cultural levels, Bihar is one of the most interesting states in India for anthropological researches.[3] In the districts of Hazaribagh and Ranchi in Bihar there is a little-known nomadic tribe, the Birhor, that wanders from jungle to jungle in quest of food.[4] Owing to their nomadic traits, they are the most primitive tribe of Bihar and present a great problem from the rehabilitation point of view. Their total population cannot be enumerated exactly; however, according to the census report of 1941, their total strength in Bihar comes to 2499, and in other states 256. Earlier travellers, administrative officers, and anthropologists of the late nineteenth century, including Col. Dalton,[5] Driver,[6] and Risley,[7] have written a very little—and that very vaguely—about this wandering tribe. Credit goes to Roy[8] who did extensive field work among the Birhor and wrote an authentic monograph in 1925. He threw light only on the socio-economic and religious life of the Birhor and did not attempt a scientific investigation regarding the nature and extent of wandering among the tribe. However, he brought to notice that a section of the Birhor had ceased wandering and were leading a settled life. According to their mode of life the Birhor have been classified as the Uthlu Birhor (wanderers) and the Jaghi Birhor (settlers).

Last year in the months of September and October the present author visited six settlements of the Uthlu Birhor in the police station of Bishunpur in the Gumla Subdivision of Ranchi, and made extensive studies of four of them with a view to examining the nature and function of the Birhor wandering. For this investigation, he not only depended upon interview and observation, but concentrated on door-to-door census and on taking down the genealogies of all the families of the four settlements. The details of these settlements, or the temporary villages under study, may be tabulated as on the following page.

From the first table it is clear that the four villages are located in the two regions of Bishunpur, each in a group of two. In each region the two villages are in close proximity, while the two regions—south-east and north-west—are far apart. This question of location, as examined in the following paragraphs,

	Names of villages	*Number of Families*	*Number of Migratory groups*	*Number of Huts*	*Number of Persons*	*Distance from Bishun Pur with direction*
1.	Manjira	3	2	9	27	3m. SE.
2.	Serka	2	1	3	9	2m. SE.
3.	Chatak Pur	7	3	10	28	10m. NW.
4.	Narma	3	3	5	10	8m. NW.
	Total	15	9	27	74	

presents several facts for understanding the functional implication of wanderings among the Birhor. The second interesting point that comes out from the above table is regarding the composition of the Birhor temporary villages. The villages are not homogeneous units since we find differences in the numbers of families, migratory groups, and habitation in each village. This problem deserves consideration to understand the nature of Birhor nomadism.

FACTORS FOR THE LOCATION OF BIRHOR SETTLEMENTS

On the basis of the analysis of our data regarding the four Birhor settlements, it appears that they take three facts into consideration before selecting a place for their temporary settlement. They are (1) close proximity to the forest, (2) accessibility to the weekly market and also to the neighbouring villages, (3) availability of drinking water.

The first factor is chosen from the point of view of the availability of food resources and other forest products. Unlike most nomadic tribes of the world, Birhor economy does not revolve round cattle or the horse but round the forests. Not only their material culture but also the spiritual culture has been very greatly influenced by the forests. Their gods and goddesses are from the forests, live in the forests, and are to be propitiated in the context of forests.

Every morning the male Birhor of the settlement go to the dense forest collectively in quest of game and other jungle products. With them they take the nets which are their chief hunting appliances. They also carry clubs for killing the trapped animals, and axes to clear the bushes and cut the woods. The Birhor are famous monkey-hunters and the monkey is one of the important features of Birhor economy. They live upon the flesh of the monkey, sell the monkey skin, and also use the bones for some magical purposes. Their other notable game are rabbits, wild goats, and squirrels which are usually found in these forests. Next to hunting the second economic occupation of the Birhor is rope-making. From the forest they extract *chop* strings from suitable trees and make rope out of them. Both males and females are expert in rope-making, and this craft has found increased emphasis owing to local demands. The Birhor also make certain wooden objects like the frames of drums, bowls, pestles, and mortars. They also collect seasonal wild fruits from the forests. The Birhor women gather *mahua* flowers, roots and leaves from the outskirts of the forests to supplement their family diets. They also collect honey from the forests; honeycombs together with eggs make a delicious dish. The Birhor depend on the forests not only for their food but also for branches and leaves for making their poor temporary huts.

In the two areas of Bishunpur police station, *i.e.*, in the north-western and south-eastern parts, the hilly forests are especially suited to Birhor economy.

These areas are occupied by some Birhor families and they continue to live there till the resources are exhausted. On these resources depend the size and number of settlements as well as the duration of stay.

As stated earlier, the accessibility to the nearby villages and to the weekly market in order to dispose of jungle products and home-made things is also taken into consideration at the time of making a settlement. When the Birhor were exclusively in the hunting stage, or where they are still in that stage, perhaps the first factor might be the only determining one. But with the addition of rope-making and other things, the second factor has also become important. They meet the demand for ropes in their region. In the course of our field investigation we found that each Birhor settlement was near a village after which it derived its name. Moreover each group of two Birhor settlements was also near two different markets. The Birhor of Manjira and Serka villages used to go to the Bishunpur weekly market every Thursday with their ropes and wooden goods, while the Birhor of Chatakpur and Narma villages used to go to Banari weekly market on Monday. From the respective groups of villages, Bishunpur market would be at a distance of about two or three miles while Banari would also fall within four miles from the other group of villages. On each market day each Birhor family would sell things worth Rs. 5, or about a dollar. This facility for sale or exchange of Birhor goods for grains and other such things has brought changes in their food stuffs and food habits. So their pure forest economy has undergone modification with the impact of markets.

The third consideration for their choice of a temporary settlement, *i.e.*, availability of drinking water, needs no elaboration. As they cannot afford to dig wells every time where they go—and that often in rocky areas, they have to depend upon natural water supply. Owing to this the Birhor explore the possibility of being near a river, a spring, or such other water sources. At times the Birhor women have to fetch water from two or three miles away, especially in the dry season.

So, on the basis of the study of the above four settlements, we note these three factors—nearness to forests, accessibility to markets, and availability of drinking water—for the location of a Birhor settlement. But all these conditions are available only in a few places, and, owing to this, we find the migratory groups occupying the same places at some intervals in order of availability. It is very interesting to note that in some cases a migratory group wanders from place to place in quest of food and market in a cyclical order.

SOCIAL ORGANIZATION

The next problem that needs our attention is the social organization of the Birhor settlement. The Birhor call their settlement a *tanda.*[9] A number of leaf-huts, varying from three to ten, occupied by one to four families, composes a tanda. From a door-to-door census in each tanda, it was established that each hut is occupied by husband and wife, and also the children under ten years. It was also noted that boys over ten and girls over eight are not allowed to sleep in the huts of the parents. For such boys and girls of the entire tanda, two huts, separately for the boys and the girls, are constructed. The Birhor call these *gitichora,*[10] or the sleeping dormitory. The unmarried youths live in the gitichora till they are married and construct a new house to live with their wives. In case of the Manjira tanda, they did not possess any such traditional youth dormitory as there were no boys and girls above ten years. The Chatakpur tanda had two common huts for the youths, while in Narma tanda we found one family having a hut of its own for the grown-up girls.

When a boy is married and brings his wife to the tanda, he begins to live with her in the newly constructed hut. But living separately does not mean, in any way, separation from his parents. They work together, cook their food together, and for all socio-economic purposes they constitute one primary unit. But in course of time, within two to five years, quarrels arise on certain domestic issues, and separation of hearth takes place. With the separation of hearth, the one joint family breaks into two or more individual families. Now they work separately, cook separately, but worship the same Bonga Kumba, *i.e.*, the spirit hut that remains located at the back of the hut of the eldest member of the joint family. This common allegiance to the same Bonga Kumba keeps the different individual families of brothers in the tanda of their father. However, after the death of the father, the brothers may either continue in the same migratory group or may join other tandas individually at their convenience, and may install their own Bonga Kumba. In some cases, after marriage and death of his father, a man may join the tanda and migratory group of his wife's father.

Our studies in the four tandas bring to light a number of examples of such familial unity and discord. The following cases from Manjira tanda may further illustrate the point. Dugal and Budhu are both sons of Gend and both live in the tanda of their father. But Dugal with his wife and children has a separate hearth, where they cook and work separately, while Budhu, who has been recently married and has no issue, continues to cook and work together. Manjra's only son Sukhram (22) has recently separated his hearth from his father's because of a quarrel between his wife and his mother.

The genealogies[11] of Sukhdeo and Manjra show that all their sons, though married several years earlier, continue to live together in one tanda, and they move together with their fathers from place to place. The elder brother of Sukhdeo has also continued to move together, though his eldest brother Donu left them after the death of his father, Thukru. Genealogy and personal interview with Sanichar lead us to think that after his marriage he began to move with his wife's father, mainly because his mother married another man after the death of his father, and partly because his wife preferred to live with her father.[12]

A Birhor tanda may consist of more than one totemistic clan. In the case of the Manjira tanda, three clans, Kher, Induar, and Mura, are represented. Out of these three clans, Kher and Mura are of primary importance as each is represented by three to four generations in the tanda, while Sanichar is the only member of the Induar clan. Some tandas may consist of only one totemistic clan; for example, the Sirka tanda consists of only one clan and moves together in one group. In Narma and Chatakpur tandas we recorded more than one clan and also more than one migratory group.

These families and groups stay or migrate to other places at their own convenience. It is not at all essential that all the groups living at one place should leave the place simultaneously. However, when living at one place, these families and groups constitute one community and move about as one band from jungle to jungle in search of food and raw materials for ropes. In every tanda, or two tandas, there is a chief or Naya. The chief is selected by the will of the spirit made known through the ghost-doctor or *mati*. Every tanda has a mati. The mati is neither elected nor appointed; he is such owing to his spiritual command over ghosts and diseases. The chief of both the neighbouring Manjira and Serka tandas, Mangra, lives in Serka, and, at the time of social disputes, he convenes meetings of the elderly people of the tanda and makes necessary decisions in consultation with the other members of the tanda. Chatakpur and Narma have their separate chiefs. The function of the chief, as leader of the hunting expeditions, is also on the decline.

EXTENT OF MIGRATION

Regarding the extent of migration, the present investigator attempted to trace the names of places that the nine migratory groups had visited, and recorded the distance and the duration of stay at each place during the last twenty-five years. In several cases the informants did not recollect some of the past events to furnish me with the relevant data. On the basis of the data that have been collected, it appears that the duration of stay of a Birhor migratory group at one place varies from four months to five years. The distance that a wandering group covers every time also varies from four miles to sixteen miles. A comparative study also indicates that the distance covered by each group in the past, *i.e.*, fifteen years or more back, was more than that of the present time. The three migratory groups discussed below illustrate some of the points and give an idea of the pattern of nomadism among the Birhor.

The first migratory group of Manjira tanda headed by Genda (55) includes families of two sons, his younger brother, and two sons of his younger brother; the only person in the lineage to leave this migratory group was his eldest brother. He left the group after the death of his father, and now his sons and grandsons constitute a separate migratory group.

During the last thirty years this group has moved to several places, and various events of the life cycle have occurred in different places. While Genda was married at Manjira about thirty years before, his brother, Sukhdeo was married at Gilingca and his brother's two sons were married at Banari and Manjira respectively. Similarly, births and deaths of their children took place at different places. To mention a few, the third issue of Sukhdeo died at Hesa (10 miles from Manjira), his fourth issue died at Mundar (1 mile from Manjira), the fifth one died at Jehan (2½ miles from Manjira).

Now we come to describe the places they have occupied and the distances they have covered. During the last twenty years, it appears that the group has changed five places. Twenty years ago it migrated from one area to another in Simdega Subdivision of Ranchi District. Then it reached the Gumla Subdivision of the same district and began to wander in restricted areas of one police station to another. For the last thirteen years its movement has been further restricted to the forested villages of Bishunpur police station. So this group, from the stage of inter-subdivisional movement, came to the stage of inter-police-station movement, and now it has reached the stage of inter-village movement under the jurisdiction of the same police station. In point of duration of stay at different places, we find a gradual increase. The details may be tabulated as below.[13]

	Village	*Police station*	*Sub-division*	*App. distance from Bishunpur*	*Duration of stay*
1.	Biru	Simdega	Simdega		3 years
2.	Navagarh	Raidih	Gumla	40 miles	2 ,,
3.	Tendar	Ghagra	Gumla	8 ,,	1 year
4.	Simdari	Bishunpur	Gumla	4m. W.	5 years
5.	Serka	Bishunpur	Gumla	2m. SE.	4 ,,
6.	Manjira	Bishunpur	Gumla	1m. SE.	4 ,,

The other migratory group that inhabits the Manjira tanda includes the members of two families, one of Manjra (55) and his son and the other of his daughter with her husband and children. Nomadic events of this group are all the more remarkable. About thirty years ago it roamed near about Chakardharpur in the district of Singhbhum. Moving westward it entered the Simdeya subdivision in Ranchi district. It continued to wander in the villages of Simdega for five years, and then it came to Chainpur police station of the same subdivision. In this area its stay was about three years, and then for the last six years it has been wandering in the forested areas of Bishunpur police station. In this way the migratory group of Manjira is an example to show how a group goes from inter-district movement to the stage of inter-subdivisional movement, then to inter-police-station movement, and then to movements in the areas of the same police station.

These two migratory groups clearly indicate how the frequency in nomadic behaviour among the Birhor is slowing down. A section of the Birhor has already passed this stage and today is leading a sedentary social life under the name of Jaghi Birhor, as mentioned earlier. But still there is a section of the Birhor among whom the frequency of wandering is fairly great and the period of stay very short. Out of the nine migratory groups studied, three can be classified under this category. Here is one such case for illustration. This migratory group of Chatakpur tanda, led by Silwar's father, was wandering in Palkot police station. When Silwar's father expired, he together with the family of his brother, migrated to Bishunpur police station and settled at Gunga Toli, about half a mile from the Banari weekly market. After a four months' stay, they left Gungatoli and went to Mahuadanr in Palamau district, about fifteen miles from Bishunpur. They stayed there for five months and again came back to Gungatoli where they stayed for three months during the rainy season. During this period and at this place, his elder son Baijyanath was married. Then they went to Helta, about eight miles south-east of Bishunpur; they stayed there for six months. Then they came to Ankori about six miles from Helta and stayed there for about one and a half years. This was their longest stay at a particular place. From there they came to the Chatakpur tanda where they were living in September of last year.

SUMMARY

Thus, on the basis of the studies of the above settlements, we arrive at the following conclusions.

First, the location of the temporary settlements of the Birhor is mainly determined by three factors: (a) close proximity to the forest; (b) accessibility to the weekly market and some neighbouring villages; (c) availability of drinking water.

Second, with the impact of markets and adoption of rope-making craft by the Birhor, their exclusive forest economy has undergone modification, and with this their food habits, social behaviour, and nature of wandering have undergone a change.

Third, the Birhor settlement or tanda is no longer a homogeneous unit as it is composed of more than one wandering group as well as totemistic clans. Owing to this heterogeneity, economic competition and group rivalry are increasing, and the influence of the chief as the leader of the hunting expedition is fast dwindling.

Fourth, in recent times, the frequency in wanderings of the Birhor has gone down. Most of the wandering groups now cover little distance, varying from four to sixteen miles. Comparative statistics show that the distance formerly covered by them was much more than the present ones.

Fifth, the duration of stay of a wandering group at one place has also increased, and a part of them have left wandering and now lead a sedentary life.

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Notes

1. Census Report of India, 1951, Vol. I.
2. Census Report of India, 1941, Vol. I, Part III.
3. Vidyarthi, L. P. "Problems and Possibilities of Anthropological Researches in Bihar," *Bihar University Journal*, Vol. I.
4. Vidyarthi, L. P. "Economic Gradings of the Adivasi of Bihar," *Vanya Jati*, 4: (2), New Delhi (Hindi). Also *Indian Nation*, Feb., 1956 (English).
5. Dalton, C. *Ethnology of Bengal*, 1872.
6. Driver, W. H. P. "Notes on The Birhor," *J.A. Society of Bengal*, 57 : 12–15, 1888.
7. Risley, H. H. *Tribes and Castes of Bengal*, 1891.
8. Roy, S. C. *The Birhor*, 1925 (Ranchi).
9. Tanda-Birhor settlement. Some Birhor also call their settlement *Tandla*. Neither of the two is a Birhori word. *Tanda* is also used for their caravan by the nomadic Banjara of Rajasthan. The Lubana of the Punjab, once a nomadic community, also use *tanda* for settlement. It appears that *tanda* is used for a temporary settlement. It is still uncertain how and when the Birhor picked up this word.
10. In the Mundari language *Giti Ora* is the word for "Youth Dormitory." Instead of *Giti Ora*, I found the Birhor using the term *Giti Chora*, "Sleeping Platform." However, Roy has also used the word *Giti Ora* in his book, "The Birhor."
11. Vidyarthi, L. P. "Traditional Education among Tribal Bihar," *Bihar Educationist*, 2 : (4); "Education in Tribal Bihar." *Man in India*, Vol. 35.
12. Genealogical tables in the Appendix.
13. Vidyarthi, L. P. "Notes on the Wandering Families of the Birhor." *Proceeding of the All India Science Congress Association*, 1956.

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THE INTER-RELATIONS OF CASTES AND ETHNIC GROUPS IN NEPAL

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The caste-societies of India and other South Asian countries are attracting the attention of increasing numbers of anthropologists. While previously the phenomenon of caste was viewed mainly in a historical perspective and Sanskrit scholars endeavoured to trace the development of the Hindu caste-system on the basis of literary evidence, anthropologists are concentrating on the study of the present-day structure of castes and the inter-relations of the castes which constitute the societies of individual regions. Investigations of this type have led to a clearer appreciation of the characteristic features of a caste-society, but they have also shown that even within the sphere of Hinduism there is no uniformity in the inter-relations of the various castes and caste-groups. Despite the common acceptance of certain basic principles, such as the hierarchic order of castes, the correlation between status in the caste-hierarchy and ritual purity, and above all the endogamy of castes, there occur within the Indian subcontinent a great many local variations in the composition of society and the ranking of castes. When we leave the areas of Hindu predominance and turn to a country such as Ceylon, we find still greater deviations from the traditional pattern of the fourfold division of society, even though Singhalese Buddhists recognize the principle of hereditary and ritual distinctions in the rank of caste-groups.

Another country where Buddhist societies can be observed in constant interaction with Hindu societies is the Kingdom of Nepal. To the student of South Asian anthropology Nepal is of special interest. While in India Hindu society has been modified by centuries of Muslim, and lately European, rule, Nepal was never subjected to the prolonged influence of foreign conquerors. Moreover Buddhism, which in India has long ceased to be a socially significant force, survives in Nepal as an active religion and a philosophy with a profound influence on human relations. A special point of interest is the interaction between the hierarchically ordered Hindu societies and the numerous tribal populations which lack internal divisions of rank, though in their relations with Hindu and certain Buddhist castes they assume themselves the character of caste-groups.

The picture of Nepalese society is further complicated by differences of race and language. Through Nepal runs the dividing line between Europoid and Mongoloid racial types as well as that between Indo-Aryan and Tibeto-Burman languages. Even in the Kathmandu valley, locally known as the "Nepal Valley," the densely populated cultural and political heart of the country, we find populations belonging to the Indo-European and to the Tibeto-Burman world in close interaction. For the indigenous Newars, a people of Tibeto-Burman speech, live here side by side with Hindu immigrants from Northern India, who sought refuge in Nepal when Muslim armies invaded their homeland. Most of these immigrants are of high caste and they have influenced the local populations no less than they themselves have adjusted themselves to the ways of thinking current among the Buddhist people of Nepal.

A survey of inter-caste relations and the interplay of several separate hierarchies begins most conveniently with an outline of the social structure of the Newars, the predominant population of the Nepal valley, whose kings ruled the country until the Gurkha conquest in 1768. Newar society is vertically split into a Buddhist and a Hindu segment, but ethnic solidarity outweighs religious differences and a Hindu Newar feels more closely akin to Buddhist Newars than to Hindus of different language and ethnic background.

Both the Buddhist and the Hindu segments of the Newar people are hierarchically structured, and although there are altogether no less than sixty Newar castes, the broad outlines of these parallel caste-hierarchies can be reduced to a simple table:

Buddhist Newars	*Hindu Newars*
Guwaju (priests) Bare (inferior priests and gold- and silver-smiths)	Deo Brahman (priests) Jha Brahman (inferior priests)
Sheshyo (landowners and farmers)	Sheshyo Chhathari (Grade 6) Panchthari (Grade 5) Charthari (Grade 4)
Jyapu (peasants and agricultural labourers)	
Artisans	Artisans
Untouchables	Untouchables

The Buddhist part of the Newar community no less than the Hindu part presents the picture of an elaborately stratified society, and in this the Newars differ radically from other Tibeto-Burman-speaking Buddhist populations of Nepal, such as Sherpas and Tamangs.

Ideally all Newar castes are endogamous, but whereas in India the rigid endogamy of castes and subcastes is an immutable and essential feature of the Hindu social system, caste-endogamy is not strictly observed among the Newars and there is customary provision for inter-caste unions. The tolerance shown towards such unions is partly due no doubt to the Newar attitude to marriage, an attitude which differs fundamentally from that of Sanskritic Hinduism.

Unlike the marriage of high caste Hindus a Newar marriage is not conceived as a sacred and indissolvable union. For long before a Newar girl is given to a husband, she is married to a *bhel* fruit, and through this rite she acquires the ritual status of a married woman. All subsequent marital unions can be dissolved at will, and there is no objection to the remarriage of a widow or a divorced woman.

The attitude of Newars to inter-caste unions depends on the social status of the two castes concerned. Unions between Guwajus and Bares, the two highest Buddhist castes, for instance, are viewed with tolerance. The Bare bride of a Guwaju can enter the kitchen of her husband's family and join in the family worship. Her children have the ritual status of their Guwaju father and their mother's origin is not the cause of any social disability.

Such hypergamous unions are not the only example of inter-caste marriages. Social recognition is accorded also to hypogamous unions of Guwaju girls with Bare men, and in such cases the children belong usually to the caste of the husband. This rule, however, is not without exception and there are occasions when a woman married to a man her inferior only by one step in the caste-hierarchy can maintain her natal status and even pass it on to her children. This custom, which has no parallel in Indian societies, has made possible the development of a type of hypogamy known by the Nepali term *shiri charnu* ("climbing the ladder").

Among the Sheshyos, for instance, there are three divisions distinct in status, namely Charhari, Panchthari and Chhathari, terms which might be translated as Grade Four, Grade Five and Grade Six. A rich man's ambition to rise in status can be satisfied by his marrying a girl of the next higher grade. Thus a wealthy Sheshyo of Grade Four may be able to marry a girl of Grade Five, and their children, if supported by wealth, are likely to succeed in claiming Grade Five status, and may eventually marry partners of Grade Six and thus rise to the highest level of Sheshyo society.

This system of social advancement is basically different from that prevalent in most parts of India. There a man can gain in caste-status only if he changes his style of living in emulation of high caste customs and at the same time manages to carry a few inter-marrying families with him along the path of reform. These families then form the nucleus of a new endogamous subcaste of superior status. In Nepal, on the other hand, a man may rise above his own caste and follow a solitary path by advancement through favourable marital unions. Even major caste-boundaries can be crossed in that way. Thus it is not unknown for wealthy men of the peasant caste of Jyapu to marry girls of Sheshyo caste, and gain for their children recognition as Sheshyos.

We find thus that among the Newars the castes are not as rigidly endogamous units as they are in India. Intermarriage between castes or subcastes not too far removed from each other in social status is permissible, and if the social distance between a man and a woman is too great to allow of a formal marriage, there is customary provision for the establishment of a permanent sexual union which has some, though not all the implications of a marriage. Thus a man of high caste can live with a girl of considerably lower caste without losing his personal high status provided he does not eat ritually relevant food cooked by her. If the children observe the same precaution and do not eat from their mother's hand, they too may be granted the father's high status or, at least, a status considerably higher than that of their mother.

This principle is operative also in the relations between Newars and other ethnic groups. Though still the numerically strongest population in the Nepal valley and the three towns of Kathmandu, Patan and Bhatgaon, the Newars have long yielded political preeminence to a group of Gurkhali-speaking clans known collectively as Chetris, the highest of which have probably a Rajput strain. Ever since the so-called Gurkha conquest in 1768, Chetris have constituted the ruling race, one clan furnishing the Kings of Nepal, and another clan, known as Rana, providing the hereditary prime-ministers who held power until 1951.

The Chetris are orthodox Hindus, claim the status of Kshatriyas, and intermarry—particularly in so far as the royal family and the Ranas are concerned—with some of the princely Rajput houses of India. But unlike the Rajputs they are tolerant in regard to intercaste marriages, the social climate of Nepal being not conducive to exaggerated sensibilities regarding miscegenation.

Many Chetris have taken Newar wives, and such unions bridge not only the gulf between two castes but also that between an Indo-Aryan and a Tibeto-Burman-speaking people. Provided the Newar wife belongs to a caste ranking high within the Newar caste-hierarchy, the children from such a union succeed to their father's Chetri status as long as they do not accept ritually relevant food, i.e., cooked rice, from the hand of their Newar mother.

Even a great disparity in the respective rank of a Chetri man and a Newar girl is no obstacle in the way of a union approximating the character of a marriage. The children from such a union, however, are normally not accorded the status of Chetri, but will be known as Khatri, a term which designates them as the progeny of an intercaste union, without having any derogatory implication. The offspring of a Newar and a Chetri wife are also described as Khatri, and there are considerable status-distinctions between different types of Khatris.

There was official recognition of inter-caste marriages even among the Ranas. While only the sons from unimpeachable marriages between Ranas and girls of other high Chetri clans were in the line of succession to the positions of prime-minister and commander-in-chief, the children of Rana fathers and Newar, Tamang, Sherpa or other non-Chetri mothers were still recognized as Ranas —though of a lower class—and provided with less elevated positions in the administration and army.

The tolerant attitude towards inter-caste unions extends also to some of the Brahman castes of Nepal. Apart from the Newar Brahmans, there are three main groups of Brahmans in Nepal: Upadiya or Purbe Brahmans, Kumai Brahmans, and Jaisi Brahmans. The highest are the Upadiya Brahmans, and it is they who furnish the family priests (*purohit*) of most of the higher Hindu castes, including Jaisi Brahmans.

In principle each of these Brahman castes is endogamous, but cases of Brahmans living in marital unions with Newar or Tamang women are not infrequent, and such behaviour is not subject to any sanctions on the part of the Brahman community. The status of the Brahman husband remains unimpaired as long as he refrains from accepting cooked food from the hands of his non-Brahman wife, and the children, who have to observe the same restriction, are counted as Khatris and wear the sacred thread.

The Brahmans of Nepal have also shed the Hindu prejudice against divorce and widow-remarriage, and there is at least in some Brahman communities a recognized tariff of compensations which a man has to pay to a husband whose wife he has abducted. Neither the women who have left their husbands and entered such second marriages, nor their children, seem to suffer under any social disability.

As most Brahmans are undoubtedly of Indian origin, their very "unbrahmanical" standards in matters of marriage are obviously the effect of acculturation. The community of Jaisi Brahmans from whom I obtained most of my data on this subject possesses records listing 18 generations since their emigration from India, and, during the four centuries they have spent in the Nepal hills, they must have been in contact with populations in whose value system marital fidelity and the exclusiveness of a woman's sexual relationships do not rank as highly as in the eyes of high-caste Hindu society in India. We do not have far to look for such populations. The Newars traditionally accord their women the right to leave a husband and to marry again in his lifetime, and the polyandrous customs of Sherpas and Tibetans are diametrically opposed to the Hindu ideal of marriage. It appears that the example of such populations and the social philosophy of Nepalese Buddhism has created a moral climate, under the influence of which even the highest Hindu castes gradually modified their own attitude to inter-caste marriage as well as to the principle of the sacredness and permanence of the marital tie.

Another factor which may have influenced the Nepal Brahmans' attitude towards inter-caste marriage is the absence of occupational differences between themselves and other castes. When Indian Brahman communities emigrated to the hills of Nepal they lost their status as an economically privileged caste. Only a few individuals obtained employment at the royal court or in some of the Hindu temples. The majority settled in villages and took to tilling the soil. While in their homeland they were supported by numerous lower castes, they now form compact peasant communities consisting entirely of Brahmans except for an occasional blacksmith or potter. In the absence of menial castes, who could render them specialized services, they have themselves to perform many of those tasks which in India fall to the lower castes, and hence there is not the same feeling of superiority vis-à-vis cultivators of other castes and ethnic groups.

For the style of life of the Brahman peasant is much the same as that of the cultivators of any other caste, and this similarity extends to the diet, most Nepal Brahmans being meat-eaters. The great distinctions in habits and values which separate the Indian Brahmans from the agricultural castes do not exist in Nepal and this may account for the comparative ease with which a non-Brahman wife can be assimilated to the household of a Brahman peasant.

The lack of rigidity in the maintenance of caste-endogamy is not matched, however, by a general casualness in the observance of caste barriers. Newars no less than Brahmans and Chetris are extremely pollution-conscious, and are strict in their compliance with the rules of interdining. The acceptance of food from anyone except members of the same subcaste is subject to rigid restrictions. Severe sanctions, including in some cases excommunication, are imposed on those offending against the taboo on interdining with members of other and particularly lower castes.

So far I have dealt mainly with the multi-ethnic society of the Nepal Valley, which consists of Newars and a number of upper Hindu castes of Gurkhali speech. A society of similar composition is found also in a number of trading centres and in villages along the main lines of communications. Large areas of Nepal, however, are inhabited by populations which we may describe as tribal. Some of these, such as the Sherpas and Tamangs, profess Buddhism in its lamaistic form, whereas peoples such as Rais and Gurungs are neither Buddhists nor Hindus, but follow their own tribal religion.

The attitude of these tribes to social relations with other communities is fundamentally different from that of any of the Hindu communities or even the Buddhist Newars. Not only do they lack any fear of pollution through commensal or other contacts with members of communities other than their own, but they also readily admit such persons to connubium. The Sherpas, for instance, intermarry occasionally with such diverse populations as Tibetans, Newars, Gurungs, and Chetris, and the offspring from inter-ethnic unions are accepted as fully privileged members of the tribe.

The relationship between the hierarchically ordered caste-society of Newars, Brahmans and Chetris, and the casteless Buddhist and tribal societies is non-reciprocal. While the members of the former can easily be integrated in the latter, the multi-ethnic caste-society has erected certain barriers against a free social intercourse with those who do not subscribe to the concept of ritual purity and are hence considered polluting not only by high-caste Hindus but even by the Buddhist Newars, who fully share the Hindu preoccupation with ritual purity and pollution.

We have seen, however, that these barriers relate mainly to commensality and are surmountable in so far as sexual congress is concerned. Marital unions between caste-Hindus and tribal women are tolerated and there is customary provision for the acceptance of their progeny within the multi-ethnic, hierarchically structured caste-society.

If we compare this caste-society of Nepal with the caste-societies of India, we realize that a number of principles operating in the Indian system are lacking in Nepal, and that a hierarchic caste-order can be maintained even in the absence of principles usually considered inseparable from the Hindu caste-system.

A rigid endogamy of castes, mitigated only by hypergamous relations between closely allied sub-castes, has always been regarded as a basic feature of the Indian caste-system. In Nepal we find that a caste-system can operate without this strict insistence on endogamy, and that men retain a high ritual status even though they live in marital unions with women of greatly inferior status. While acceptance of cooked food from the hands of a person of lower status has the effect of depriving the recipient of his own high status, sexual intercourse does not have this effect and is a neutral action in so far as caste-purity is concerned. In the absence of the traditional Hindu horror of the "confusion of castes," no stigma attaches to the children from inter-caste unions; they either succeed to the father's caste-status or, if the social distance between the parents was too great to allow of such a course, are accorded an intermediate but still entirely respectable status. This provides the possibility of the emergence of new caste-groups known under the collected term Khatri, a possibility which does not exist in India, where new castes arise not so much from miscegenation but as a result of fission within an existing caste.

Another fundamental difference between the Indian and the Nepalese caste-systems is the possibility of individual mobility within the rank order expressed in the custom of *shiri charnu* ("climbing the ladder") whereby marriage with a girl of higher status can improve a man's own rank and that of his children.

The study of Nepal society, which I began with a preliminary survey in 1953 and hope to continue next year, is not sufficiently far advanced to warrant definite conclusions. But the data here reviewed suggest that the immutability and rigidity of endogamous units should not be considered essential features of every caste-system. The concept of ritual purity and of pollution through social and particularly commensable contacts, on the other hand, appear fundamental to the maintenance of a caste-hierarchy. In the Buddhist and tribal societies lacking these concepts there are no caste distinctions, and in the multi-ethnic caste-society of the Nepal valley it is these concepts which uphold the social distance between the individual castes.

Many Indologists are of the opinion that the extreme rigidity of the Hindu caste-system is a development of mediaeval times and that ancient Hindu society was in many respects more flexible. There was then no absolute bar against marital unions between members of different castes, and, while caste endogamy was always the ideal, there existed customary provision for fitting the offspring of inter-caste unions into the body of Hindu society. A similar position persists in present-day Nepal: a multi-ethnic caste-society has retained a flexibility which permits a limited social interaction not only between such distinct ethnic groups as Chetris and Newars but even between Hindu castes and the casteless societies of Buddhist and tribal populations.

London, England.

SECTION V

ARCHAEOLOGY

HUMAN TYPES AND PREHISTORIC CULTURES AT KSÂR 'AKIL, LEBANON

J. Franklin Ewing, S.J.

The human events which are briefly described in this paper occurred in the period from the upper part of the Middle Paleolithic to the basal section of the Mesolithic, in the Lebanon. We shall briefly describe the geology, the cultural events, the paleontology, and the human remains, all of which characterized this stretch of time.

GEOGRAPHY

Ksâr 'Akil is approximately 10 kilometers by road and trail to the northeast of Beirût, capital of the Lebanese Republic.

It is in the valley of Antélias, which valley is named for a town on the coast, some 3 kilometers from our site. The valley of Antélias, a short distance above a spring and a cave, divides into two. Our site is on the north branch of this division. On the south branch we have a cave described in the publication of Zumoffen under the name of Grotte d'Antélias. The native name is Mugharet el-Bileni. Further up the south branch is the Abri Bergy which we have named after Père Bergy, of the Université St-Joseph in Beirût. Interestingly enough, atop the hill which stands between the two branches of the Wadi Antélias is an ancient Semitic high place.

Thus noting a long tradition, we should also remark on the fact that the Wadi Antélias must have been a fertile and well-furnished habitat for ancient man.

The site of Ksâr 'Akil is a rock shelter, facing south as rock shelters do.

HISTORY OF EXCAVATIONS ON THE SITE

A somewhat romantic history brought about the recognition of this site as a scene for scientific investigation. A Lebanese who was searching for ancient treasure in the Wadi Antélias decided that the Mugharet el-Bileni had been dug over by too many people to have any treasure left. He noted, however, that a peculiar hollowed-out rock formation above the cave was echoed in the north branch of the valley above our site. At Ksâr 'Akil he sank a shaft some fifteen meters deep, against the rock wall (naturally, the richest part, from the point of view of a prehistorian), and the stone tools and fossilized bones which he dug up were recognized as such by a geologist from the American University of Beirût, Dr. Day. Dr. Day succeeded in retrieving some of these objects, and through him people in Europe knew that we had to deal with at least an "Aurignacian" site.

A subsequent, and very passing investigation by Passemard[1] caused a trench to be dug against the face of the rock shelter, thus robbing us of valuable material.

Excavations in a scientific way were begun in 1937 by the Rev. Joseph G. Doherty, S.J., then a graduate student at Cambridge University. The writer joined him in 1938, and subsequently took over the directorship of the excavations.[2]

In the course of these excavations, we have had the technical assistance particularly of the Rev. Joseph G. Murphy, S.J., of Fairfield University, of Professor Herbert E. Wright, Jr., of the University of Minnesota, and the consultant help of many other people.

GEOLOGY

The immediate geology of the site indicates an outstanding characteristic. These are the three Complexes. The typical Complex is composed of two layers of angular, limestone flakes, one above and one below a layer of relatively sterile red clay.

According to our current estimate of these Complexes, an estimate on which we are backed up by every geologist who visited the site, they were laid down during a very humid period. In other words, they represent local "pluvials." This affords us the possibility of correlating Glaciation dates of Europe with our own Near Eastern scene.

Our current opinion is that the three Complexes represent correlations with Glaciation IV Stadials 2, 3 and 4.

In a previous note,[3] we represented these Complexes quite otherwise. This statement represents our considered opinion on the Complexes, after a prolonged study.

We should mention here the fact that our geologist, Dr. Wright, considers the whole of our excavation to be safely within the last Glaciation period.

CULTURAL EVENTS[4]

From the bottom (22.6 meters below datum) of our excavation at Ksâr 'Akil, to shortly below 15 meters below datum, we have a culture known as the Upper Levalloiso-Mousterian. This industry is similar in general character to that found in the upper levels of Tabūn, Skhūl and other sites in Palestine and Syria. In a few sites hand-axes were found with this industry, but only in its early stages; none were found in Ksâr 'Akil.

The implements from Ksâr 'Akil are in general smaller than those found in the Palestine sites, and the broad Levallois flakes so typical of the other sites are rare and small.

In general, all the tools from Ksâr 'Akil are small. It is possible that the type of flint found in the neighborhood (nodular) as opposed to the type of flint found in the neighborhood of Mount Carmel (tabular) may have had a great deal to do with this difference.

Further study is needed in order for us to be absolutely accurate about the relative typological relationships between the industries of Mount Carmel and those at Ksâr 'Akil; however, the industry present at the 3rd Complex suggests that the Levalloiso-Mousterian has come to an end.

In the red clay between the two stony layers of Complex 3, a part of a human maxilla was found. Inasmuch as we consider this specimen to be Neanderthaloid, the associated archaeological material is important. Unfortunately, in this layer there were very few implements available; however, the material to hand,

and the material from the layers above and below, give us a very good idea concerning the place of the industry to be associated with this find.

The most important single object was an Emireh point. This point, made famous by Dr. Garrod, is otherwise associated with an industry of mixed Levalloiso-Mousterian and blade-burin-end-scraper elements. This mixture belongs to the beginning of a development of which there are at least two, both found at Ksâr 'Akil and both preceding the Aurignacian. Whether we consider the layer in which this point occurs to be late Levalloiso-Mousterian or the beginning of the subsequent series of developments is of little importance. In any case, it is the beginning of a transitional period.

Another important point is: how does our specimen compare in age with the human remains from Mount Carmel? There can be no question that the Ksâr 'Akil fragment is later than the skeletal material from Mount Carmel. It therefore appears that our fragment brings the age of the Neanderthaloid up to a point considerably later in time than had previously been known.

The industry which succeeds the Emiran is descended directly from it. It continues with some development to the base of the 2nd Complex, after which the Aurignacian begins.

The significance of these cultural remarks for our human remains is as follows:

The completely modern type of skeleton which was unearthed at 11.46 meters below datum is associated with an industry which is in a direct line of descent from the industry associated with the Neanderthaloid half-maxilla. This in turn is derived from the Levalloiso-Mousterian, the industry of the Mount Carmel men.

PALEONTOLOGY[5]

Besides remains of fishes, amphibians, reptiles and birds, representatives of some twenty-seven mammals (other than *Homo*) are found at Ksâr 'Akil.

The best represented species is *Dama mesopotamica* (a total of 10,000 specimens). This is also well represented at Mount Carmel. However, the second best represented element here is not *Gazella* (as at Mount Carmel), but *Capra* (4,000 specimens). At no point does *Gazella* ever outnumber *Dama*. *Capreolus capreolus* is the third commonest species at Ksâr 'Akil (almost 3,000); this species is very rare at Mount Carmel. *Cervus* and *Bos* are generally rare at both sites.

The most interesting of all the sections into which the paleontology of Ksâr 'Akil may be divided is that from level XXVIA to XXIXB inclusive (approximately 15.5 to 17.5 m.). In this section we notice a very marked change in the composition of the fauna. *Dama*, as always, is in the lead, but *Rhinoceros* and *Bos* are much more abundantly represented than they are at any other part of the sequence. In fact, all the *Rhinoceros* remains found at Ksâr 'Akil are from levels XXVIA–XXIXB. Noticeable is the small number of specimens of *Capra* in this section: it is less than one-tenth that of *Dama*, while in other sections of the sequence the number of *Capra* specimens is roughly one-third to two-thirds that of *Dama*. *Bos* is over four times more abundant than is *Capra*, while in other sections *Capra* is at least six times more abundant than is *Bos*.

Somehow *Rhinoceros* and *Bos* must have replaced a good deal of *Capra* on the Paleolithic menu during the time span represented by levels XXVIA–XXIXB. It should be emphasized that this is a temporary change, for the composition of the fauna in the bottom section of levels XXX–XXVI is much the same as that in the sections from level XXV on upward.

This phenomenon is interesting inasmuch as a faunal change seems to have taken place immediately before the occurrence of the Neanderthaloid specimen found in layer XXV. However, its interpretation, at this time, is very doubtful. Cultural choice may be cited, although there is no lithic correlation. The fact that the period was humid may argue for the continuation of *Rhinoceros* on the coastal plain, and, to a certain extent, for a favorable habitat for *Bos*.

HUMAN REMAINS

(a) *Neanderthaloid fragment.* Only very tentative conclusions may be drawn from this fragment, found in layer XXV.

The fragment consists of part of the right maxilla, together with a small portion of the left. No teeth are present, except for part of the root of the right canine. The lower borders of the Apertura piriformis are absent, and only the base of the Spinum nasale is indicated. This and the subnasal section of the left maxilla are crushed posteriad; the borders of the alveoli are eroded. There is no observable Fossa incisiva.

However, we may make the following positive remarks. From the alveoli, we may judge that the first molar was very large, in fact extremely large in comparison with the premolars, canines, and incisors. The A-P and/or linguobuccal dimension of this tooth could have been as large as 13 mm. This (even allowing for subjective exaggeration) would make the tooth larger than, or at least as large as, the corresponding tooth of Skhūl V or even Tabūn I.

The incisors, canines and premolars, while probably within the modern range, did not lack robusticity. The crushing of the anterior portion of the maxillary fragments interferes with our estimate of the size of the incisors, but not that of the canine and premolars. The impression is very strong that we are here dealing with reduced anterior teeth (premolars, canines and incisors) but with very large molars (judging by M^1).

An attempted reconstruction of the total dental arcade indicates an outline which is somewhat more U-shaped, and less parabolic, than is the case with modern arcades. Certainly the impression is gained that there is a definite break between the anterior teeth and M^1, the latter being more modern, the M^1 being larger than modern, and, so to speak, impinging on what would otherwise be a modern palate and dental arcade.

An estimated breadth (at the alveolar border of the two first molars) would be some 51 mm. This is small, compared with a similar breadth for Skhūl V, and other Neanderthaloids. Our impression is that the fragment belonged to a female.

In summation, we may state these conclusions.

If we were to look for a midway form between the progressive Neanderthaloid of the Lower Levalloiso-Mousterian skeletons of Mount Carmel and the modern (H. sapiens) form which we found at 11.46 m. at Ksâr 'Akil, we should find it in the type which left its fragment in our layer XXV. Of course, our conclusion is limited to its premises. A fragment of jaw cannot tell us the state of supraorbital ridges, nuchal region, etc.

(b) *Homo sapiens.* In layer XVI were found the skeletal remains of a completely *sapiens* child. Its age at death was approximately seven and three-quarters years; this estimate of age is derived from an analysis of the X-rayed teeth.

The first permanent molars are fully erupted, and the crowns of the second permanent molars are well advanced in development. The lateral permanent upper incisors, while not erupted, are well along the way towards eruption.

In view of the state of preservation of the total skeleton, it was decided to concentrate on the reconstruction of the skull. Although we believe that the postcranial skeleton has in general not been sufficiently exploited by anthropologists, we found the state of preservation of the skeleton so deplorable that the skull only has been extracted and reconstructed. In general, the skeleton was embedded in breccia, most of the skull being found in a much softer matrix.

The body was originally buried, under a pile of stones. Two upright stones, supporting a large horizontal one, were evidently displaced soon after the burial. The horizontal stone fell, badly damaging the skull, especially the right side. However, we have been successful in reconstructing the skull in what we hope to be a true manner.

The skull is at the upper limit of the range of dolichocrany. Its cranial index is 74.25. It is moderately high (105 mm.). Its facial development is modern. Its teeth are in the modern range, and its mandible is very modern.

Even allowing for the subjective element in the reconstruction of the skull, we still think that it has, and should have, a quite Mediterranean aspect.

In fact, we feel safe in saying that this specimen is not only the oldest *sapiens* form found in the Lebanon, but the oldest representative of the Mediterranean race.

SUMMARY

Ksâr 'Akil, a Paleolithic site near Beirût, Lebanon, is a key site in the Near East. It yields flake and blade industries, and industries transitional between flake and blade. Human remains have been found which are associated with the beginning of the transitional cultures (Neanderthaloid) and associated with the beginning of the blade industries (sapiens).

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New York, New York.

Notes

1. E. Passemard, "Mission en Syrie et en Liban," *Bulletin de la Société Préhistorique Française*, 24: 70–72, 1927, Paris; and L. Delcourt, "Observations sur l'Abri de Ksâr 'Akil (près d'Antélias, Liban)," *ibid.*, pp. 56–61.

2. These excavations were subsidized by Boston College and by Fordham University. The 1947–48 seasons were made possible by a grant from the then Viking Fund. The work of our geologist, Dr. Wright, was assisted by a grant from the Oriental Institute of th University of Chicago.

3. Ewing, J. Franklin. "Preliminary note on the Excavations at the Palaeolithic Sit of Ksâr 'Akil, Republic of Lebanon." *Antiquity*, 21 : 186–196, 1947.

4. We hope soon to publish a full report on Ksâr 'Akil. In the meantime, Dr. John Waechter of the University of London (who will write up the lithic industries) has supplied me with the basis for these notes.

5. Dr. Dirk Hooijer, of the University of Leyden, who will report on the paleontology of Ksâr 'Akil in our final publication, has supplied the basis for these notes.

*

CULTURE CHANGE IN EUROPE AT THE START OF THE SECOND MILLENNIUM B.C.

A CONTRIBUTION TO THE INDO-EUROPEAN PROBLEM

Marija Gimbutas

The final solution of the problem of the origin of the Indo-European speaking people will be brought about only by an exquisite synthesis of results achieved by linguists and cultural and physical anthropologists. Admittedly some branches of research, such as archaeology, have not yet reached sound enough results to be widely used for this interdisciplinary synthesis. Also, many questions which could be answered by archaeological means are far from solved. Nevertheless, archaeology, even in this state, is of basic importance in the search for the homelands of the Indo-Europeans. Its role is growing steadily with the accumulation of finds and the improvement of methods. We are already in a position to acknowledge certain significant data as key evidence for answering such questions as: from where and when did Indo-European speaking people spread into the continent of Europe. One key piece of evidence is the culture change at the start of the second millennium B.C.

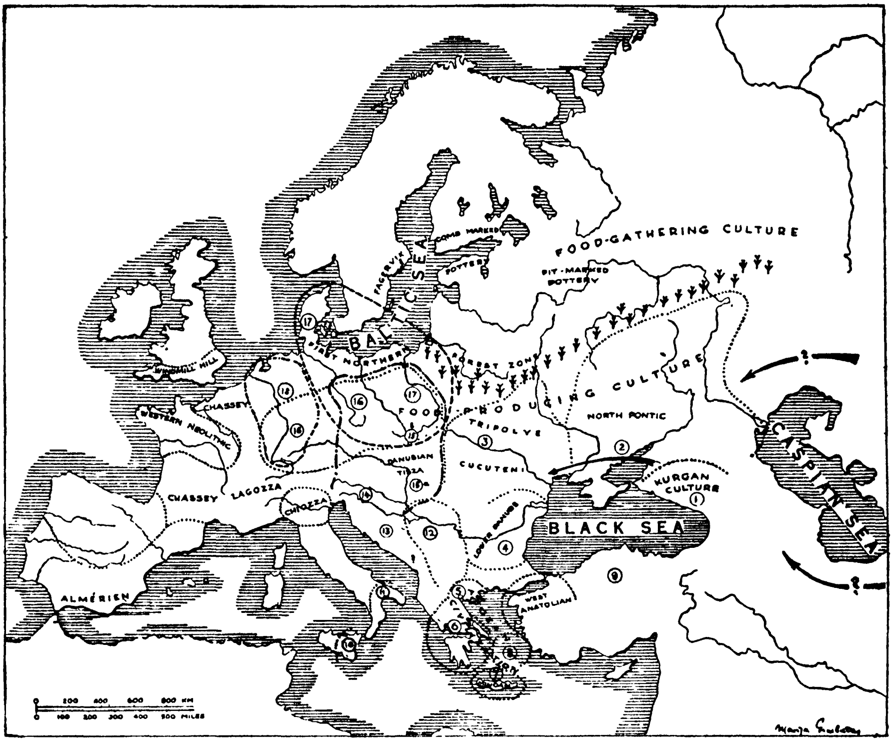
In this short paper I wish to show schematically and in summary by means of maps and illustrations the nature of this cultural change. In order to clarify the picture of cultural continuity and change, three maps will be presented. Map 1 will show cultural groups in Europe at the end of the third millennium B.C. Map 2—the migration period at the start of the second millennium B.C. with directions of diffusion of new cultural elements. Map 3 will show distribution of cultural groups in the period immediately after the migration.

MAP 1

At the end of the third millennium B.C. there existed several cultural blocks, differing distinctly one from the other. The continent of Europe was divided into peripheral zones and culture centers. The highest culture was around the Aegean Sea, and its influence radiated northwards to the Balkans and via the Danube to Central Europe, along the Black Sea coasts to the Ukraine and by sea to the western Mediterranean area. The culture of Western Europe and that of the forested zone in northeastern Europe were peripheral, the latter forming a long-lasting preserve of hunters and fishers.

Cultural groups at the end of the third millennium B.C. show a gradual continuity from the earlier period. Most of the entities indicated on the map have their beginnings in the fourth millennium B.C. or earlier.[1]

Cultural groups between southern Scandinavia, the Mediterranean Sea and the Black Sea were agricultural. So far as is known at present, in many of these


FOOD-GATHERING CULTURE
COMB MARKED
POTTERY
PIT-MARKED POTTERY
FAGERVIK
BALTIC SEA
FIRST NORTHERN
FOREST ZONE
FOOD-PRODUCING CULTURE
NORTH PONTIC
TRIPOLYE
CUCUTENI
DANUBIAN
TISZA
KURGAN CULTURE
BLACK SEA
CASPIAN SEA
LOWER DANUBE
WEST ANATOLIAN
WINDMILL HILL
CHASSEY
WESTERN NEOLITHIC
LAGOZZA
CHIOZZA
ALMÉRIEN
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15a
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0 200 400 600 800 KM
0 100 200 300 400 500 MILES

Map 1. Cultural Groups at the End of the Third Millennium B.C.

1. Kurgan culture of Early Kuban period.
2. North Pontic culture: Mariupol, lower Dnieper, and other sites preceding the earliest pit-graves of the Kurgan culture.
3. Painted Pottery culture: Tripolye and Cucuteni complexes.
4. Lower Danube culture: Gumelniţa and Bulgarian Mound (Tell) complex.
5. Early Macedonian Bronze Age culture.
6. Early Helladic III culture.
7. Early Minoan III culture.
8. Early Cycladic culture.
9. Central Anatolian Copper and Early Bronze Age.
10. Painted Pottery complex (intrusive from the Balkans) and continuing Stentinello traditions.
11. Painted Pottery complex (Balkan origin).
12. Morava-Vardar culture (Vinča D, Bubanj IIa, b).
13. Butmir complex.
14. Slavonian and western Hungarian Legyel complex.
15. Middle Danubian culture: 15a: Tisza (Tiszapolgar) complex in Hungary. 15b: Jordansmühl and Painted Moravian groups in Bohemia, Silesia and southern Poland.
16. Banded Pottery culture (Bandkeramische Kultur): Rossen, Stroked Pottery, and Late Linear complexes
17. First Northern culture: Funnel-necked Beaker complex, phases C and D.
18. Michelsberg complex, related to Funnel-necked Beaker complex.

groups people lived in valleys in large village communities and built large, usually long houses. The dead were buried in caves, pit-caves, trench-graves, regular flat graves or in megalithic collective tombs, sometimes of gigantic size, as in the Funnel Beaker culture. In the Aegean area and the Balkans pottery was of outstanding workmanship: monochrome or polychrome painted, incrusted, stamped, incised or excised, etc., and was of a rather sophisticated, frequently artistic, shape (decorated predominantly with spiral and plant motifs and their combinations). The stone or clay female figurines in the Aegean, Mediterranean, Balkan, Danube and Pontic areas may denote a predominant cult of a mother goddess. A female deity is frequently portrayed in association with bulls or other male animals, horns and other fertility symbols. The system of symbols, the importance of a female deity and portrayals of woman in art, allow the assumption that woman had a significant role in religion and in society.

No sudden changes involving the whole continent of Europe are evidenced in the third millennium B.C. Only gradual economic change, relatively rapid spread of agriculture toward northern Europe and local shifts of farmers as well as commercial relations can be traced. The Painted Pottery people of Tripolye spread northwestward at the expense of the Linear Danubian group. The Middle Danube (Tisza) group expanded northward up to northern Poland. Between the Danubians and the First Northern (Funnel-Beaker) groups there was a continuous competition for better lands as mixed assemblages show. Each cultural group had its own character and way of development. There is no doubt about the continuity of such entities as the North Pontic, Tripolye, Lower Danube, Northern Balkan, Danubian and First Northern cultures. The Mesolithic and Sub-neolithic groups in the north and west also display a continuity in gradual change.

In the northern Caucasus, along the river Kuban and the shores of the Black Sea a vigorous group of people appeared with knowledge of agriculture, stock-breeding, copper, gold and silver. They buried their dead in house-like structures of stone or timber under large barrows or "kurgans" ("kurgan" is the Russian word for barrow). Hence the designation "Kurgan Culture." One type of their burials, as at Maikop and Tsarskaia, contained tremendous amounts of jewelry made of gold, silver and precious stone, silver and clay vessels, animal sculptures of copper and silver as well as copper tools, cauldrons, etc. Other graves had globular pots, sickles with flint inserted, flint knives, scrapers, red ochre, etc. A distinct system of classes is clearly seen in this differing grave equipment: the richly furnished royal, or other high class, burials and the regular graves, equipped with merely a flint tool and a pot. Habitation sites, comprising small villages and cemeteries were set on high river banks. The remains of houses and the house-like structures in kurgans, suggest that they lived in small rectangular houses.

This grave inventory has its nearest analogies in Transcaucasia, northern Iran (Hissar II and III) and the Anatolian Copper Age complex. Through these the Kurgan culture of the Early Kuban period can be assigned to the last centuries of the third millennium B.C.

The Kurgan culture is related to the Near Eastern civilization, as seen from metal art objects and the limited evidence of social structure. It stands in rather sharp contrast to the North Pontic culture and other Neolithic or Chalcolithic cultural entities in the Balkans, central and Baltic Europe. The Kurgan People emerged rather suddenly without having predecessors in the same area or at least it is still too early to speak about the culture sequence between the Neolithic and Copper Age stage in the Caucasus. From the present material it seems that the intrusion of the Kurgan people into the northern Caucasus from the southeast or east is probable. The Caucasus Mountains, as early and later history has shown, were not impossible to cross. However, the solution of the problem of from where and by which way the Kurgan people came to the northern Caucasus or whether they developed locally, must be left for future research. At present the area around the Caspian Sea is far too little known.

Shortly after their appearance in the northern Caucasus, the Kurgan people spread along the northern coasts of the Black Sea as far as the Dniester River in the west at the expense of the local north Pontic culture.

Their rapid advance over the north Pontic area is definitely proven by archaeological finds. Totally new elements of culture were brought to south Russia and the Ukraine by the Kurgan people during the so-called Pit-grave phase. They are: (a) social (distinct system of classes and of patriarchy), (b) religious (new burial rites: burial mounds, graves in form of house imitation, contracted position of the dead against extended, numerous domestic animal bones in graves, ritual importance of the horse, dominance of symbols associated with the sky deity, particularly often solar symbols), (c) architectural (small rectangular houses in hilltop settlements as against valley sites), (d) economic (vehicles and predominance of stock breeding over agriculture), (e) technological (greater number of copper artifacts, new pottery decoration and hammer-axes of stone), and (f) racial (a delicate, but tall Mediterranean type contrasting with the local north Pontic massive wide-faced Crô-Magnon type people). These new elements, found between the lower Volga and the lower Dniester, may belong to around 2000 or the last centuries of the third millennium B.C.

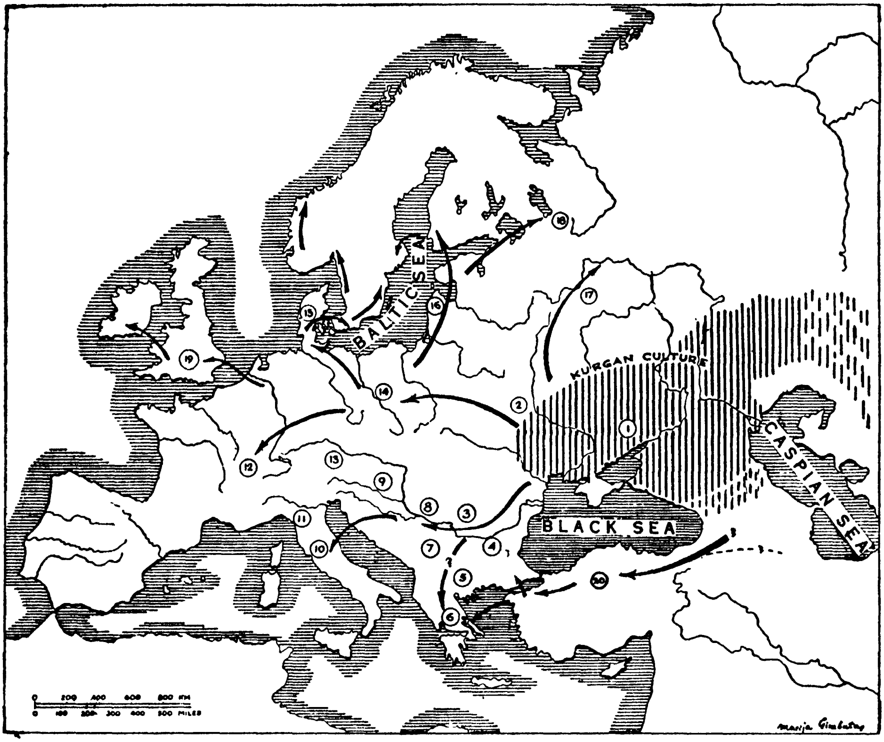
The next stage of the Kurgan culture in the northern Caucasus, southern Russia and the Ukraine shows several phases of its development as seen from the stratigraphy. In a later phase, called Early Middle Kuban in the northern Caucasus and Hut-grave phase in southern Russia and the Ukraine, at the beginning of the second millennium B.C., pottery was decorated chiefly by cord impressions and incisions. The predominant motifs were hanging triangles, horizontal impressions, rows of incisions or a herringbone motif with a chisel-like implement (Figure 1). Hammer-axes or "battle-axes," which attained a wider distribution during this phase, were already known at the end of the third millennium B.C. Metallurgy blossomed at this time in the Kuban-Terek basins. Artisans were masters of fine jewelry technique, stamping and embossing.

The cultural pattern of the Kurgan culture north and east of the Black Sea at the beginning of the second millennium B.C. is of utmost importance since undoubtedly related elements occurred at the same period in the larger territory of Europe.

MAP 2

The elements which were brought first to the north Pontic area also diffused in the beginning of the second millennium B.C. over the Balkans, central Europe, the Apennine peninsula, northwestern Europe, the southern and eastern Baltic area and central Russia. Two main routes of diffusion are evident: one—south of the Carpathian Mountains to Serbia and along the Danube to Hungary and Austria; the other—north of the Carpathians to central Europe and the Baltic area. The long-lasting local cultures began to disintegrate. No undisturbed cultural continuum can be recognized; the process of destruction of distinctive cultures and a trend toward uniformity had set in.

The new elements typical of the Kurgan culture appeared in the above mentioned parts of Europe. In some areas almost the whole complex of these is to be found, in others only some of them. This depends on how well the area is known archaeologically, and how strong the influence of the cultural substratum was. New burial rites with barrows, house-like structures, mostly stone



Map 2. Diffusion of the Elements of the Kurgan Culture at the Beginning of the Second Millennium B.C.

1. Kurgan culture: Hut-grave complex (second phase of "Pit-graves") in southern Russia and the eastern Ukraine and the early "Middle Kuban" complex in the northern Caucasus.
2. Middle Dnieper complex showing Kurgan elements.
3. Transylvanian complex with intrusive elements (Marodécs cemetery; "Furchenstichkera mik").
4. Corded Pottery and Battle-axe complex in Bulgaria (Karanovo mound, layer V, central Bulgarian Brezovo finds).
5. Final Early Bronze Age culture of Macedonia.
6. End of Early Helladic III (Eutresis and Hagia Marina finds including corded Pottery and Minyan ware on the other, showing west Anatolian influences).
7. West Serbian tumuli with burial rites reminiscent of Kurgan burial.
8. Srpski Ksztur finds of northern Yugoslavia.
9. Baden (pecél) and Bodrogkereshtur complexes.
10. Rinaldone complex in central Italy.
11. Remedello-Polada complex of northern Italy.
12. Rhine groups of corded pottery.
13. East Alpine culture: Mondsee complex.
14. Globular Amphora complex: First Northern (Funnel-necked Beaker) and Kurgan hybrid.
15. Kurgan elements in the Globular Amphora complex; earliest phase of Corded and Battle-axe complex in northern Germany, Denmark, and southern Sweden (corded beakers of A-group, "Under-grave" complex).
16. Earliest phase of East Baltic Corded and Battle-axe complex.
17. Central Russian Fatianovo complex.
18. Intrusive elements in northwestern Russia (beakers, V-perforated amber beads, etc.).
19. Beaker complex in the British Islands.
20. End of the Early Bronze Age in central Anatolia with evidence of destruction. Coming of the Hittites.

cists, evidence of horse, sheep and cattle sacrifice, and horse bones in graves and habitation sites, strike the eyes of prehistorians as contrasting to the local traditions. Different conditions of settlement are implicit in the hilltop sites as contrasted to the earlier valley sites, and in the fortifications of the villages.

Map 2 shows possible routes of diffusion from north of the Black Sea. The open Pontic grasslands served well as a corridor. A further track led to the fertile loess lands and sources of copper, salt, flint and amber in the Balkans, central Europe and the Baltic area. This is indicated by a particular concentration of finds, definitely belonging to the intrusive people, in metal, salt, flint and amber source regions. In central and Baltic Europe they established commercial relations with the Bell Beaker traders and remarkably accelerated exploitation of local copper ores and Baltic amber. These new people can be regarded as stimulators of the development of metallurgy. To the forested zone of northeastern Europe they brought the food-producing economy. In general, the Kurgan people seem to have possessed an extraordinarily aggressive and daring nature. The two- and four-wheeled wagons, which are now reported from north of the Black Sea and from Hungary, seem to have served to overcome long distances during migration.[2]

The numbers indicate the location of find assemblages, which contain intrusive elements of related or even identical nature. As an illustration the author has chosen to show the spread of a single technological phenomenon—the well known "corded pottery" with a specific decoration by cord impressions and incisions. The same motifs are known from the areas separated by thousands of kilometers and in many localities. Such similarities could not have originated independently. (Figures 1–10.)

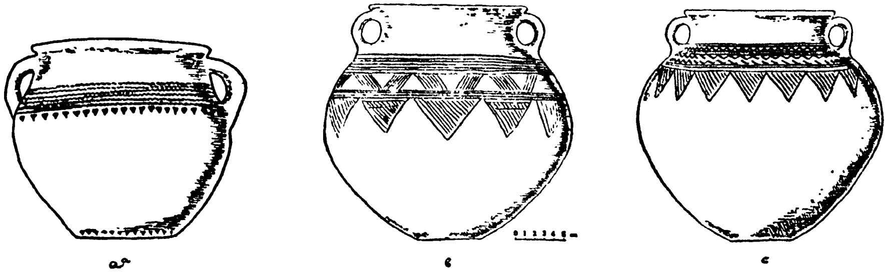


Fig. 1. Northern Caucasus

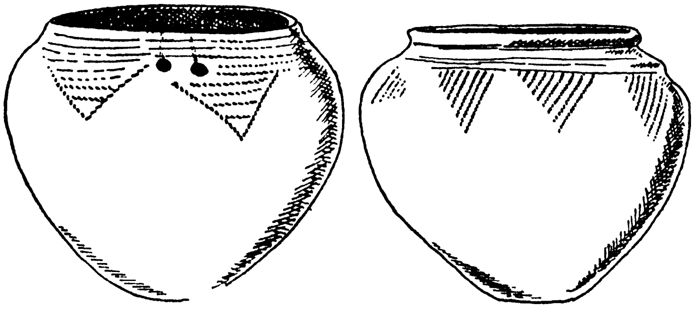


Fig. 2. Southern Russia

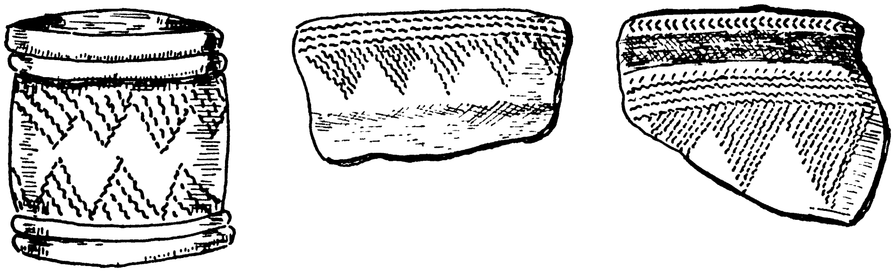


Fig. 3. Rumania

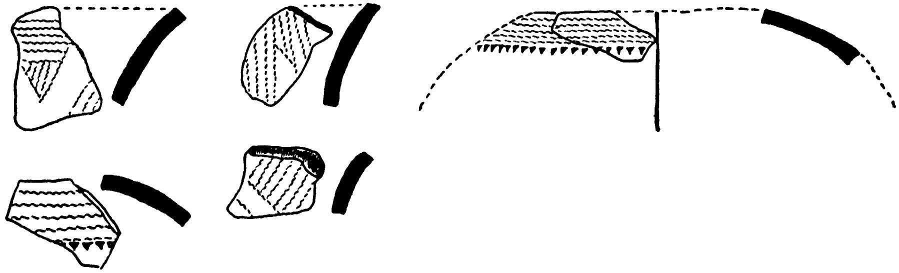


Fig. 4. Greece

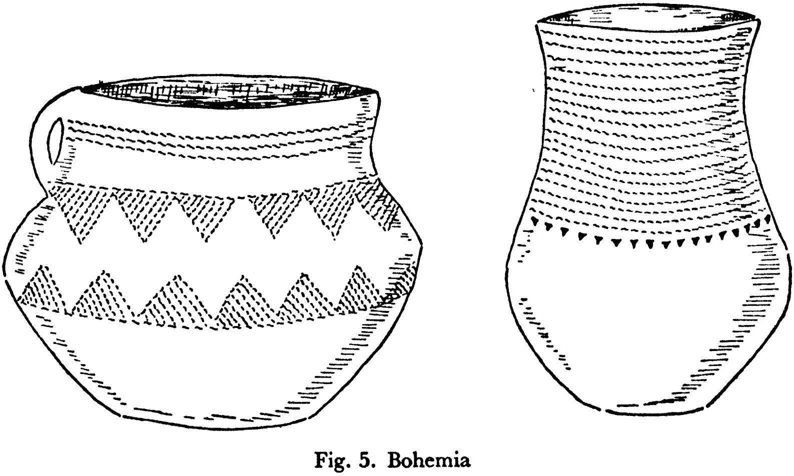


Fig. 5. Bohemia

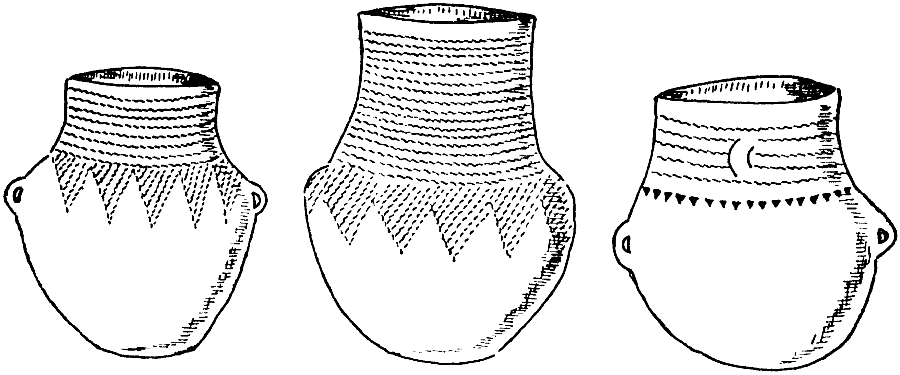


Fig. 6. Saxo-Thuringia

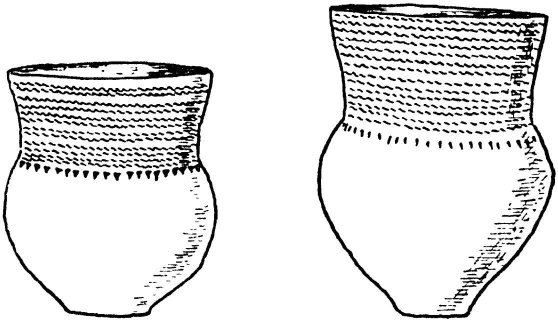
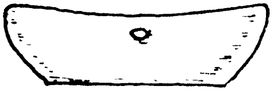
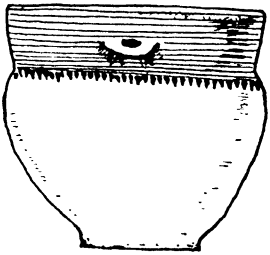
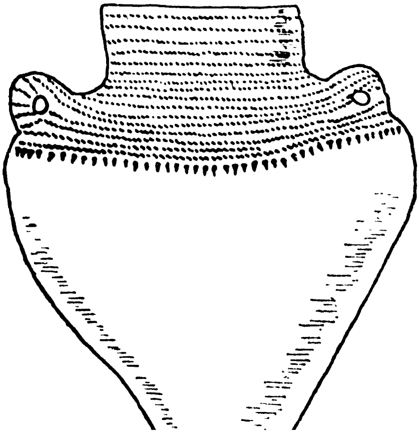
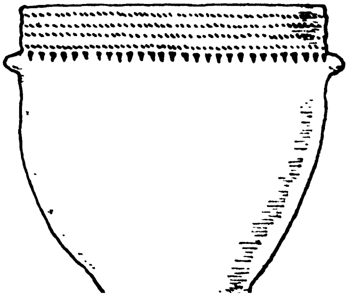


Fig. 7. Northern Poland on the Baltic Sea









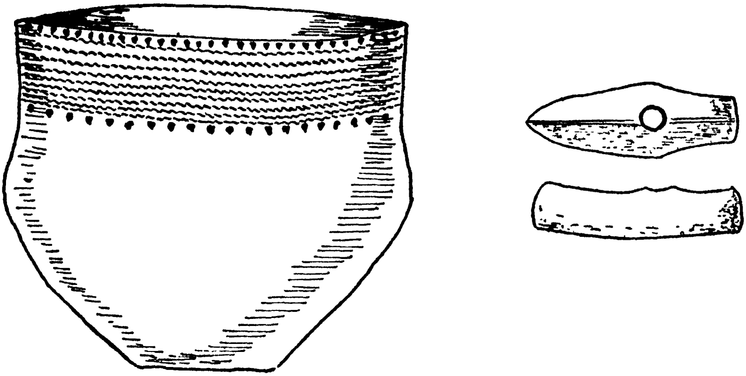


Fig. 9. Finland

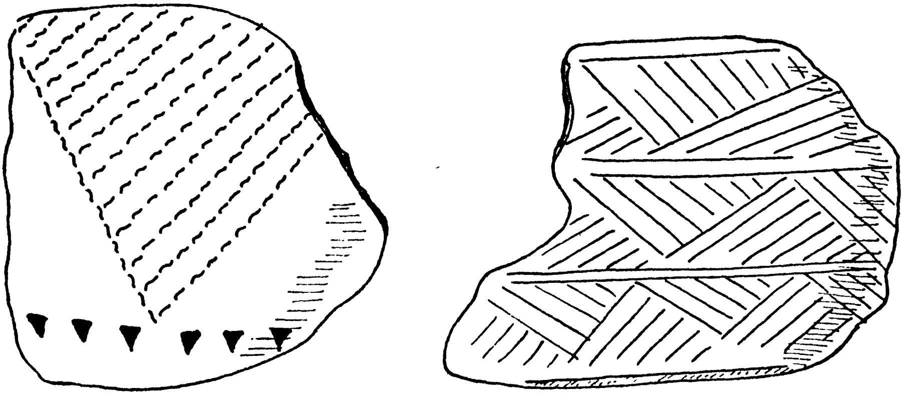


Fig. 10. Northwestern Russia

These astonishingly similar motifs executed in the same technique do not appear on identical vases in various parts of Europe. We do not find a uniform "corded pottery." Pottery is not easy to transport en masse. The pots were not carried by the people, only their skill in pottery making and its decoration. It is therefore natural that the identical decorative motifs are found on different kinds of pottery. "Corded pottery" is a misleading term if it is used to describe a pottery of the immigrant peoples. The northern Caucasian pottery forms differed from the south Russian forms and the latter from those of the rest of the mentioned areas. The central and northern European "corded pottery" developed chiefly from the Funnel Beaker type and under the influences of the Bell Beaker style. In the Balkans the specific cord decoration appears on local high-handled vases and other types.

An analogous enormous spread in the same territories in association with the same intrusive elements is that of the specific form of stone or copper axes called battle-axes. Hence, the name "Battle-axe" or "Warrior Culture" is used instead of or alongside the "Corded Pottery Culture."

During the migration period and immediately after it, cultural groups between Scandinavia and Greece and between France and the Black Sea show a hybrid character culturally. The influx of new people did not destroy the economy of earlier cultures nor did these peoples exterminate local inhabitants. Wherever these allegedly Indo-European speaking peoples spread, they adapted themselves to the environment and traditions of the local cultures with which they came into contact. Most of the assemblages of finds, indicated on Map 2, have a more or less mixed character, which evolved through the fusion of intrusive and local elements. A typical example is the "Globular Amphora" complex of central Europe, in which the Funnel Beaker elements were fused with the Kurgan elements. In many instances the coexistence of local and alien cultural traits are noted. In the Painted Pottery area local elements persisted for a long time, at least for several centuries.

In northeast Europe the food-producers did not succeed in eliminating the culture traits of hunters and fishers at all. In southwestern Finland and northwestern Russia the immigrant food-producing culture did not last. Thus only gradually did the new people attain cultural domination. About the political rule archaeologists cannot say anything.

However, the character of the culture change and the considerable quantity of data are sufficient to prove a tremendous migration. The area north of the Black Sea was not the starting point of migration, but the first territory affected by the intruders from the east. I have arrived at these conclusions after completion of my monograph on the Mesolithic, Neolithic and Chalcolithic cultures of Russia and the Baltic area.[3]

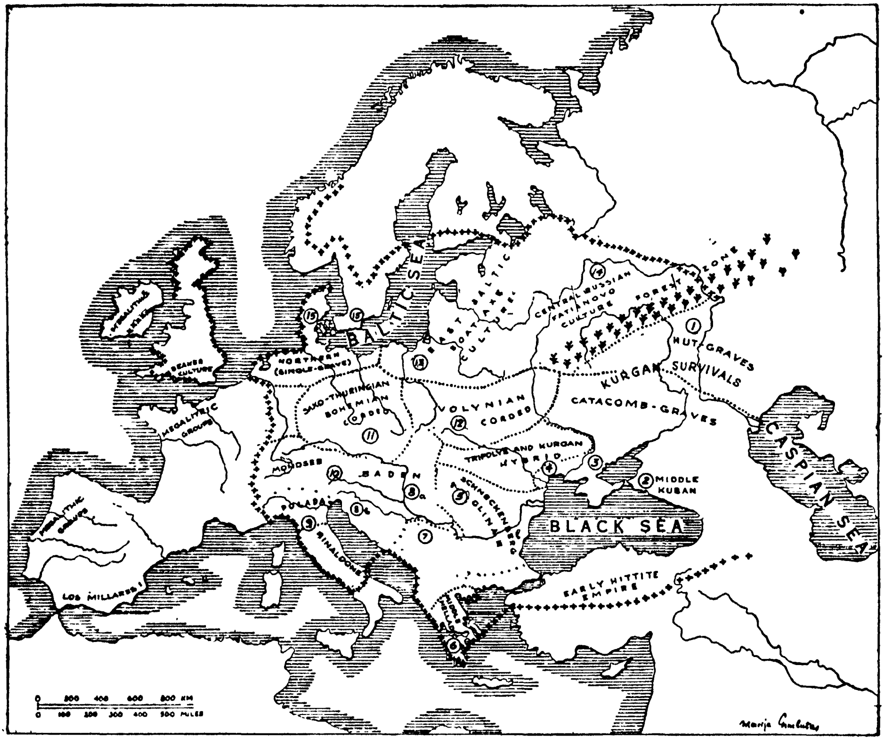
In Asia Minor a destruction period in connection with the invaders from the east (Hittites) is evidenced at 2000–1900 B.C. The appearance of Greeks in Greece may be in connection with further movements westward from western Anatolia[4] as well as with the intrusion from the north.

There is no reason to doubt that in most parts of Europe the evidence of migration was in connection with the appearance of the Indo-Europeans, which successively caused the formation of new Indo-European dialects due to the influence of local languages. Whether or not this wave of the Indo-Europeans represented the first Indo-Europeans everywhere in the Balkans, Anatolia, and the Caucasus remains to be solved by future research.

MAP 3

Map 3 shows cultural units during the postmigration period. The blend of local and intrusive components produced various results. The process of crystallization into new cultural groups started. These do not coincide with the limits of the groups existing during the third millennium B.C. The intrusion of new people did not mean a complete unification of material culture. The affected area was too large and too varied geographically. Also, the preceding cultures were too greatly differentiated, some of them being highly civilized, such as the Early Helladic. Such a variable sub-stratum could not be easily unified. However, the evident trend toward cultural uniformity must be due to the fact that people of the same origin superimposed their rule, skills and techniques over the local population.

Many cultural groups shown on this map are in step with the formalization of the cultural units that continued throughout the later periods, and it is possible to identify the greater part of these with the early historic names of separate Indo-European speaking branches. The numbers on Map 3 indicate


BALTIC SEA
15
15
13
EAST BALTIC BOAT-AXE CULTURE
14
CENTRAL RUSSIAN FATIANOVO CULTURE
FOREST ZONE
1
HUT-GRAVES
KURGAN SURVIVALS
CATACOMB-GRAVES
CASPIAN SEA
MEGALITHIC
BEAKER CULTURE
NORTHERN (SINGLE-GRAVE)
SAXO-THURINGIAN BOHEMIAN CORDED
11
VOLYNIAN CORDED
12
MEGALITHIC GROUPS
MONDSEE
10
BADEN
8a
8b
TRIPOLYE AND KURGAN HYBRID
4
3
2 MIDDLE KUBAN
SCHNECKENBERG-GLINA
5
BLACK SEA
POLADA
9
RINALDONE
7
MIDDLE HELLADIC
6
EARLY HITTITE EMPIRE
MEGALITHIC GROUPS
LOS MILLARES I
0 200 400 600 800 KM
0 100 200 300 400 500 MILES
Marija Gimbutas

Map 3. Cultural Groups during the Postmigration Period. Beginning of the Second Millennium B.C.

1. Chalcolithic Hut-grave complex. Continuum: Timber-grave culture, continuous throughout the Bronze Age.
2. "Middle Kuban" complex, related to the Catacomb-graves. Continuum: north Caucasian Bronze Age.
3. Catacomb-grave complex north of the Black Sea and around the Sea of Azov, related to the northern Caucasus. Continuum: cist-grave complex of the Bronze Age.
4. Kurgan culture, influenced by Tripolye substratum. Usatovo complex.
5. (*a*) Schneckenberg-Glina iii Chalcolithic complex. Continuum: Transylvanian Bronze Age culture. (*b*) Central Bulgarian Corded and Battle-axe complex (Karanovo mound, layer V).
6. Middle Helladic Bronze Age culture. Continuum: Late Helladic Bronze Age (Mycenaean) culture.
7. West Serbian tumuli complex. Bubanj-Hum III complex.
8. Chalcolithic Baden-Pécel complex. Continuum: 8*a*. Hungarian Early Bronze Age (Kisapostag, Wieselburg-Gáta or "Transdanubian Incrusted Pottery" group); 8*b*. Slavonian (Vučedol) complex.
9. Chalcolithic Rinaldone complex. Continuum: Apennine Bronze Age complex. Northern Italy: Terramara.
10. East Alpine group, characterized by Mondsee complex. Continuum: Laibach (Ljublijana) complex.
11. Saxo-Thuringian corded pottery group in central Germany, western Czechoslovakia and western Poland. Continuum: Proto-Unetice and Unetice Early Bronze Age (with influences from the middle Danube area).
12. Volynian corded pottery group in eastern Poland and the western Ukraine. Continuum: Volynian Early Bronze Age.
13. East Baltic boat-axe group, continuous throughout several phases. In the northern part (Estonia, southwestern Finland and northwestern Russia) boat-axe culture did not survive; in the southeastern Baltic area continued throughout the Bronze Age and later.
14. Central Russian Fatianovo complex. Continuum: Early Bronze Age Balanovo complex.
15. North German-Danish-southern Swedish corded pottery group (also called Single-grave culture). Continuum: several phases of corded pottery complex and "Northern Area Bronze and Iron Age".

Limits of the area affected by the cultural change.

the successive cultural groups during the Bronze Age (see the key attached to Map 3).

I hope that many of my colleagues will agree that we possess a certain amount of information about the Bronze and Iron Age cultures in some parts of Europe, which allows one to assume cultural continuity to the beginning of the historic era. Some parts are still backward in prehistoric research. We find least confusion in the marginal zones (in the geographic sense). There is not much doubt that the inhabitants of Middle Helladic Greece were direct ancestors of historic Greeks, since there was only the well known Mycenaean period in between to fill the gap between the Middle Helladic period and the fifteenth century B.C. when the earliest documents in Greek appear. On the Baltic Sea, the archaeologically well explored southeastern Baltic area exemplifies another extreme: here the Neolithic boat-axe culture was separated from the earliest written sources by some seventeen hundred years. However, the undisturbed continuum of the prehistoric cultures throughout the Bronze and Iron Ages permits us to tie it in with the Baltic-speaking branch. Analogous evidence is available for the identification of the northern German, Danish and southern Scandinavian Bronze and Iron Age culture with the Germanic speaking group. The eastern central European group, spread over the eastern part of Poland and the northern part of the western Ukraine, called on this map "Volynian," through ever increasing knowledge of its successive developments throughout the Late Bronze and Early Iron Age periods, reveals more and more data for the insight that it may have belonged to the Slavonic speaking branch. The oldest Slavonic names are known from eastern Poland, Volynia, Podolia and the middle Dnieper area. Groups north of the Black Sea almost certainly belonged to the Iranians. The continuity of the Hut- and Timber-grave complex in southern Russia makes the Russian scholars believe that its people were early Scythians. It is quite possible that the cultural group along the northern coasts of the Black Sea and in the northern Caucasus (the Catacomb-grave culture, Borodino-Faskau assemblages, the Koban culture) was made up of the Kimmerians, who were driven out from there by the Scythians in the eighth century B.C. or earlier.[4] In the Balkans, to the west of the Black Sea and to the south of the Slavonic group, in ancient times lived the Thraco-Phrygians, but their Bronze and Iron Age culture is still not sufficiently known. In central Europe the problem of cultural succession is in a more confused state due to Urnfield, Kimmerian, Scythian and Celtic migrations, intrusions, or influences. The early Celts are identified with La Tène and Hallstatt culture of the Iron Age and the Tumulus group of the Bronze Age in the Alpine area and southern Germany.[5]

Illyrian speaking people must have lived in the south and southeast of the central European group and to the east of the Celts. For this hypothesis there are the Illyrian names east of the Adriatic. Here, in northern Yugoslavia, Hungary, and Lower Austria, one of the strongest and most creative Bronze Age cultures of central Europe was distributed.

In this paper I had neither the purpose of following the prehistoric development of every cultural group into the historic period, nor of solving the problem of the Indo-European origins. However, I strongly believe that prehistoric research promises in the very near future to reveal many important facts about these problems. Already at the present time it offers much more than it did some twenty years ago.

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Cambridge, Massachusetts.

Notes

1. The author is indebted to Dr. Robert W. Ehrich and Mr. Vladimir Markotic for their kindness in furnishing information on the chronology and distribution of the cultural groups in the Balkans.

2. F. Hančar, 1956. Das Pferd in prähistorischer und früher historischer Zeit, Pls. II and III.

3. M. Gimbutas, 1956. Prehistory of Eastern Europe, Vol. I. *American School of Prehistoric Research, Peabody Museum of Harvard University, Bull.* 20.

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FIG. 2. Shpakovka kurgan near Izium on the Donets: V. Gorodtsov, 1905, Materialy arkheologicheskikh issledovanii na r. Dontsa, Iziumskogo uyezda, Kharkovskoi gubernii. *Trudy XII Arkh. Syezda*, I, Pl. III.

FIG. 3. Gheja and Căpud sites, Transylvania: H. Schroller, 1933, Die Stein- und Kupferzeit Siebenbürgens. *Vorgeschichtliche Forschungen*, 8, Pls. 26 and 27.

FIG. 4. Eutresis and Hagia Marina sites: H. Goldman, 1931, Eutresis, p. 122; H. Frankfort, 1927, *Studies in Early Pottery*, 2 : 44, Pl. IV, No. 6.

FIG. 5. Bylany site: J. L. Pić, 1899, *Čechy předhistorické*, Pl. II.

FIG. 6. "Grosser Stein" kurgan, grave III: H. Kretzsch, 1955, Drei schnurkeramische Grabanlagen auf dem "Grossen Steine" bei Seifartsdorf, Landkreis Eisenberg, Thüringen. *Alt-Thüringen*, I, p. 199.

FIG. 7. Succase habitation site near Elbing on the Lower Vistula: B. Ehrlich, 1934, Ein jungsteinzeitliches Dorf der Schnurkeramiker in Succase, Kr. Elbing. *Altschlesien*, V.

FIG. 8. A-group corded beakers from Jutland: P. V. Glob, 1945, Studier over den Jyske Enkeltgravskultur, p. 65.

FIG. 9. Uusi-Jara site: A. Äyräpää, 1955, Den yngre stenålderns kronologi i Finland och Sverige. *Finskt Museum*.

FIG. 10. Modlona site: A. IA. Briusov, 1951, Svainoe poselenie na r. Modlone i drugie stoianki v Charozerskom raione Vologodskoi oblasti. *Materialy i Issledovaniia po Arkheologii SSSR*, 20 : 37.

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ROCKER-STAMPED POTTERY IN THE OLD AND NEW WORLD[1]

Robert E. Greengo

Among indications suggesting diffusion between Old and New World cultures is the circum-boreal distribution of Neolithic pottery.[2] Studies of phenomena of this nature are bound to consider the theoretical implications as to whether a given trait may have been independently invented a number of times, or whether it was more likely diffused to its separate localities. If a particular habit pattern can be demonstrated to occur consistently in time and space, so as to fit a diffusion hypothesis, that hypothesis will thereby be strengthened. Rocker-stamping of Neolithic pottery may be a trait of this order.

This technique is executed with the smooth or notched edge of a thin, flat, or slightly curved tool by walking it over the incompletely dry clay surface so as to produce either a plain or dentate zig-zag design.[3]

To begin with the Hopewell Phase of southern Ohio,[4] dating ca. 300 B.C. to A.D. 100, plain and dentate rocker-stamping occurs either as background roughening or in designs featuring contrast between rough and smooth areas, often marked by curvilinear, incised lines. While rocker-stamping is prominent among various techniques on Hopewell Ware, this ware occurs in minor quantity compared to the cord-marked, conoidal-based "utility ware." The cord-marked ware is widely distributed in the Boreal Zones of North America and Eurasia, and constitutes a prime element in inter-continental diffusion hypotheses, but will not be of direct concern here. In Ohio Hopewell, the fine, decorated ware was apparently made as burial furniture, though some is found in refuse deposits.

Between ca. 500 B.C. and A.D. 500 there was a mound burial complex related to Ohio Hopewell in southern and western Illinois.[5] Here again both plain and dentate rocker impressions occur in minor quantities, though with some indications that it was applied also to utility ware. Related Hopewellian phases in Wisconsin and Michigan are similar in this regard.[6] Another complex related to Illinois Hopewellian has been found near Kansas City, where rocker-stamping seems to have had a different emphasis in the culture.[7] Here it appears on 30% of the utility pottery from occupation debris, mostly in the plain variety, though a few vessels bear zoned curvilinear designs.

Possibly the earliest rocker-stamping in the New World is found in modest quantities in the Point Peninsula I Focus of upper New York state. In the form of plain and dentate rocker marking in horizontal rows on most of the vessel wall, the technique becomes characteristic in Point Peninsula II.[8] Ritchie has C-14 dates of ca. 2500 B.C. for Point Peninsula I and 1000 B.C. for Point Peninsula II, and believes rocker-stamping came in some time between these two dates, probably nearer the later one.[9] He further suggests a dual wave of pottery diffusion from Asia, corresponding to the above C-14 dates, and postulates that rocker-stamping, brought by the second wave, was coincident with an Early Woodland burial cult, in turn related to the Adena-Hopewell mortuary complex.[10]

	New York	Central U.S.	Lower Mississippi V.	Meso-America	Peru
-1000			COLES CREEK		
				"FINE GREY"	
			TROYVILLE		
-500			ISSAQUENA		
			MARKSVILLE		
A.D. -B.C.		HOPEWELL			
			TCHEFUNCTE		
-500				TLATILCO	CHAVIN
-1000	POINT PENINSULA II				
-1500	(ROCKER STAMPING)				
-2000					
-2500	POINT PENINSULA I				
-3000					

Fig. 1. Rocker-Stamping Occurrences in the New World

On the East Coast, rocker-stamping is found from Long Island to Nova Scotia, in contexts that suggest affinities with the Point Peninsula foci.[11]

Turning now to the mouth of the Mississippi river, we find an archaeological phase known as Tchefuncte.[12] Like Point Peninsula it carries over features from the preceding Archaic Stage. Diagnostic of Tchefuncte is a pottery type bearing notched and plain rocker impressions, and while probable connections would seem to lie to the Northeast, its affiliations and dating are by no means well understood.

Somewhat farther up the valley in central Louisiana is a phase known as Marksville,[13] that again has zoned, dentate rocker-stamped pottery placed with human burials in earth mounds. Although there are no trustworthy C-14 dates for this phase, dates for the succeeding phase and typological considerations indicate a period around the beginning of the Christian era that would allow for what may have been direct contact with people of Ohio Hopewell.

Next to appear in the lower Mississippi valley, is the Issaquena Phase,[14] which developed out of the Hopewell-Marksville tradition, but lacks many specifically Hopewellian traits. Its most important ceramic technique is zoned, dentate rocker-stamping on utility ware. Associated with this, in minor quantities, is some zoned, plain rocker-stamping, which completely replaces the dentate variety in the succeeding Troyville Phase. Appearing also in Troyville, and characteristic of the next phase, Coles Creek,[15] are three additional variants of zoned, plain rocker impressions. An internally consistent series of C-14 dates places Issaquena ca. A.D. 400 to 600, and Troyville-Coles Creek ca. A.D. 600 to 1100.

To the east on the Florida Gulf coast the Santa-Rosa-Swift Creek Phase is related to Issaquena, particularly in its rocker-stamped ceramics.[16]

An attenuated Illinois Hopewellian complex with dentate rocker-stamping is found in Oklahoma,[17] and in a very different cultural milieu in the Belcher Focus in central Texas, there is a late survival in the Marksville tradition, called Cowhide Stamped, dating ca. A.D. 1200 to 1500.[18]

In Meso-America let us first consider the important site of Tlatilco in the Valley of Mexico.[19] Here, in conjunction with an elaborate mortuary complex we find rocker-stamping allied with a variety of pottery wares rich in plastic manipulation. Though not abundant, zoned plain rocker impressions embellish animal and human effigy vessels, as well as pots of simpler form. An affinity is apparent between this site and those of the Olmec phase of southern Veracruz and Tabasco. There are also striking similarities at Tlatilco to the Chavin culture of northern Peru. At El Trapiche, Veracruz, the lowest levels yielded pottery nearly identical to that at Tlatilco, including plain rocker-stamping. Also, the only sherd of dentate rocker-stamping published from the Meso-American Formative Stage, was found at this site.[20] La Venta, the type site of the Olmec Phase, has a minor proportion of zoned plain rocker-stamping on a coarse fabric.[21] In a related component at the site of Chalcatzingo, Morelos, there is a similar occurrence but involving two wares.[22]

In the Maya area, rocker-stamping has been noted from the early levels at the Barton Raimey site, British Honduras,[23] while in neighboring (Spanish) Honduras zoned plain rocker impressions are a ceramic feature in the sites of Santa Rita and Yarumela of the Ulua-Bichrome phase.[24]

In spite of the fact that there are no C-14 dates specifically referable to these particular phases—we have some notion of the temporal order through stratigraphy and cross-dating. Dates of ca. 1450 B.C. for Zacatenco I in the Early to

Middle Formative, and ca. 470 B.C. for Ticoman in the late Formative,[25] conform well with a range of ca. 2000 B.C. to ca. 300 B.C. for the Formative of highland Guatemala. Tlatilco and Olmec are thought to be Late Formative and to equate with Miraflores of Guatemala, which dates ca. 500 B.C.[26] If these dates are of the right order of magnitude a measure of contemporaneity between the Meso-American Formative and Hopewell to the North may be assumed.

There are late anomalous finds of both plain and dentate rocker-stamping in the Maya area, one from Tzimin Kax, British Honduras, and the other from Tecolpan, Tabasco. They definitely belong in the Late Classic, which dates from ca. A.D. 400 to 700 (or some 260 years later, by another correlation).[27]

Examples of rocker-stamping in Peru are found in sites associated with the Chavin horizon, mostly on the north coast. Two varieties of dentate and one of plain rocker impressions appear on sherds set off by incised lines and zoned panels at the sites of Ancon and Supe,[28] where this decoration is one of the most frequent of the textured treatments. It occurs in at least seven other sites including Chavin de Huantar and Cupisnique.[29] This horizon is on the Formative Stage of development in Peruvian prehistory, which in a general way compares to the Meso-American Formative. C-14 dating for Cupisnique falls around 700 to 800 B.C.[30] not substantially different from that indicated for Tlatilco.

Recently other Formative components have been revealed in Ecuador.[31] These not only have rocker-stamping, but a number of other fairly specific traits that would seem to link them with Meso-America.

In summary of the New World data on rocker-stamping, we find it disseminated through nearly contiguous areas on both continents. The contextual relationships in most cases are comparable, and radiocarbon dating points to a diffusion from the northern Woodlands, where it appears at ca. 1500 B.C., to Meso-America. From there Willey[32] offers a cogent argument for the diffusion of Formative Stage culture to Peru. Thus an hypothesis for a north to south dispersion of rocker-stamping in the New World would seem to be the most economical one to offer at this time.

Rocker-stamping in the Old World appears to occur first in early Neolithic contexts in the Near East. The best stratigraphic evidence is from the Amouq plain in Syro-Cilicia, where there is a modest frequency of the technique in phases A and B. Amouq A has been given an estimated date by Braidwood of ca. 4500 B.C.[33] Plain rocker-stamping was also found in the lowest levels of Mersin,[34] and of Sakje Geuzi[35] in the same general area. Dentate rocker impressions occur on similar ware at Wadi Hamman,[36] and in the lowest levels at Chagar Bazar in north Syria.[37] Finds in a later context have been noted at Tell Billa, and Tepe Gawra, on an intrusive gray-green ware in strata dating ca. 3000 B.C. Other occurrences in southwest Asia include an isolated sherd, probably from the intermediate period at Susa dating about 3000 B.C., and some undocumented surface finds from Ibn Derre, Wadi Jidr, and Nippur all probably dating after 500 B.C.[38]

In the Khartoum Mesolithic, in the Anglo-Egyptian Sudan, dentate rocker-stamping impressed with worked catfish spines is associated with a Dotted Wavy Line Ware.[39] A Micaceous Ware, apparently belonging to this period also bore dentate rocker impressions, as well as some plain rocker-stamping, probably done with a shell. In the succeeding Khartoum Neolithic, pottery

	Near East	Upper Nile	Western Mediterranean	Ukraine Kazakstan	Urals-Ob	East Asia
					GORDDSHCHE	
-1000				KARASUK		
-1500						
						"RED POTTERY"
-2000						
			CARDIAL			SHABARAKH USU
-2500			MOLFETTA-MATERA, I		SHIGIR	
					(MOLOTOV AREA)	
				KEL'TEMINAR		
-3000	TELL BILLA-TEPE GAWRA					
-3500				NORTH PONTIC		
		KHARTOUM NEOLITHIC				
-4000						
		KHARTOUM NEOLITHIC				
-4500						
	AMOUQ A					

Fig. 2. Early Occurrences of Rocker-Stamping in the Old World

decoration evolved into an Impressed Ware, of which some twelve varieties have been isolated, all of them rocker-stamped.[40] The usual implement employed on this slightly polished, (or burnished) fabric is made from a shell, the edge notched in a variety of ways, or left smooth. Arkell suggests a date of ca. 3900 B.C. for the Khartoum Neolithic, and believes that it may have come from the region of the Nigerian Sahara where there is an early incised-punctated ceramic complex including some plain rocker-stamping. Certain comparisons have been made in the opposite direction, however, and the question is still open.[41]

Across North Africa some plain and dentate rocker-stamping from the cave of d'El Arouia on the south side of the Atlas Mountains in Algeria may be related to that in Nigeria.[42]

Rocker-stamping is fairly abundant on some of the Cardial Ware from Tangier,[43] with a mean estimated date of about 2300 B.C.[44] This ware is found on the Mediterranean and Atlantic coasts of North Africa, quite concentrated in the caves of southeastern Spain and diffused to much of coastal and insular Western Europe,[45] but is rocker impressed apparently only in Africa.

The earliest Neolithic in southeastern Italy is identified with the fortified village sites of Molfetta and Matera,[46] where great quantities of simply incised, punctated and rocker-impressed pottery are found. Plain and dentate rocker-stamping is said to be varied and highly characteristic of the coarse ware of Period I. This phase appears to have little specific affinity with the North Italian site of Arene Candide, nor with the Afro-Hispanic Cardial Ware tradition, but rather looks to the Balkan and Danubian areas. On Sicily the famous Stentinello culture seems to have the closest relationships with Molfetta-Matera II, in which the characteristic ware is scratched. There is rocker-stamping, but with a narrow chisel-like instrument, largely replacing shell impressing.[47] A consensus of estimated dating would seem to place Molfetta-Matera I and II at about 2500 B.C.[48]

In Bohemia, characteristic of Danubian Ib, there is found a stroked Ware with simple forms and plain and dentate rocker-stamping.[49] Designs consist of single or multiple rows of impressions spaced to give a zoning effect. This appears to be an isolated occurrence of rocker-stamping in the Danube Valley, which could be related to that in southeastern Italy, where there are other Danubian connections.

Near Dnepropetrovsk in the southern Ukraine, the first Neolithic is signalled by a coarse, thick pottery featuring simple incising and punctating, along with plain rocker impressions in single rows. This material belongs to what Gimbutas calls the North Pontic culture which she dates in the fourth and third millennium B.C.[50]

Dentate rocker impressions are noted for the Shigir culture in the middle Urals, in the third millennium B.C.[51] Just west of the Central Urals, in the vicinity of Molotov, rocked comb markings appear on conoidal-based pots, of the sub-Neolithic or Early Bronze Age,[52] before the mid-third millennium B.C. To the east of the Central Urals, not far from Omsk, pottery of the Gorodishche culture has fairly abundant plain and dentate rocker-stamping, usually arranged in horizontal bands on pointed based vessels.[53] This culture is dated at about 500 B.C. At the mouth of the Ob river in sites near Salekhard, there is some dentate rocker-stamping,[54] dating from around A.D. 500 to 1000.

Farther east in central Asia at Kel-teminar dentate rocker impressions are found in a culture dated ca. 2800 B.C.[55] Later, around 1000 B.C., plain rocker markings have been noted at Karasuk, between Omsk and Novosibirsk.[56] While there is somewhat of a gap in the known distribution of rocker-stamping in central Siberia, we do have it reported for Outer Mongolia, at the site of Shabarakh Usu, with an estimated date of ca. 2000 B.C.[57]

The picture definitely comes into focus again in Inner Mongolia at Hung-shan-hou, in the province of Jehol, Manchuria.[58] Here, on fine and coarse fabrics, plain over-all rocker-stamping is the major ornamentation. Dentate rocker impressions are present in significant quantity as well. Other treatments include fingernail punctation, comb striation, together with some cord marking. In the same general region, at the sites of West Barin and West Obo, plain rocker markings are found, apparently to the complete exclusion of the dentate variant—again in an incised-punctated context.[59] The lowest levels of the cave deposit of Sha Kou T'un in the Mukden region of southern Manchuria, also yielded plain rocker-stamping on a coarse ware. This was associated with mat impressed, and string impressed treatments. To the south at the site of Ho Yin, in Honan, there was a minor quantity of dentate rocker impressions.[60] All of these sites are apparently manifestations of the late Neolithic Red Pottery Culture (Yang Shao), dating ca. 2000 B.C.

Japan too qualifies in the rocker-stamping tradition where the plain variety is known from the Kokura site in west central Japan, assigned to the Moroiso Culture of the Early Jomon Period. Groot says that shell impressions, of which this is a variant example, seems to have died out in subsequent phases.[61] There is a C-14 date from the Kamo Site, Chiba Prefecture, of the Early Moroiso phase of Early Jomon Period of 3150 ± 400 B.C.[62] This appears to be of acceptable order of magnitude for the Period.

Two other concentrations of rocker-stamping are known. One in the Haut Laos region of Indo-China, where plain and dentate variants occur associated in a pre-Han context with cord-marked pottery, estimated to date ca. 450 to 150 B.C.[63] These traits, and a number of others point to diffusion from central Asia. Undoubtedly tied in with these southeastern Asiatic diffusional pulsations are the recent finds on New Caledonia and Vuatom in Melanesia. Here, rocker-stamping occurs in a cultural complex dated ca. 660 B.C., by C-14.[64]

In the Asian distribution of rocker-stamping we see that it first appeared in the Neolithic of the southwest, and is found in similar cultural contexts with an age-distance gradiant consistent with a diffusion hypothesis. The Khartoum, Cardial Ware, southeastern Italian and Bohemian occurrences only roughly fit the hypothesis at present, but since there is little doubt that these are related in the larger Neolithic picture, more specific connections along the lines indicated here cannot be ruled out.

In connecting Asia and North America, the major difficulty lies in the New World, where there is a considerable gap between the known distribution of rocker-stamping and the assumed northwestern entryway. Significantly the area in question is one of the least known archaeologically in the New World.

In conclusion it is submitted that the argument for the diffusion of Old World Neolithic Culture into the New World is significantly strengthened by the study of the distribution of the technique of rocker-stamping on pottery.

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Notes

1. Many thanks are herewith expressed to all who contributed generously toward the documentation of this paper, particularly to D. F. Brown, M. Gimbutas, H. O'N. Hencken, B. Howe, H. L. Movius, P. Phillips, L. Ward, and G. R. Willey of the Peabody Museum, Harvard University; to R. J. Braidwood, of the Oriental Institute of Chicago; and to R. H. Dyson of the University Museum, Philadelphia.

2. Among those writing on the subject are: Richthofen, 1932; McKern, 1937; Spaulding, 1946; Porter, 1953; Tolstoy, 1953; Ritchie, 1955; Willey, 1955.

3. For a good discussion of the technique see Arkell, 1953 : 70.

4. The concepts "Phase" and "Stage" as used here were defined by Phillips and Willey, 1953. All important sources on Hopewell are listed by Wedel, 1943. Dates are the writer's interpretation of present C-14 dating, or estimated dating, with an attempt to reach a consensus of areal authorities. Some dates suggest contradictions to the main line of development plotted in this paper. E.g., the Hopewell-Tlatilco-Chavin relationships are by no means clear. These do not, to the writer's view, confute the present thesis so much as they focus on problems well worth attention.

5. Deuel, 1952. Griffin, 1952.

6. "Hopewell" will refer to Ohio Hopewell; "Hopewellian" to closely related phases elsewhere. See McKern, 1931, and Quimby, 1941.

7. Wedel, 1943.

8. Ritchie and MacNeish, 1949. This type, while strikingly similar to vessels in Kansas City Hopewellian, occurs in an entirely different and much earlier milieu.

9. Ritchie, n.d.

10. Ritchie, 1955.

11. Smith, 1950; Willoughby, 1935; Smith and Wintemberg, 1929.

12. Ford and Quimby, 1945.

13. Setzler, 1933; Ford and Willey, 1940.

14. This is a new phase, based upon recent excavations by Dr. Philip Phillips and the writer in the vicinity of Vicksburg, Miss. See Greengo, n.d.

15. Ford, 1951; Phillips, Ford, and Griffin, 1951.

16. Willey, 1949.

17. Bell and Baerreis, 1951.

18. Suhm and Krieger, 1954.

19. Porter, 1953; Covarrubias, 1950.

20. Porter, 1953 : 48; Garcia Payon, 1951 : 91.

21. Drucker, 1952; Drucker, 1947.

22. Pina Chan, 1955.

23. Willey, n.d.

24. Strong, Kidder, and Paul, 1938; Canby, 1951.

25. Libby, 1955, Nos. C-196, C-200.

26. Kidder, n.d.; Shook, n.d. *b*; Willey, 1956.

27. Thompson, 1931, Pl. 43; Shook, n.d. *a*.

28. Willey and Corbett, 1954 : 40–65.

29. Willey, 1951.

30. Bird, 1951 : 37–49.

31. Evans, n.d.

32. Willey, 1955 : 35.

33. Braidwood and Braidwood, 1953 : 290–291; Braidwood, n.d.

34. Garstang, 1937 : 63, Pl. 3 : 7; Garstang and Goldman, 1947 : 372.

35. Taylor, *et al.*, 1950 : 86, Fig. 12 : 10.

36. O'Brien, 1933, Pl. O : 8.

37. Mallowan, 1936 : 53, Pl. 3 : 12.

38. Speiser, 1933 : 253, Pl. 71 top center. Dyson, n.d. *a*.

39. Arkell, 1949; 1953.

40. Arkell, 1953 : 66–77.

41. Childe, 1953 : 48–49; Cole, 1954 : 211–214.

42. Vaufrey, 1939, Fig. 42 : 9, 14, 44 : 4, 9.

43. Excavated by Coon, Nahon, and Doolittle at Cape Achakar, now in Peabody Museum, Harvard University, and see Martinez Santa-Olalla, 1949.

44. Childe, 1950; Hawkes, 1940; Vaufrey, 1939.

45. Aparisi, 1942 : 108 ff.

46. Stevenson, 1947 : 85–86; Brown, n.d.

47. Brown, n.d.

48. Childe, 1950; Hawkes, 1940.

49. Stocky, 1926; 1924.

50. Danilenko, 1950. Dating for Russia and China is here based upon: Gimbutas, n.d.; Gimbutas, 1954; and Tolstoy, 1953.

51. Rauschenbach, 1951 : 58, Fig. 18 : 7.

52. Bader, 1951, Fig. 9 : 2; 11; 13 : 3.

53. Chernetzov, 1953, Pl. 9, 10, 14, 17, 20.

54. Moshinskaya, 1953, Tabl. V : 15.

55. Formozov, 1951 : 7.

56. Chernetzov, 1947. Jettmar, 1950, mentions coglike and zig-zag impressions that may indicate rocker-stamping, but none is illustrated.

57. Porter, 1953 : 85.

58. Hamada and Mizuno, 1938 : 5, Fig. 37, Pl. 36–41, 43—all from Dwelling Site II.

59. Torii and Torii, 1914 : 58, Pl. 6–8.

60. Andersson, 1923 : 22, Pl. 2, 3a, b, 8a–c.

61. Andersson, 1947, Pl. 137 : 6–7.

62. Groot, 1951, Pl. 20, 3rd row, center; 43. There is more, however, in the U.S. National Museum (Willey, n.d.).

63. Movius, n.d.

64. Colani, 1935, Pl. 86–88, 90–92; Dyson, n.d. *b*.

65. Gifford and Shutler, 1956; Lenourmand, 1948; Avias, 1950.

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The following abbreviations are used:

AAn	*American Antiquity*. Menasha, Salt Lake City.
AMNH-AP	*American Museum of Natural History, Anthropological Papers*. New York.
KS	*Kratkiye Soobshcheniya Instituta Istorii Materyal'noy Kul'tury*. Moscow.
MIASSSR	*Materialy i Issledovaniya po Arkheologii* SSSR. Moscow.
SIMC	*Smithsonian Miscellaneous Collections*. Washington.
n.d.	No date.

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NORTH MEXICO AND THE CORRELATION OF MESOAMERICAN AND SOUTHWESTERN CULTURAL SEQUENCES

J. Charles Kelley

The Southern Illinois University has completed three summer seasons of archaeological field work in the State of Durango, Mexico, in 1952, 1954, and 1956. This field work was financed by the Southern Illinois University and by grants from the Wenner Gren Foundation for Anthropological Research, Inc. (all three seasons), the American Philosophical Society (1952), and the University of Chicago (1956).[1]

Added to earlier work by Gamio,[2] Noguera,[3] Mason,[4] and Brand,[5] and the current work of Lister and Howard,[6] the results of these studies provide significant new data for the correlation of Mesoamerican and Southwestern cultural sequences. The material presented in this paper is based primarily on the studies of the writer and Mr. Howard Winters but also derives from the researches of field associates such as Ramón Piña Chan, Agustín Delgado, Wm. J. Shackelford, Carroll L. Riley and a number of able student assistants.

THE CHALCHIHUITES SEQUENCE

The first excavated site of the Chalchihuites Culture is that of Alta Vista located in the Suchil Valley on the Zacatecas-Durango boundary near the town of Chalchihuites.[7] This ruin consists of a number of mounds located on a hilltop. Excavation produced a masonry-walled hall very nearly filled with columns of stone and adobe brick, an associated platform with stairway, and other masonry rooms or courts. In addition to polished black and red wares, the principal pottery types are Suchil Engraved—tripod vessels in black or brown ware with exterior geometric designs engraved after firing (and later filled with red pigment) in a band around the vessel just below the rim, and Suchil Red-on-Brown—simple bowls decorated with geometric designs in red on the brown-surfaced exterior, interior, or both. Conventionalized animals in bands are included in the engraved designs of Suchil Engraved and occasionally such animals occur in the interior bottoms of Suchil Red-on-Brown bowls. This manifestation is now known as the Alta Vista Phase of the Suchil Branch of the Chalchihuites Culture.

About 65 miles northwest of Alta Vista is the modern city of Durango in the valley of Guadiana, in the Rio Tunal (Mesquital–San Pedro) drainage. Several sites of the Chalchihuites Culture occur in and near this valley. Especially important is the Schroeder Site located on the low hills of the Sierra de Ayala south and east of the Rio Tunal and some five miles south of the city of Durango. Still farther north at Guatimapé, at Hervideros and even at Zape some 125 miles north of the city of Durango, Chalchihuites sites are known but have not been thoroughly investigated.[8]

Two seasons of excavation have now been completed at the Schroeder Site, and a sequence of three cultural phases has been identified there. The Schroeder Site consists of a number of masonry structures scattered over two hills and an intervening saddle. On the eastern hilltop there is a terraced pyramid, and artificial terraces cover the northern slopes of the western hill.

The pyramid on the hilltop and the associated ceremonial structures at its base appear to have been constructed and most widely used during the earlier or Ayala Phase of occupation. The Ayala Phase masonry consists largely of rough stones, some shaped, laid in horizontal courses forming raised platforms and rooms surrounding courts. The principal decorated pottery types are Amaro Red-on-Cream, whose simple bowl forms and interior decoration appear to have been derived from Suchil Red-on-Brown, and Mercado Red-on-Cream in tripod forms with exterior decoration seemingly evolved from Suchil Engraved. Occasional sherds of Suchil Engraved and Suchil Red-on-Brown occur in Ayala Phase deposits and there is also what appears to be a local copy of Suchil Engraved, called El Conejo Engraved. On the evidence, the Ayala Phase appears to be a colony or local development of the Alta Vista Phase.

Notably, the principal Ayala Phase pottery type, Mercado Red-on-Cream, is characteristically decorated by encircling exterior bands of geometric figures or conventionalized birds or horned serpents (surrounded by fine dots), or both combined and rarely other life forms. These appear to be derived from similar life forms on Suchil Engraved vessels, and the animal forms are often present as quadrant or sector fillers for the interior bottoms of Amaro Red-on-Cream bowls. In the subsequent Las Joyas Phase, Mercado Red-on-Cream develops into Neveria Red-on-Brown (a transitional ware, Refugio Red-on-Brown is now known), and Amaro Red-on-Cream apparently disappears. The conventionalized animals in bands around the exterior of Neveria Red-on-Brown bowls are quadrupeds or, rarely, anthropomorphic forms rather crudely drawn; the birds and serpents of Mercado have disappeared together with the surrounding dots which are large and few if shown at all. Basket-type handles, which occurred occasionally in Mercado, appear to be characteristic of Neveria. These handles—and the vessel walls as well—are often decorated profusely with small elements such as crosses, swastikas, bulls-eyes, etc.

During the Las Joyas Phase appeared a new masonry type characterized by the use of large vertical slabs in wall construction. Some new construction in this style was made in the ceremonial area at the foot of the pyramid but there was little use of either the pyramid or the adjacent ceremonial area during the Las Joyas Phase, and the evidence is that this general ceremonial area was abandoned fairly early in the phase. Plumed serpents abound in the life-form depictions on pottery from the pyramid area; but following the abandonment of this area the plumed serpent design also disappears, suggesting possible ceremonial changes.

During the subsequent and apparently final Chalchihuites occupation of the Schroeder Site, the Rio Tunal Phase, several new cultural elements appear. Otinapa Red-on-White, the dominant pottery type, appears to have been derived in part from Neveria Red-on-Brown but it contains new and intrusive elements. Ollas and simple bowls of this ware appear almost identical to Three Circle Red-on-White vessels from the Mogollon Culture of the Southwest. Other new wares include Madero Fluted, Morcilla Molcajete, and Nayar White-on-Red (late in the phase). The latter ware is almost identical with late white-on-red wares from Nayarit and closely resembles the white-on-red wares that appear in the Southwest at about the same time. Other new elements in the Rio Tunal Phase are copper belts, chains, awls, needles, discs, spindle whorls in quantity (several types) and pottery tobacco pipes. Copper artifacts, pottery *molcajetes*, spindle whorls in quantity, and tobacco pipes form an extremely significant complex and their appearance in the Rio Tunal Phase is of considerable aid in dating the culture.

The Rio Tunal Phase occupation at Schroeder was limited in space, as far as now known, to the large agglomeration of masonry courts, platforms, stairs, and rooms known as Structure I, and to the adjacent burial terrace area, although it occurs in other sites in the region. With this phase, the Chalchihuites occupation of the Schroeder Site (known as the Guadiana Branch) came to an end.

CHALCHIHUITES–COASTAL SINALOA RELATIONSHIPS

The work of Sauer and Brand,[9] Ekholm,[10] Isabel Kelly,[11] and Gifford[12] has revealed a sequence of cultures for coastal Sinaloa, west of Durango and Nayarit which is of utility in dating the Guadiana sequence. The earliest-known Sinaloa cultures are the Early and Middle Chametla phases, thought to be of late Classic age in Mesoamerican terms.[13] There are sherds doubtfully identified as Early and Middle Chametla Polychromes from the Schroeder Site, apparently in an Ayala Phase association, and several small engraved spindle whorls of Middle Chametla type were found in Las Joyas Phase association. Also in Las Joyas Phase association were found many sherds of Chametla Red-Rim. This ware was thought by Kelly to belong with the Aztatlan complex at Chametla, but her sherd distribution tables show that it appeared earlier than the other Aztatlan wares and in fact may have overlapped the temporal distribution of the Middle Chametla phase.[14] At Schroeder its associations are definitely and consistently Las Joyas Phase, in which it appears in quantity without Aztatlan types. In the subsequent Rio Tunal Phase, Aztatlan ware occurs and with it such types as Sinaloa Polychrome and various incised and engraved wares of Aztatlan origin. Aztatlan-type spindle whorls and tobacco pipes also occur in Rio Tunal association and the copper artifacts may have had a similar origin.

In summary, the Rio Tunal Phase can be correlated with the Aztatlan Phase of Sinaloa, the Las Joyas Phase with a "Late Chametla" phase preceding Aztatlan and characterized by Chametla Red-Rim pottery—and possibly with terminal Middle Chametla as well. Perhaps also the Ayala Phase may equate with Middle Chametla and the Alta Vista Phase with Early Chametla, although little specific evidence exists for such a correlation. Although Ekholm originally dated the Aztatlan Phase as beginning perhaps as late as A.D. 1300 his most recent public statement estimates Aztatlan beginnings at shortly after A.D. 900.[15]

CHALCHIHUITES–MESOAMERICAN RELATIONSHIPS

The Chalchihuites Culture looks like a peripheral Mesoamerican manifestation. Among the principal traits which it shares with Mesoamerica are: *architectural features* such as elaborate masonry structures, a terraced pyramid, hall of columns (absent at Schroeder), stairways, house platforms, courts with drains, use of plinth or *cuerpo*, banquettes, circular structures on platforms, ball-courts (?), periodic resurfacing of platforms, ceremonial approaches, and the concept of ceremonial centers versus domiciliary areas; *ceramic features* such as elaborate painted and engraved pottery, red-on-brown ware, white-on-red ware, polished red and black ware, paint cloisonné, engraved black ware with red-filled designs, legged vessels, annular base vessels, effigy vessels, handled vessels, miniature vessels, composite silhouette vessels, pottery *molcajetes*, simple bowls, dishes, ollas, and special design elements including S-scrolls, parallel wavy lines, horned-serpent motifs; *clay artifacts* such as spindle whorls, cylinder seals, pottery stamps, pottery tobacco pipes, effigy whistles, pottery discs, mold-made and modelled figurines; and such *special artifacts* as mosaic mirrors, composite mosaic plaques, turquoise mosaics, lunate shell ornaments, incense burners, stone crosses, obsidian blades, and bells, chain, celt, disc, awl and needle of copper.

Utilizing these traits and the information available as to the relationships of the Sinaloa cultures with Mesoamerica, specific period relationships with the Valley of Mexico sequence may be inferred. There is no trace of Aztec-period occupation in the Chalchihuites area and apparently all known Chalchihuites sites were already ruins at the time of the Spanish Conquest. Three sherds of Dunn Ware of the Culiacan Culture found in *late* Rio Tunal association at Schroeder probably date the terminal occupation there at around A.D. 1200 to 1300. In general the Rio Tunal-Aztatlan interval shows strong connections—pipes, pottery molcajetes, spindle whorls, copper, mold-made figurines, etc., with the Toltec period of the Post Classic horizon in the Valley of Mexico.

At the other end of the time scale the engraved black tripod ware and the rare paint cloisonné of the Alta Vista Phase suggest Teotihuacán affiliation, while the leading Alta Vista pottery type, Suchil Red-on-Brown, is quite similar to Coyotlatelco ware from the Valley of Mexico. It now appears that this latter ware probably developed in late Classic times—probably Teotihuacán IV—and survived into the early Post Classic, forming the earliest dominant ware at Tula.[16] This, plus the apparent position of the Alta Vista Phase with relation to the Sinaloa sequence, suggests its approximate contemporaneity with Teotihuacán IV. The position of the succeeding Ayala Phase with reference to the Sinaloa sequence suggests a very late Classic dating for it, while the Las Joyas Phase will probably prove to be early Post Classic.

Concepts of actual chronology of the Valley of Mexico appear to be changing with such rapidity as to make estimates by non-specialists quite dangerous. The critical time plane is the Classic–Post Classic boundary, and current estimates of the age of this horizon seem to vary from A.D. 600 to 900, with general approval still given the latter date (based on the Goodman-Martínez-Thompson correlation of Maya and Christian calendars).[17]

CHALCHIHUITES–SOUTHWESTERN RELATIONSHIPS (PRIMARILY HOHOKAM)

Regardless of its obvious Mesoamerican affiliations, the Chalchihuites Culture shares many traits with the cultures of the Southwestern United States, especially the Hohokam Culture of southern Arizona. Thus, the Chalchihuites *stone complex* is overwhelmingly Southwestern with shared traits such as troughed metates and two-hand manos, stone pestles and mortars, 3/4 and full-grooved axes, full-grooved mauls, "medicine stones," stone rings, cupped stones, stone balls, plain and carved stone bowls, polishing stones, stone crosses, plain slab palettes and associated paint pestles, and small stemmed triangular, and side-notched arrowpoints.

Also shared with the Hohokam are many *ceramic traits* such as concentration on red-on-brown (red-on-buff, red-on-cream, etc.) painted wares and associated red and smudged wares; small jar forms, olla forms, simple bowl forms, tripod vessels, human effigy vessels, vessels with effigy figure at bowl edge facing bowl, basket-handled vessels; and such decorative elements as interlocking S-scrolls and ticked (or fringed) scrolls, groups of parallel wavy and straight lines, rim fringe decoration with parallel diagonal lines, opposed terraced figures, repeated small unit figures (S, plus, swastika, bulls-eye, etc.), repeated small conventionalized animals in bands, horned-serpent depictions and frequent quadrate division of bowl interiors.

Special artifacts and traits held in common are mosaic plaques or mirrors, turquoise mosaics, small carved shell animal figures, circle-dot decorated shell gorgets, clay figurines, potsherd discs, spindle whorls (late), copper bells (late), mortuary ollas (late), clay floor basins, platform mounds and courts (late), and possibly ball courts.

Such a long list of similarities suggests contacts or common origin or both. The presence of such obvious Mesoamerican intrusives as copper bells in the Hohokam calls for actual contact and there are sufficient specific relationships between Chalchihuites and Hohokam to suggest the former as the source of Mesoamerican influences on the latter, rather than the culture of coastal Sinaloa.

With regard to specific period relationships there are no apparent contacts between the Alta Vista–Ayala phases and the Pioneer Period of the Hohokam —unless the simple quadrate division of painted designs on bowl interiors be such. Both Colonial and Sedentary Periods show strong pottery design similarities with the Chalchihuites Culture, including especially several forms of plain and ticked interlocking scrolls, opposed terraced figures, parallel wavy lines, fringed rims, and the use of repeated unit figures and conventionalized small animals in bands. However, with their first appearance in Colonial times the conventionalized animals include quadruped and human forms which seemingly do not appear in the Chalchihuites Culture until the early Las Joyas Phase (Refugio Red-on-Brown). Since copper artifacts appear for the first time in the Rio Tunal and the Sedentary Period it seems that the Las Joyas Phase may well correlate with the Colonial Period and the Rio Tunal with the Sedentary Period, perhaps with early Classic Hohokam as well. Verification for the Sedentary–Rio Tunal correlation is found in the close resemblance between Otinapa Red-on-White of the Rio Tunal Phase with Three Circle Red-on-White of the Mogollon culture—which correlates also with the Sedentary Period.

Absolute chronology is as difficult in the Hohokam as in central Mexico. Most workers would agree at an initial date for the Pioneer Period at around the beginning of the Christian Era or shortly before. Orthodox dating for the beginning of the Colonial Period is around A.D. 550–600, for the Sedentary around A.D. 900, for the Classic Hohokam around A.D. 1200.[18] However, Di Peso has suggested informally that a dating of *circa* A.D. 900 to 1300 for the Colonial-Sedentary interval might better conform with the facts.[19] Neither dating seems secure at the moment.

MESOAMERICAN SOUTHWESTERN CORRELATIONS

Applying the conclusions reached in the above discussions to the problem of synchronization of Mesoamerican-Southwestern cultural sequences, a tentative correlation may be made. In time, the Pioneer Period of the Hohokam seems to equate with the Classic Horizon of the Valley of Mexico. However, there seems to have been little contact between Mesoamerica and the Hohokam (or the Southwest in general) at this time. Following the original introduction of agriculture and then pottery from Mesoamerica the Pioneer Hohokam went its own way, probably working out a stable pattern of agricultural village existence.

However, the Chalchihuites Culture was established in Durango, bringing Mesoamerica closer than before to the Hohokam. Apparently at about the beginning of the Post Classic Horizon and the Toltec Period, Chalchihuites influence began to be felt on the Hohokam, changing almost overnight the simple Pioneer Hohokam into the efflorescent Hohokam of the Colonial Period. With the arrival of copper in the Sedentary period the Hohokam were exposed to such full Toltec influence that by the beginning of the Classic Hohokam even Mesoamerican architectural features such as platform mounds and courts had begun to take root in the Hohokam. During the Las Joyas and Rio Tunal Phases indirect contact between Chalchihuites and Hohokam Cultures may well have been effected through medium of the simple mountain culture, the Loma San Gabriel pattern, which had spread along the high Sierra Madre Occidental through much of the intervening area.

Interestingly enough, contact between Chalchihuites and Hohokam seems to have been realized on an effective basis just at the time that the pyramid (and perhaps the ceremonial center) complex at the Schroeder Site (the northernmost pyramid site known) had fallen into disuse. If the contact had been made somewhat earlier, pyramids and ceremonial centers might well have spread into the Southwest via the Hohokam.

Actually, Mesoamerican contacts with the Classic Hohokam appear to have come directly from the Sinaloa cultures—specifically late Aztatlan (where a Mixtec–Puebla type of ceremonialism had begun to flower) and Culiacan Cultures, probably owing in part to the coincident disappearance of the Chalchihuites Culture. Hence, much of the Mesoamerican influence that seems to have spread throughout the Southwest at this time appears to have entered the Southwest through a Sinaloa rather than a Chalchihuites screen.

Colonial, Sedentary, and Classic Hohokam, then, appear to correlate with Post Classic Mesoamerica; the first two periods with the Toltec Culture, the latter at least in part with the Aztec horizon. Pioneer Hohokam appears to correlate with Classic Mesoamerica, and the origin of basic Southwestern ceramics and agriculture must lie in the Pre-Classic horizon or earlier. As to absolute chronology, the interval of variation in accepted chronologies is such as to allow perfect correlation or some inconsistency. Thus, if the critical Classic–Post Classic break is dated at around A.D. 600 through use of the Spinden correlation or other means, the correlation given here would favor the more orthodox Southwestern estimates for the beginning of the Colonial Period at *circa* A.D. 550–600. If the more orthodox Goodman-Martínez-Thompson calendrical correlation and derivatory dates are accepted this critical date line would be placed at around A.D. 900, which would correspond well with the recent suggestions of Di Peso as to the beginning of the Colonial Hohokam at that time.

The tentative correlation of Mesoamerican and Southwestern (Hohokam) cultural sequences proposed here must, of course, be tested intensively and will undoubtedly suffer considerable modification. In the meantime, however, it appears that we have made considerable progress in the correlation of the sequences of the two areas and that further work in Northern Mexico may soon produce conclusive evidence along these lines.

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KELLEY—NORTH MEXICO AND THE CORRELATION OF MESOAMERICAN AND SOUTHWESTERN CULTURAL SEQUENCES Sept. 1956

Dates?	Valley of Mexico	Coastal Sinaloa	Durango-Zacatecas	SW (Hohokam plus)
	Aztec			
		Culiacan	?	*Classic*
	POST-CLASSIC			
	Toltec	*Aztatlan*	*Rio Tunal*	*Sedentary*
Possibly		(*Late Chametla-Chametla Red-Rim*)	*Las Joyas*	*Colonial*
600–900 A.D.				
		Middle Chametla	*Ayala*	
	Teotihuacan IV			
		Early Chametla	*Alta Vista*	*Pioneer*
	CLASSIC			
	Teotihuacan III			

Notes

1. This work was carried out under permits from the Instituto Nacional de Antropología e Historia, México, D. F. My most sincere thanks are due to Dr. Eduardo Noguera, Director de Monumentos Prehispánicos, of the Instituto for his continued aid and advice.

2. Manuel Gamio, "Los Monumentos Arqueológicos de los Inmediaciones de Chalchihuites, Zacatecas." *Anales del Museo Nacional de Arqueología, Historia, y Etnografía,* 3a Epoca, 2 : 469–492, Mexico, D. F., 1910.

3. Eduardo, Noguera, "Ruinas Arqueológicas del Norte de Mexico, Casas Grandes (Chihuahua), La Quemada, Chalchihuites (Zacatecas)." *Publicaciones de la Secretaria de Educación Pública.* Mexico, 1930.

4. J. Alden Mason, "Late Archaeological Sites in Durango, Mexico, from Chalchihuites to Zape." *Twenty-fifth Anniversary Studies, Publications of the Philadelphia Anthropological Society,* 1 : 127–146. Philadelphia, 1937.

5. Donald D. Brand, "Notes on the Geography and Archaeology of Zape, Durango." *So Live the Works of Men,* Seventieth Anniversary Volume Honoring Edgar Lee Hewett, pp. 75–105. Albuquerque, 1939.

6. Robert H. Lister and Agnes M. Howard, "The Chalchihuites Culture of Northwestern Mexico." *American Antiquity,* 21 : (2) 122–129. Salt Lake City, 1955.

7. See Gamio, 1910; Noguera, 1930.

8. See Mason, 1937; Brand, 1939.

9. Carl Sauer and Donald D. Brand, "Aztatlan, Prehistoric Mexican Frontier on the Pacific Coast." *Ibero-Americana*: 1. Berkeley, 1932.

10. Gordon F. Ekholm, "Excavations at Guasave, Sinaloa, Mexico." *Anthropological Papers of the American Museum of Natural History,* 38 : (2), New York City, 1942.

11. Isabel Kelly, "Excavations at Chametla, Sinaloa," *Ibero-Americana*: 14, 1938; "Excavations at Culiacan, Sinaloa," *Ibero-Americana*: 25, 1945. Berkeley.

12. E. W. Gifford, "Surface Archaeology of Ixtlán del Rio, Nayarit." *University of California Publications in American Archaeology and Ethnology,* 43 : (2), Berkeley, 1950.

13. Chart (mimeographed accompanying paper presented before the American Anthropological Association, Boston, Mass., November 1955, by Gordon Ekholm.

14. Kelly, 1938, Tables 1, 2, 3; pp. 18–19, 36.

15. Ekholm, Mimeographed Chart, Boston, 1955. See Note 13.

16. Bertha P. Dutton, "Tula of the Toltecs," *El Palacio,* 62 : (7, 8) 239–242, Note 27. Santa Fe, 1955.

17. Dutton, 1955. See Chronological Chart, pp. 208–209. Also Ekholm, Boston Chart.

18. Joe Ben Wheat, "Southwestern Cultural Interrelationships and the Question of Area Co-tradition." *American Anthropologist,* 56 : (4) 576–591. 1954. See Fig. 1, p. 577.

19. C. C. Di Peso, Personal communication, August, 1955. Santa Fe, New Mexico.

PALEO-ESKIMO IN DISKO BAY, WEST GREENLAND

Helge Larsen

In 1949 a Danish amateur archaeologist, Hans Mosegaard, brought to the National Museum in Copenhagen a collection of stone artifacts which he had excavated the previous summer in a cove west of the present settlement of Satqaq in the northern part of the Disko Bay, West Greenland. With the exception of a fragment of a shallow bowl of soapstone, reminiscent of a Dorset lamp, all the types were well known in finds from West Greenland; in fact, the National Museum already possessed hundreds of similar artifacts sent in by private collectors who had picked them up on beaches particularly in the Disko Bay area. As early as 1907 O. Solberg had described the same types of artifacts and ascribed them to a Greenland "Stone Age," but Mathiassen, who from 1929 to 1934 conducted the archaeological investigations in West Greenland, denied the existence of such a "Stone Age" and maintained that this stone industry was the result of a special and rather late development in West Greenland. This interpretation was questioned by Henry Collins who in 1934 and again in 1937 and 1940 pointed out the "resemblance between these stone implements and those of the Old Bering Sea and Dorset cultures," and suggested "that the 'Stone Age' complex may represent a mixture of Dorset types —or special Greenland variants of these and Thule types." With Mosegaard's find at Sarqaq, Solberg's "Stone Age" gained new actuality, and particularly after Jørgen Meldgaard in 1952 published the material and presented it in a new light. Meldgaard interpreted the Sarqaq material "as evidence of an Eskimo culture closely related to the earliest Eskimo cultures in Alaska," referring to Ipiutak, Near-Ipiutak, and the Denbigh Flint Complex. According to Meldgaard the "Sarqaq phase" in West Greenland had apparently later been influenced to some extent by the Canadian Dorset culture. For the the whole stone complex Meldgaard suggested the term "The West Greenland Paleo-Eskimo Culture."

The time was now ripe for renewed investigations in West Greenland, and in the summer of 1953 the National Museum sent a party of four to Disko Bay for the purpose of locating new paleo-Eskimo sites, if possible to find dwelling remains of this culture, and to solve the problem of the relation between the Sarqaq phase and the Dorset. It was decided to work in two groups; one, consisting of Professor P. V. Glob from the University of Aarhus and myself, made a reconnaissance by motorboat, visiting known archaeological sites and locating new ones; and the other group, consisting of Meldgaard and George Nellemann, spent most of the time making systematic excavations at Sermermiut, probably the largest abandoned settlement in West Greenland and situated at the entrance to the ice-filled Jakobshavn Isfjord.

When we chose Sermermiut as the main objective of our investigation—the place at which we hoped to find stratigraphical evidence of the cultural sequence in West Greenland, it was first because Sermermiut was known as one of the main sources for the chipped stone artifacts we are dealing with here, and secondly on account of its thick midden, known from earlier excavations by Thomsen and Porsild. And we were not disappointed. Sermermiut faces a small bay which is separated from Jakobshavn Isfjord by a rocky promontory. As we shall see later, this location at the base of a promontory is characteristic of the paleo-Eskimo sites in Disko Bay. The huge midden terminates in an approximately 2 m. high, almost vertical wall in which the layers of deposit are visible and exposed to constant invasion by the sea. Meldgaard's and Nellemann's main task was to make a 20 m. long, vertical profile in this wall and to record every artifact and piece of refuse found in the profile. Other profiles were made but I shall deal only with the main profile. In many places the culture layers were disturbed by digging, probably mainly for house pits by neo-Eskimo who occupied the site from the 12th to the 13th century until it was abandoned about 1850, but fortunately the excavators also found undisturbed layers as in the 12–14 m. section. The bottom layer (A) is virgin soil consisting of gravel with many stones. Resting on this is a 5–10 cm. thick layer (B) consisting of grey-black humus with remains of grasses and containing artifacts belonging to the Sarqaq phase primarily in the low part of the layer, which also contained scattered stones with sooty or cracked surface indicating contact with fire. Layer (C), 10–15 cm. thick, consists of brown, only slightly humified turf, and contained no artifacts, and artifacts did not occur in the lower part of layer (D), a 6–12 cm. thick layer of fine, yellow sand which in places is folded. In the uppermost part of (D) and the lower part of layer (E), however, Dorset artifacts were found, (E) being a 4–16 cm. thick, brown layer reminiscent of layer (B). Also here fire-marked stones were found with the artifacts. A thin, sandy layer separates (E) and the next layer (F) which is 8–14 cm. thick and consists of black, sphagnum-like turf with many plant remains but no artifacts. A very pronounced layer of compressed grass, indicating an old grass surface, separates layers (F) and (G) and at the same time marks the termination of the paleo-Eskimo habitation at Sermermiut, because on top of this is a mixture of turf and gravel and of paleo and neo-Eskimo artifacts which came from the excavation of a pit between the 11 and 12 m. mark. Overlying this mixed layer (G) is 140 cms. of midden (H) containing much organic material, remains of houses, and artifacts which belong to the neo-Eskimo culture.

The undisturbed layers in the profile (which have just been described) give us undisputable evidence of the culture sequence at Sermermiut and we can be fairly certain that it is valid for the whole Disko Bay area and probably much more. In addition to the fact that the two culture-bearing layers (B) and (E) are separated by sterile layers indicating two successive occupations, we learn from the artifacts that we are dealing with two different cultures. Artifacts from the Sarqaq horizon of the profile are all made of silicified slate, and with the exception of a concave side-scraper they are all chipped on both surfaces. The burins and two out of four arrowpoints are partly ground. The other artifacts shown are symmetrical and asymmetrical blades, the latter undoubtedly knife-blades, some or all of the symmetrical ones probably projectile points. To this assemblage we may add five types from layer (B) in other parts of the profile, viz., a symmetrical slender, oval blade, a side blade, a short adze blade, a thumb-nail end-scraper, and an end-scraper made from a long blade. It is also worth mentioning that the 99 pieces of stone refuse from section 12–15 m. all are of silicified flint.

The Dorset types from layer (E) in the same section are asymmetrical notched blades, an asymmetrical blade without notches, two large and a small end-scraper, microflakes, a three-sided, ground point of silicified slate, and a sherd of a soapstone lamp. Except for the two last mentioned the artifacts in this horizon are made of chalcedony, flint, and rock crystal. This preference for harder materials is also borne out by the stone refuse: of the 210 stone chips found in section 12–15 m., 179 are of chalcedony, 32 of silicified slate, 8 of flint, and 2 of rock crystal. The chipping technique is as a whole inferior to that of the Sarqaq people. Two characteristic Dorset types were not found in this section but in other parts of the profile with good stratigraphy: micro-cores of flint or rock crystal, and small, convex end-scrapers with flaring edges.

The significance of the finds from Sermermiut is obvious: in the first place they gave us definite proof that two paleo-Eskimo cultures, an earlier Sarqaq culture and a later Dorset culture, preceded the neo-Eskimo or Thule culture; and secondly, the assemblages of artifacts, small as they are and yet representative, may help us identify finds which occur under stratigraphically less favorable conditions. Even the stone refuse is indicative, because when we bear in mind that 97% of all stone chips in the Sarqaq horizon are of silicified slate, and approximately 80% of the stone chips in the Dorset horizon are of chalcedony, it is not accidental, hence the refuse can be used to identify finds which do not contain identifiable artifacts.

While Meldgaard and Nellemann were unravelling the problems of chronology at Sermermiut, Glob and I were touring the coast of Disko Bay from south to north and going ashore at every place which offered the slightest possibility for former habitation. Realizing that we could not expect to find visible ruins or other surface remains of paleo-Eskimo habitation and would have to search closely for traces of it, we were particularly careful in searching terraces, which Eigil Knuth had found to be the preferred paleo-Eskimo camping places in Peary Land and Northeast Greenland, and it was actually on a terrace that we did find the first remains of paleo-Eskimo dwellings or camp site in West Greenland.

It was towards evening on the first day of our reconnaissance that we dropped anchor at a rocky point on the north shore of Kangersuneq about six miles south of Christianshaab. Connecting the point with a gently sloping hill is an approximately 100 feet wide terrace of sand and fine gravel with spots of low vegetation 13 feet above sea level. Having searched in vain all day we were pleasantly surprised to find on the surface a number of stone chips and three artifacts of Sarqaq type, two burins and a thumb-nail end-scraper, but that was only the beginning. The following day brought to light three irregular pavements of hand-sized stones which were brittle or broken and blackened by fire. The pavements were covered by two inches of drift sand and were lying in a row, 50 to 100 feet apart and each of them covering an area of approximately 6 by 6 feet. On top of the stones were found stone chips, ashes, and a few artifacts. Judging from the appearance, the pavements seemed to be contemporaneous and it was therefore surprising to find the following assemblages of artifacts: in no. 1 a projectile point of Sarqaq-type and half of a circular, rather crude stone lamp, in no. 2 a burin, a partly rubbed arrowpoint with serrated edges, and a triangular end-scraper, all good Sarqaq-types, and in no. 3 a wide-based flint knife-blade with side notches and a microflake, both typical of the Dorset horizon in Sermermiut. The stone refuse corresponds to the distribution of artifacts with a preponderance of silicified slate in the first two and of flinty material in no. 3. Even if this seems to indicate that nos. 1

and 2 were made by Sarqaq people and no. 3 by Dorset people, we must take into consideration first that the find is rather small, and second that it was made so close to the surface that we cannot exclude the possibility of later mixture. The real significance of this find is, however, that it gave us the clue as to where we should look for paleo-Eskimo sites and what to look for. Of the twenty other sites we found during the summer I shall limit myself to a brief description of those which contained dwelling remains.

The next major site we examined, on the island Igdluluarssuit at the north end of Atâ Sund, had several traits in common with the first. The site is situated on a slightly sloping plain of gravel and sand at the base of the rocky, northwest point of the island, and consists of three irregular pavements of hand-sized and larger stones, some of them with marks of fire. The pavements, which were covered by a two to four inches thick sod layer, were larger than at Igdlorsuatssiait, one of them 18 by 18 feet, and quite close together. They were so close to the high-water mark that part of them had been carried away by the sea, a clear indication that the land is lower than when the site was occupied. This, by the way, was not an isolated phenomenon; in many places we noticed that paleo-Eskimo sites were more or less washed away by the sea. We were unable to interpret the meaning of these accumulations of stones but on account of their extension and the presence of some flag-stones I am inclined to consider them as being floors in tents.

The artifacts found on and among the stones were a mixture of Sarqaq and Dorset types with the former as the more numerous (76 out of a total of 97 artifacts). Among the Sarqaq types were 10 burins, 3 burin spalls, and a variety of finely chipped blades and scrapers, while the Dorset types were limited to 16 micro-flakes, 3 micro-cores, an end-scraper with flaring edges, and an asymmetrical blade with vestigal side notches. In the find is one type which occurs for the first time in Greenland, a piece of pumice with a longitudinal groove, probably used for polishing arrow shafts, and common in finds from Alaska. Another western type, pebbles with two or four notches, was found in other paleo-Eskimo sites and also for the first time in Greenland.

We also visited Igdlorssuit, a large site well known from Mathiassen's excavations in 1933. Here an area of about twenty square meters was cleared of the sparse vegetation, revealing a considerable number of flat stones, some of them forming a regular pavement, and a fireplace with a thick layer of black ashes covering an area of about 3 by 4½ feet. A culture layer with stone artifacts and refuse was found under a rather thin layer of sod and resting upon gravel or bare rock. The culture layer petered out towards the edges of the excavation and there was nothing to indicate the extent or shape of what must have been the floor of a dwelling. It is worth noticing that a stone grave, probably contemporary with the neo-Eskimo house-ruins, was built on top of this floor. The assemblage of artifacts was composed of virtually the same types and with a similar proportion of Sarqaq and Dorset types as in the find from Igdluluarssuit. Half of the Sarqaq specimens were burins and the Dorset types were exactly the same. Of the refuse, 503 pieces were of silicified slate and 104 of harder materials.

Next to Sermermiut the island Qeqertaq is the best-known source of stone implements, particularly micro-blades and other Dorset types, and we had therefore expected to make some interesting observations here. We soon discovered, however, that the chances of finding undisturbed dwelling remains were nil because the main site, a rather low peninsula south of the present settlement of Qeqertaq, had apparently for ages been a favorite place for cutting peat, which is used extensively as fuel by the Greenlanders. This peat-digging had exposed large quantities of stone chips, and a considerable number of artifacts could be picked up on the surface, but at the same time it had destroyed the many remains of dwellings which must have been there. We did learn, however, that, contrary to most of the sites we investigated, this was primarily, but not exclusively, a Dorset site. Of 112 identifiable artifacts found, 98 are Dorset types like micro-flakes and cores, end-scrapers with flaring edges, and small, trapezoid end-scrapers, knife-blades with side notches, and three-sided ground points of silicified slate. In accordance with this we found 627 pieces of chalcedony, jasper, and flint and only 267 pieces of silicified slate.

Equally disappointing as far as dwelling remains is concerned was Sarqaq which has given the name to the culture. The site Mosegaard investigated in 1948 is situated in a cove flanked by two rocky points west of the present settlement of Sarqaq. Stone chips and artifacts are abundant on the surface of the grassy, gently sloping plain and on the beach in front, but the vegetation and the many scattered stones betray late activity, probably peat-digging which, as we had experienced at Qeqertaq, diminishes the chances of finding undisturbed dwelling remains. We did find "pavements" with hand-sized, fire-marked stones and ashes similar to those at Igdlorssuit and Igdluluarssuit but none of them were intact. The best preserved covered an area of six square meters and yielded 50 specimens, half of them burins and burin spalls. With the exception of two, a knife-blade with side notches and a completely rubbed, asymmetrical blade, they were all Sarqaq types. Of a total of 113 identifiable specimens found at Sarqaq, 14 were Dorset and later types, indicating that it is not a "pure" site. The preponderance of Sarqaq types and the fact that 94% of the refuse consisted of silicified slate show that it is primarily a Sarqaq site but that also there are traces of later occupants.

About two miles further west is another cove likewise flanked by rocky points. In the western end of a slightly sloping, sandy plain is the old settlement, Igdluluarssuk, described by Mathiassen, but no remains of habitation were visible in the eastern part which was partly covered by heather, willows, and Betula nana. Under this we discovered a number of head-sized and larger stones which appeared to belong to two oblong arrangements of stones, undoubtedly the remains of old dwellings. They covered an area of approximately 40 square meters and were situated about two meters above the high-water mark. The most conspicuous feature of the northernmost dwelling was two fireplaces, one in the middle and one in the southwest corner. The one in the middle consisted of head-sized stones placed on edge, and in this as well as in the other were a considerable number of hand-sized, fire-cracked stones. In the other dwelling was a double row of rather large stones which seemed to be remnants of a wall, and facing the sea were other stones which might indicate the presence of the entrance. If our interpretation of the stones is correct, the dwelling has been oblong, rounded, possibly wider in front than in the back, and with an entrance in the front wall. East of the centre was a fireplace made of large, flat stones placed on edge, and within this frame were in the bottom small, flat stones and on top of them a number of fire-marked, round stones. More of these stones were scattered over a blackened area near the fireplace and a third group occurred west of the "entrance."

Although we are unable to say whether these stone structures are the remains of tents or houses we can be fairly certain that they represent the earliest dwelling remains hitherto found in West Greenland. Unfortunately very few artifacts were found in the two dwellings, *viz.*, seven in no. 1 and one in no. 2, but all of them are typical of the Sarqaq culture, and in addition we have supporting evidence in the refuse. Of 402 stone chips from no. 1, 394 or 98% are of silicified slate, and in no. 2 we found exactly the same percentage, *viz.*, 117 pieces of silicified slate out of a total of 119.

The last site I would like to mention is Tupersuit at Rodebay, an outpost north of Jakobshavn. Here it was the peat-digging that led to the discovery of a paleo-Eskimo site on a rather uneven slope partly covered with grass and partly denuded of vegetation, exposing either patches of black soil or bare rock. In addition to numerous stone chips and artifacts that were scattered over a large area we found three fireplaces with fire-marked stones. One of them was in a flat depression in the rock in connection with an accumulation of larger stones which presumably once had formed the walls of a dwelling. The fireplace surrounded by flagstones, consisted of three rather flat stones placed on edge at right angles to each other, and inside this frame were ashes and a heap of the usual hand-sized, fire-marked stones. Similar stones were found in two other places within the "dwelling" and in both places the earth around them was scorched. The occurrence of this kind of stones in most of the sites needs an explanation and it first occurred to me that they must be boiling stones. Considering that not a trace of stone or clay pot was found it seemed most likely that cooking must have been done with hot stones in wooden or skin containers. Meanwhile, Junius Bird has called my attention to the fact that the stones might be "fire stones" used for keeping the heat, and that possibility is worth keeping in mind. Whatever the explanation might be, it is a culture trait not recorded before from the eastern Arctic. We also know that this trait belongs in the Sarqaq culture, because not only are all 83 specimens found in this "dwelling" Sarqaq types, but virtually all the characteristic types are represented, and of the stone refuse 94% was of silicified slate.

The investigations described here were followed up by even more extensive excavations at Sermermiut in 1955, and plans have been made for a survey of the entire Greenland coast.

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THE PLAINS ARCHAIC CONCEPT

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INTRODUCTION

In a recent examination of the general problem of Paleo-Indian and Archaic relationships in North American archeology (Mayer-Oakes, 1955), I briefly discussed the concept of a "Plains Archaic" but did not explore it in any detail. This concept has not previously been the subject of a careful study. Because it has, however, proved to be a useful interpretive device and, for example, has been accepted in the recent developmental synthesis published by Willey and Phillips (1955), I feel that a detailed examination of the concept and its empirical basis is long overdue. This paper, thus, outlines the development and present status of the "Plains Archaic Concept."

TERMINOLOGY

Before discussing the specific historical steps in the development of this concept, it is necessary to note the variety of connotations for the terms "Paleo-Indian" and "Archaic," both of which are important to the concept.

In North America east of the Rocky Mountains, the term "Archaic" has fairly general usage in at least two main ways. In one sense, Archaic is considered to be a developmental stage and generally connotes the presence of a diversified hunting-gathering-fishing type of economy accompanied by a social organization reflecting the earliest stages of semi-sedentary life. The presence of ceramics, agriculture or a developed magico-religious structure is not expected in such a stage.

The other main sense in which Archaic is used is as a time-period name and, as such, it may crosscut the first usage. In the ideal case a developmental stage would correlate directly with a time period. Such a correlation is evidently often assumed in the unspecified usage of the term. Actually, complicating factors such as the presence of cultural centers and the facts of cultural isolation may operate to make the stage and the time period disagree. An example of this kind of disagreement is the interpretation given to the Eva focus of western Tennessee by Kneberg (1952).

Likewise, the term "Paleo-Indian" has been used in these two main ways. As a developmental stage the term Paleo-Indian has been applied to small nomadic hunting groups most of which give some evidence for a specialization in the nature of game killed. In the temporal sense, Paleo-Indian has been reserved for units the extreme age of which could be fairly definitely established. The presence of geological or paleontological dating factors for this period has been supplemented recently by radiocarbon dating.

While there have been differences in the usage of the two terms, not all the confusion can be ascribed to the developmental and temporal aspects of these usages. There is still a third factor implicit in the current usage of the terms. This is the factor of genetic cultural relationship or *tradition* and is most frequently expressed, for example, in terms of the exclusive association of fluted projectile points with the Paleo-Indian unit. For Archaic an association of ground and polished stone tools with a rather heterogeneous projectile-point assemblage is most often posited. The terms have, thus, come to connote not only stages of culture and time periods but actual cultural inventories.

ORIGIN AND DEVELOPMENT OF THE CONCEPT

Perhaps the earliest publication pertinent to our discussion is the site report of excavations at Signal Butte, Nebraska, by Strong (1935). Temporal interpretation in this report is based on geological factors. The original conclusion of great antiquity for the earliest levels at the site would place them in a Paleo-Indian time period. In 1950 Bliss criticized the interpretation of Signal Butte I as an early complex. On the basis of limited testing he breaks Signal Butte I into three sequent typological stages and suggests alternative and much younger dating conclusions. This more recent dating is now supported by a C-14 date of 1495 B.C. for Signal Butte I.

Excavations conducted in Nebraska in 1948 by Holder and Wike (1950) resulted in the definition of the Frontier complex, compared by the excavators to Signal Butte I and other early Plains units, some of which were just being discovered. In the detailed report published in 1949, Holder and Wike suggest that the Frontier complex is part of a larger Plains Archaic unit. I believe this is the first published use of the phrase to denote the present interpretive concept. A subsequent C-14 date on the Frontier complex of 8540 B.C. aligns this unit with the earliest known on the Plains and is completely at variance with the recent dating of Signal Butte I.

Late in 1948 I commenced my studies of the pre-pottery unit found by University of Chicago excavations at Starved Rock, Illinois, during 1947 and 1948. At the suggestion of Kenneth Orr I began an investigation of the possible western affiliations of this material. Initial studies published in 1949 (Mayer-Oakes, 1949 a & b) indicated the cohesiveness of the unit as well as the distinctiveness of the projectile-point complex. In order to acquaint myself more directly with Plains lithic complexes I attended the 1948 and 1949 meetings of the Plains Archeological Conference at Lincoln, Nebraska. Examining specimens displayed at these meetings and in the several museums in Lincoln, I was impressed with the resemblances of the Starved Rock Lanceolate type of projectile point to: 1st, points from the Angostura reservoir in South Dakota; next, points of the Frontier and Lime Creek complexes; and finally, points of the Nebo Hill complex.

In conducting the final season of excavations at Starved Rock in 1949 my interest was to *establish* the pre-pottery complex which had been suggested by the testing of the previous two years. Conclusions derived from this 1949 season did substantiate and expand our knowledge of the complex I called "Starved Rock Archaic" (Mayer-Oakes, 1951). In trying to fit this unit into the general framework of known units I was compelled by my evidence to make use of an idea latent in much archeological discussion of that time—the concept of a Plains culture area on an Archaic stage or time level. I did, as a matter of fact, draw specific comparisons between Starved Rock and several other units from the Plains of Prairies geographic areas. The most important of these comparative units were: Frontier, Angostura, Nebo Hill, and Signal Butte I. None of them was blessed at that time with absolute dates, so comparisons of typology and stage were paramount within the general framework of pre-pottery times.

The concept at this time was invoked to explain a complex of material traits which had primary similarities to Plains early lithic complexes, but also contained traits of eastern Archaic or other units. The area was conceived as a zone of interchange between east and west during the relatively recent Archaic period.

Subsequent to my specific suggestion of a Plains Archaic area, several finds which gave support to the concept have been reported. Logan (1952) has described a stratified Missouri site which documents the change through time from a Plains-oriented unit to an eastern Archaic unit. The lower levels of his Graham Cave site are definitely units to be understood as part of the Plains Archaic, typologically and geographically. As at Starved Rock, no geological or absolute dating factors were present.

Baerreis (1953) has reported what he calls an Early Archaic site from near Madison, Wisconsin. Known as the Airport village, this site he considers as transitional typologically between Paleo-Indian and Early Woodland. The "mixed" assemblage is tentatively interpreted as a unit with closest affinities to the Starved Rock Archaic. Here, again, is a Plains Archaic unit both typologically and geographically.

Reporting an Archaic complex found by test pitting and surface hunting at the Chrisman site in southern Illinois, McGregor (1954) specifically aligns it with Starved Rock Archaic, and further suggests that both are parts of an Illinois River aspect of a Plains phase within the broad Archaic pattern. This is one of the few published extensions of the concept.

The Angostura reservoir projectile-point type called "Long" type by Hughes (1949) has become a key type in the Plains area and has had its name changed, more or less by common agreement, to the "Angostura" type. Because it is a form identical with the Starved Rock Lanceolate type, I feel it is highly significant to our discussion of the Plains Archaic concept. The fact that this type is widespread can be seen from its occurrence in southern Texas (Orchard and Campbell, 1954) and extreme northwestern Canada(MacNeish, 1955). The Angostura-type site has been C-14 dated at 7715 years ago. In Texas the context is uncertain but may be early Archaic, about 6000 years old, while the Canadian date is 4600 years ago by C-14.

The 1954 discovery of a second mammoth and associated artifacts at Iztapan in the Valley of Mexico was especially interesting because the two projectile points were not fluted as would be expected on the basis of North American finds. Rather, they were lanceolate points, one a fair example of an Angostura, the other a "laurel leaf." While this extends the geographic range of the Angostura type far to the south, the time factor here is unknown. The association with extinct fauna is important, however, since Angosturas are rarely in such a context.

The most recent unit reported which fits our concept of Plains Archaic is the Havey site near Madison, Wisconsin. The surface collection reported from this site is interpreted by Nero (1955) as a complex transitional between Paleo-Indian and Early Archaic complexes. Although not cited as such by the author, the Havey site is a good candidate for the Plains Archaic both typologically and geographically.

RECENT DEVELOPMENTS

In a broad developmental synthesis of New World archeology, Willey and Phillips (1955) have recently made use of the Plains Archaic concept in an effort to interpret various components known from the Plains and Prairies. They have recognized that several of these units give better evidence for an Early Lithic stage of development than an Archaic stage, but prefer to interpret all as basically on an Archaic level. In the light of several recent C-14 dates and information presented at the recent Archaic conference, I feel this position needs revision.

For some time the well-known Old Copper complex of Wisconsin has been considered a late Archaic unit, and thus it was striking to note the *early* C-14 dates released in 1954 (Ritzenthaler, 1954). Two samples gave dates of 5600 and 7510 years ago, respectively, thus establishing Old Copper as the earliest manifestation of man in northeastern North America known from C-14 dates in 1954. This Wisconsin priority was short-lived, however.

One of the sites early considered as an important Plains Archaic unit is Graham Cave. At the 1955 Archaic Conference, C-14 dates on this site were released which indicate that the lowest levels were occupied from 8830 to 9700 years ago. The intermediate levels dated 7900 years ago. Because of the rather complete previous acceptance of this material as "Plains Archaic" the dates seem surprisingly early.

At the same conference C-14 dates on Tennessee Archaic units were released which indicated that the early Eva complex goes back 7150 years. This date is some 2000 years older than other eastern Archaic dates and apparently correlates with level 4 at Graham Cave wherein the Missouri complex has taken on a decidedly eastern flavor.

Still a third series of important C-14 dates was released at the Archaic Conference by Fowler (1955) in his report on the Modoc rock shelter from southern Illinois. This deep stratified site produced a range of eastern Archaic materials throughout its 26 feet of deposit and absolute dates ranging from 3657 B.C. to 7922 B.C. An important correlate of these dates is the fact that polished stone *is* present at the earliest surely Archaic level and dates to 6210 B.C. Fowler concludes that the Modoc finds extend the period of eastern Archaic culture back to at least 6000 B.C.; he sees these dates as support for and in turn supported by the dates on Old Copper. There is, however, no suggestion of a Plains Archaic connection with the Modoc materials.

In a recent description of an Early Archaic complex from the Upper Ohio Valley (Mayer-Oakes, 1955a) I have suggested that the typology of Panhandle Archaic projectile points is derived from late Paleo-Indian complexes. A subsequent site report and seriation study of these points (Mayer-Oakes, 1955b) indicates the western affinities of this Early Archaic shell-mound complex. Several geographers and ecologists (Borchert, 1950) have pointed out the existence of a "Prairie Peninsula" which extended eastward into Ohio, Pennsylvania and New York during the Post-Glacial Thermal Maximum (or Altithermal) about 6000 to 3000 B.C. This wedge-shaped corridor of grassland extending into the eastern Woodlands was a convenient ecological zone for exploitation by the western hunters when and if the high Plains became too arid for normal life activities.

Powell (1955) has recently suggested that we look for natural migration routes in attempting to explain movements of early men in North America. The glacial lake and Great Lakes waterways of the Northern Mississippi valley do form a logical northwest to southeast route which, at the time of the eastern grassland extension, would have increased the likelihood of movement from west to east.

This idea of a prairie peninsula is important in understanding the nature of the earliest Archaic unit in the Upper Ohio Valley. The complex of subsistence traits, based on a hunting emphasis, and tools, expressed in projectile-point styles, was brought in from the west via the prairie peninsula by units of the Plains Archaic. Contact with neighboring resident early Archaic units and adaptation to riverine life resulted in a local development of distinctive Archaic culture. This Panhandle Archaic continued to be influenced by northern Archaic units and gradually lost its western orientation. It changed, thus, from the most eastern component of the Plains Archaic to a unique local Archaic.

In addition to the various lines of recent evidence cited above which suggest that Archaic cultures are longer-lived than previously considered, there have also been signs in recent years that the Paleo-Indian cultures are quite varied in typology. Whereas the term has often been restricted to fluted-point units, C-14 dates and geological dating factors indicate that various non-fluted-point sites are equivalent in age to Folsom and that the general lanceolate-point horizon covers a large area of North America for a time span of several thousand years. I refer here to such items as the Scottsbluff and Angostura points found associated with mammoth remains in the Valley of Mexico (Aveleyra, 1955, 1956), the Paleo-Indian sequence at Blackwater Draw showing Clovis, Folsom and Portales units in stratigraphic succession (Sellards, 1952), the Montana stratigraphic sequence of points running from Folsom to Scottsbluff to Signal Butte II (Forbis and Sperry, 1952), the Lime Creek sequence of Scottsbluff to Plainview and the Red Smoke sequence of Frontier to Plainview (Davis, 1954), the early C-14 date on the Frontier complex (Johnson, 1951), the C-14 dates for Eden and Scottsbluff points at Sage Creek (Johnson, 1951), and the C-14 date on the Angostura type site (Johnson, 1951).

CONCLUSIONS

On the basis of the recent developments sketched above, we are beginning to see the eastern Archaic as a longer-lived and more broadly conceived stage of cultural development, having continent-wide relationships. The Paleo-Indian hunting cultures are seen as a specialized development partly preceding and partly contemporaneous with the earliest part of the Archaic period and related to Archaic units in at least one well-established way, as expressed in the concept of a Plains Archaic.

So far I have avoided qualifying the phrase "Plains Archaic." I have used it to imply both time period and stage as well as tradition. Originating as a simple space-time concept, it has developed both historical and developmental significances. At the present time the most reasonable use for the concept is as an historical continuum, comprising a tradition of basic hunting-gathering economy expressed materially in a restricted range of projectile-point styles and associated, though poorly-known, tools. Out of the various lanceolate-point styles present at the earliest part of the Paleo-Indian period, a small number were continued for lengths of time varying with the specific area. In general, the "unfluted Folsom" style and the broad-stemmed Scottsbluff style were carried to the north, east and south as Post-Glacial time and ecological changes went on. Contacts with resident local Archaic units resulted in complexes which can be explained and understood on the basis of this Plains Archaic tradition.

The Plains Archaic concept is thus seen to comprise a cultural unit of historical continuity over a period of perhaps 4–5000 years within the Plains and Prairies geographic areas. Roots of the tradition are in the earliest Plains Lithic complexes. There is apparently no fundamental change in basic hunting-gathering economy although there is probably a change in degree to which big game herds were utilized as a major means of subsistence.

In trying to apply their "Early Lithic" and "Archaic" developmental stages to the Plains area, Willey and Phillips recognized the dilemma posed by the available evidence. On the basis of this evidence, I think it is practically impossible to define or segregate, meaningfully, these two stages. The relationships among *all* Plains pre-ceramic units are too strong. However, by assuming that there was a development toward an Archaic stage, we can align some of the otherwise floating units into a schematic order based on this concept of a Plains Archaic, or more simply, a Plains *tradition.*

Perhaps the earliest antecedents of the Plains Archaic series would be the Frontier complex, followed by Scottsbluff and Plainview units. All these could be classed as Paleo-Indian in period, but since they are more generalized in typology than the fluted-point units they are the best basis for the general tradition. With the increasing popularity of a complex distinguished by Angostura points we are fully into the time period characterized by units of Plains Archaic tradition. Which tradition continues on as at Graham Cave, Starved Rock, Nebo Hill, Airport village, Havey site, and Signal Butte I.

The most eastward thrust of this tradition is expressed in the Panhandle Archaic of the Upper Ohio Valley, a unique combination of Plains projectile-point traditions and local seasonal adaptations to a riverine ecological niche. Interestingly enough, the crude Plainview-like lanceolate points seem to drop out of style by late Archaic times in the east, but the Scottsbluff-like Steubenville Stemmed points continue a favorite style, moving eastward to the Atlantic coast and forming in the Ohio Valley, the basic Early Woodland style.

SUMMARY

In the brief time allotted for this paper I have been able to do little more than sketch the development and present status of the Plains Archaic concept.

Origins of the concept are largely implicit in early excavation work on the Plains, but become increasingly overt and detailed in studies reported since 1948. The archeological finds which support the concept are pre-ceramic with primary typological similarities to Paleo-Indian complexes. The absence of geological, paleontological, or C-14 dating information in the early stages of use of this concept resulted in the conservative guess-dating of these finds. The presence of various elements known from eastern Archaic assemblages tended to reinforce this dating, but did not greatly help in understanding the cultural and historical situation.

Recent developments relative to the concept include a number of early C-14 dates, a deep stratified Archaic site, a unique eastern Archaic site of western flavor, as well as several syntheses which have tried to utilize the concept without examining it critically in detail. These recent developments indicate that, first, the cultures called "Archaic" in the eastern U.S. are longer lived than previously considered and are part of a broad culture base which spread over the New World at a time near the close of the Pleistocene; secondly, the cultures called "Paleo-Indian," while less varied in nature and content than "Archaic," are none the less varied in typology and cover a considerable span of time in a restricted area of North America. They are at least partly contemporaneous with the "Archaic."

The "Plains Archaic" concept helps us to understand the relationship between Paleo-Indian and eastern Archaic cultures—a relationship best expressed as a typological and economic tradition forming a cultural and historical continuum in a restricted geographic area.

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PREHISTORIC CULTURE SEQUENCES IN THE EASTERN ARCTIC AS ELUCIDATED BY STRATIFIED SITES AT IGLOOLIK

Jørgen Meldgaard

For the last few years there has been an increasing interest in the prehistory of arctic Canada and Greenland, and the problems attracting the archaeologists are those of the Dorset Culture, the "mysterious Eskimo Culture," as it was called recently. The basic problems stand out clearly: (1) Did the Dorset Culture emerge from a foundation represented by early flint sites in the Western Arctic, or are the roots to be traced in the south? (2) Is it possible sharply to distinguish from Dorset an earlier and specific culture complex in the Eastern Arctic? (3) Finally, and what is our main problem, How did the Dorset develop into the stage which can be termed "The classical Dorset," an adaptation to the environment that deviated so remarkably from other Eskimo cultures? And which were the forces generating this evolution and causing the final decline of the culture?

During the joint Danish National Museum–University of Pennsylvania Expedition in 1954, we succeeded in collecting material which may help in answering these questions. The field work was carried out in the Igloolik area 400 miles north of Hudson Bay, from May 19th to September 21st. The party included three men, besides myself, Richard Emerick from the University Museum, and Father G. Mary-Rousseliere of Churchill.

The Igloolik area turned out to be extremely well suited for chronological studies. This flat, heavily eroded limestone country exhibits very marked features derived from the postglacial marine submergence. The upper marine limit is approximately 600 feet in this area. Raised gravel beaches are visible in practically all places. Around Igloolik the Thule Culture sites were found to be situated on raised beaches from 3–4 metres to about 8 metres above present sea level, as already observed by Therkel Mathiasen during his pioneer work in 1922–23. In 1954 we located 8 Dorset sites and 4 Pre-Dorset sites (which shall here be termed Sarqaq sites) on levels from 8 to 54 metres, all of which can be placed with certainty in the chronological "schema" formed by the system of elevated beach lines. I shall here restrict the description to the two most instructive localities: Alarnerk and Kapuivik.

Alarnerk is the name given to a summer camp of the present Igloolik Eskimo on the northeast corner of Melville Peninsula. Over an area of 3 sq. kms. 208 rectangular depressions were found and all determined to be Dorset houses. This number of house ruins probably makes Alarnerk the largest archaeological site known in Arctic Canada or Greenland. However, these houses were distributed on a large number of raised beaches from 8 metres to 22 metres above sea level, and I have chosen to divide Alarnerk into five zones, each intended to represent one stage or period, since the material collected shows a gradual development of the culture from the 22 m.-terrace down to the latest traces at a height of 8 metres. The time span revealed at Alarnerk seems to include all the Dorset manifestations of which we have knowledge at present. Therefore, being able to characterize each of these five zones statistically and typologically, I have used the terms Period I to Period V.

House ruins from the Dorset Culture are scarce. In fact, only one real house has previously been attributed to this culture, namely the Mill Island ruin partly excavated by Junius Bird in 1927, and later completely excavated and published by O'Bryan. The interesting, rounded dwelling type found by Eigil Knuth in Peary Land and in northeastern Greenland is difficult to classify, but is probably to be considered as a tent foundation. The Dorset house as revealed in the Igloolik area is rectangular from the earliest to the latest period, its size being normally 5 × 4 metres, though a number of very large houses are found quite early, in Period III; they measure up to 14 × 7 metres (45 × 23 feet). Small open fireplaces are found on the floor, and low benches are indicated along the walls. No entrance passage can be traced, but the house room is dug down to a depth of as much as half a metre below ground level. The character of the walls and the roof cannot be established with certainty; only low gravel ridges are left of the walls, except in the latest periods where stones often line the inner side of the wall. Not until the last period, where also the Thule culture appears on the stage, are there apparently any radical changes in the construction of the house. Probably from these immigrant whale-hunters they learned to build an underground entrance passage, the cold trap, a trait frequently seen in the late Dorset houses. Also the house room is altered, being smaller and very narrow with small side rooms for the fireplace and for storage, and the practice of constructing one raised bench along the end wall is adopted. An interesting detail in several of the earlier houses was the finding of skulls of the bearded seal just below floor level. In eight cases they were found unbroken and placed two and two close together, always placed exactly parallel to one of the axes of the house. A likely interpretation is that they were offerings.

Alarnerk revealed for the first time the graves of the Dorset culture. Three different types were identified, but here the height above sea level gives no clue to their relative chronology. Only a few remains of human skeletons were found. Unfortunately most of the graves had been disturbed, apparently being reused as meat depots. The massive stone cist of the Thule Culture, erected on the surface, is most probably the prototype for what seems to be the latest form of the Dorset graves: a short rectangular chamber of heavy stones paved with flat stone slabs. Older than the Thule contact is a rectangular or rounded pit dug down in the gravel and edged with a few stones. The artifacts found in these graves were apparently thrown down in no sort of order, and several of them differed from those found in the house ruins, having a particularly fine decoration, but also often being broken or unfinished objects. The third, and possibly oldest, type was represented by only one untouched grave. A gravel mound two feet high covered the complex burial, which included a 2½-feet-deep pit with a few scattered pieces of grave goods, the mandible of a child and a long bone of an adult. Next to this pit and on the original surface was a square area approximately 4 × 4 feet where a small fireplace with ashes, burned animal bones, and red ochre was found in the center. Close by the fireplace lay the maxillary bones of a walrus in connection with carefully halved walrus tusks and a number of rib bones. Several artifacts were placed along the edges of this square area, the most interesting being a rather decomposed shaft of wood 26 cm. long with unworked microblades lying parallel along each side. Although no side grooves can now be distinguished in the wood, there can hardly be any doubt as to the original character of this artifact: it was provided with two rows of sideblades.

The earliest form of the Dorset Culture, as found in Alarnerk Period I, was a surprise in itself. On the basis of the material hitherto known, two features, or guide forms, had been emphasized, the presence of which should immediately identify the culture: the characteristic small harpoon heads with rectangular shaft socket, and the flint industry with a number of characteristic forms. But instead of these there lay, in the houses and middens, some earlier unknown forms, large harpoon heads and lance heads with open or partly closed shaft sockets, and blades of ground slate dominating the flint artifacts. It is, however, clear enough that these remains represent the Dorset Culture, as on the terraces which follow on the lower levels the various types show an unbroken development, through which flint gradually supersedes slate and the typical artifact forms emerge. Relatively few traits disappear completely. Period III must be considered such a transitional stage. The material, or in other words the development, goes in the direction of what can be termed classic Dorset. The terraces from 14 metres to 11 metres revealed this well-known complex of artifacts, here comprised in Period IV. In this period we meet a type of culture completely adapted to the hunting of sea-mammals, specializing particularly on the walrus. An elaborate ice-hunting pattern seems to have developed along these ice-bound coasts, including probably the snow house later to be taken over by the Thule Culture and their descendants, the present-day Central Eskimos. In the final Period V, where several Thule Culture artifacts are found in the Dorset houses, a change towards a colder climate is indicated. The preservation of a number of pieces of wood may mean that the thaw now did not penetrate so far down in the soil, and the various new traits in the house construction, including the cold trap, are features which are more than merely chance borrowings from among the new fashions which in this period appear with the Thule people.

A sample of antler from a house on the 8-metre terrace (K'mersut, 8 m.-house 1) has been dated by Tauber in the C-14 laboratory in Copenhagen. The age given is 600 years ± 150.

Another site of particular interest was discovered on Jens Munk Island off the Baffin Land coast at Kapuivik. In this pre-Cambrian area larger stones were available for house building, and the 48 Dorset houses found here were generally in a better state of preservation than those on Melville Peninsula, including Alarnerk. The houses were distributed on terraces from 8 to 23 metres above sea level, corresponding closely to Alarnerk, and the artifacts found further support the culture stages observed at Alarnerk. Surveying the levels above the 23-metre terrace, a number of stone arrangements and small tent rings appeared on terraces from 38 to 52 metres high. The stone artifacts found here are closely related to the West Greenland Sarqaq Culture, and none of the specimens can be duplicated in the material from the Dorset. Although three more Sarqaq localities were found during the field work, the material is too small to permit a division into "stages"; still the time-span represented by a total of 24 metres of rise during the time of the Sarqaq Culture probably exceeds the 15 metres in Dorset time. The stone artifacts from the Sarqaq sites are all made of hard silicified stones, mostly flint, and there is no slate. The dominating type of artifact is the burin. Small symmetrical end-blades are common, as are tanged asymmetrical blades. Microblades occur, but in small number. Unexpectedly, the bone material was also preserved. Three toggle harpoon heads were found, all made of antler, and with an open shaft socket. Two of them have one slender barb; one has a deep slit for a long end-blade. Being probably the oldest Eskimo harpoon heads known, their closest parallels, it is somewhat surprising to find, are among the Birnirk harpoon heads with simple divided spur. Slender flint flakers or rib bones and ivory needles with round eyes are common, and very distinctive from the corresponding Dorset types. Judging from the presence of small symmetrical end-blades of flint and from fragments of slender bone shafts, the bow and arrow were known, a trait also ascribed to the West Greenland Sarqaq Culture. In Dorset we have no evidences for the use of the bow.

A sample of antler from a 51-metre terrace has been dated by C-14 in Copenhagen, and the age given is 3700 years ± 300. This sample being taken from a stone ring only 3 metres below the earliest remains probably gives a date very close to the beginning of the Igloolik stage of the Sarqaq Culture.

I have here used the term "Sarqaq Culture" for the pre-Dorset remains in the Igloolik area. However, this does not mean that the finds are identical with the material known from the Sarqaq Culture of West Greenland. A rather important difference, in fact, is the lack of microblades in West Greenland. The relation to the early stage of the Paleo-Eskimo culture in Peary Land is probably closer. The stone industry found by Giddings at Knife River near Churchill in the Hudson Bay region is also without microblades, yet the specimens from this side are even more similar to those of the Igloolik stage than any in Greenland. However, they all share traits with which we are familiar in the western Arctic and in the Siberian mesolithic, and they all stand out clearly from the younger Dorset Culture. As the Dorset exhibits a variety of stages, not only in time but evidently also within the large area of the Eastern Arctic, so do the pre-Dorset manifestations. In order to avoid formulations of a number of pre-Dorset cultures, I prefer to use terms like the Igloolik stage, and the West Greenland stage, of the Sarqaq Culture.

Having here emphasized the western affinities of Sarqaq as contrasting to Dorset, I am in opposition to the predominant view of the origin of the Dorset Culture. The western parallels in Dorset have especially been advanced by Henry B. Collins. In his recent publication on the T-1 site at Southampton Island he has summarized the typological resemblances between the early Dorset on one side and pre-Dorset sites in the Eastern Arctic and early flint sites in Alaska-Siberia on the other side (*Anthropological Papers of the University of Alaska*, Vol. 4, 2, 1956). Before dealing with these traits I want to suggest the term Proto-Dorset for the T-1 site should be given up. Although the material differs from the classic Dorset it seems to be identical in all respects with the Dorset found in the Igloolik area in the second and third periods, and therefore a transitional stage within the Dorset Culture proper.

The occurrence of burins in Dorset has been stressed as the most obvious parallel to the West. However, there are apparently no true burins in Dorset. In the very large collection from T-1 only one specimen is said to exhibit the typical negative bulb of percussion, and even this specimen seems to be an atypical form. In Igloolik there was not a single true burin in any of the Dorset levels.

One distinctive type in the T-1 inventory, what is called the "triangular microlith," is, however, said to show the burin technique. Since this particular type is also being paralleled to some of the Mesolithic microliths from Eurasia, I shall here attempt a revised interpretation based on the Igloolik material. In T-1 this slender triangular blade is described as a highly specialized type not previously reported from America, and as the most important and characteristic implement of the T-1 site, next to the microblade. They are divided into six general categories. At the Igloolik sites this type also appeared proportionally most frequent on the earlier levels, and completely absent in Periods IV and V. Now, another type disappearing at the same time is a variant of the typical triangular, concave-based end-blade, a harpoon blade. This variant is characterized by the fact that one side has no chipping or very little, but a ridge goes from the point to the base. The majority of the triangular end-blades from T-1 are said to have the bulbar surface unmodified or only partly worked. This plain side, however, is probably, like the Igloolik variety, not unworked but reworked, and what is removed is simply the triangular microlith under discussion. The end-blade has first been chipped equally on both sides, like the ordinary type, whereafter two long flakes were pressed off from the pointed end on the same side, each removing approximately one half of the chipped surface. This is a fluting technique parallel to the fluted blades of the Folsom type. Since by this process a few millimetres of the larger part of the edges were removed, these "sharpening blades," as I shall prefer to call them, exhibit a peculiar cross-section. The first sharpening blade removed has a flat triangular cross-section: on the upper side the original chipped surface of the end-blade, on the reverse side a facet revealing the former chipped edge of the under side of the end-blade, and finally the bulbar surface. Sharpening blade no. 2 has in its typical form an almost rhomboid cross-section, being provided at the pointed end with an extra, narrow facet derived from the scar left on the end-blade after sharpening blade no. 1 was removed. This facet on blade no. 2 is apparently what is suggested as a burin scar on the T-1 blades.

The fluting technique is of interest in several respects. It is a most ingenious way to produce two sharp, even, and strong edges on a pointed blade. But I also ventured into this rather detailed and long description because this technique indicates southern affinities of the early Dorset. In the Dorset material from Newfoundland I have found that 60–65% of the triangular end-blades are of this fluted variant, and the sharpening blades are equally frequent. This is an even higher proportion than in the early periods of the Dorset in Igloolik, and, since the use of this technique gradually diminishes in importance as one goes farther away from Newfoundland (in Greenland it was never used), it seems rather evident that the origins of these fluted blades must be looked for to the south.

From the T-1 site a smaller group of straight-sided blades with the outer surface carefully worked is also emphasized as being closely similar to side blades from Mesolithic and Early Neolithic sites in Siberia and Mongolia. In Dorset Periods II and III in Igloolik a few apparently corresponding blades are fluted from larger double chipped blades in a way very similar to the triangular sharpening blades described above.

A group of microblades in T-1 is termed "backed blades" and compared in a general way with Old World Paleolithic and Mesolithic. These heavy triangular-sectioned blades exhibit one thick and blunt edge which in most cases probably is a part of the prepared surface of the microcore. Consequently this backed blade can hardly be considered a special type and added as an Old World trait. However, the microblade industry as such *may* very well be an element in Dorset that can be traced directly through the Arctic to the West. Despite the rather general occurrence of microblades in North America, the distinct prepared-core type in Dorset and the Sarqaq stage in Igloolik and Peary Land strongly indicate a connexion. Thus apparently there was an association between a late Sarqaq stage and the early Dorset. Also the existence of a burin-like tool in the Dorset with the same function as the true burin may be explained by such a connexion. The basic sources of the Dorset Culture, however, must be searched for to the south. The presence of a southern or Indian stamp in the Dorset has been discussed before; in fact, it was referred to by Jenness as early as 1925. But the general view as to these connexions has been that expressed in Martin, Quimby and Collier's textbook of 1947. The Dorset Culture influenced some of the early cultures in the New York area (Frontenac and Laurentian) and perhaps some early cultures in the Wisconsin–Minnesota area. One of the traits emphasized was the ground slate blades. But, as pointed out by W. A. Ritchie (1951), the C-14 dates of these southern cultures exclude the theory of the Dorset Eskimo being the donors of the ground slate industry, since this slate was supposed to have been introduced in the latest stage of the Dorset from the slate-using Thule Culture.

Now since the earliest Periods of the Dorset at Igloolik surprisingly reveal that the Dorset was initiated as a slate-and-flint industry we are again within the time-span of the southern cultures in question. But we are not far enough back in time to make it possible once again to claim the Dorset people as the donors.

Some of the traits in the early Dorset that now exhibit a southern stamp are the following: Among the slate implements especially the symmetrical side-notched blade and the four-sided awl, and among the flint blades slender, symmetrical, side-notched points. The soapstone vessels are in the first two Periods rectangular with sloping sides, probably copied from wooden vessels. The house type, and the pit graves with ochre can also be considered here. I may add to this Igloolik material the evidences from Newfoundland, which I suggested revealed an early stage of the Dorset, and where there are strong indications that heavy wood-working tools like adzes and gouges are an integral part of the Dorset Culture.

Generally speaking, the early Dorset "smells of forest." More specifically it *is* possible to point out parallels in late Archaic and early Woodland cultures along the southern limits of the coniferous forest belt, including "The Old Copper Culture." There is not time here to go into detail as to these parallels, and without a presentation of the full inventory of the early Dorset such an attempt would take the form of postulates. It is not impossible that a copper industry was the source of the Dorset Culture; I shall venture to say that it is even probable. It is my impression that not only some slate implements but also artifacts of bone in Dorset may find their counterparts in copper; even the bone and ivory lances with partly closed shaft socket could have been copied from lance heads of copper like those of the Old Copper Culture with bent flanges for a socket.

Birket-Smith proposed the existence of a wide-spread boreal, proto-Eskimo ice-hunting level of culture, in part Eurasian, in part North American in origin. Spaulding later suggested that this level in particular is a representative of a boreal complex which strongly influenced the Laurentian. I do believe the Dorset Culture supports this view, but also that it is necessary to accept the theory that out of this ice-hunting horizon emerged not only Eskimo cultures in the Western Arctic. From an eastern manifestation developed a distinct culture which crystallized in Dorset. What we find is apparently a culture which along the arctic coasts of Canada grew more and more Eskimo-like, an adaptation where environment was the main factor, although the evolution to some extent may have been directed by influences from the Western Arctic represented by the Sarqaq Culture.

This paper has been dealing with culture sequences. It is, however, possible to present a more complete picture of the Dorset people since the last stage of their prehistoric culture is in part "history," although the last Dorset man lived some 600 years ago. Among the legends of the Central and the Eastern Eskimos there is a well-known group of traditions which has a historical core; the tales of the Tunit people, and it was an important additional result of the field work in 1954 to give a new identification of the Tunit. We know of this group of tales from throughout the eastern Eskimo area including Greenland, and the Tunit, originally often interpreted as Indians, are now considered to have been the people of the Thule Culture. But, faced in Igloolik with a number of additional illuminating stories and descriptions, it became obvious to us that the Tunit had nothing to do with the Thule Culture—they were the Dorset people.

The Eskimo generally denied, first and foremost, that the Tunit had lived in the large houses of stone and whalebone which belong to the Thule Culture; these were the houses of their own forefathers. We were told consistently that the dwellings of the Tunit were rectangular in outline and that the wife sat by an open fireplace within the doorway and prepared the food, and that they lay on a bench which was so short that they had to rest their legs vertically up the wall. Various sites could still be pointed out where the Tunit had left remains of their buildings, and where one could see stone chips from the manufacture of their weapons—and they were Dorset. The Tunit were good caribou hunters, and could bring down the beasts on foot, armed only with a spear. But during the winter they caught seals at the breathing holes in the ice. And, as nowadays, the time of waiting could be long, and therefore to keep warm the hunter brought with him a little round stone lamp, which he set up before him on a little elevation of snow. By leaning forward he could let the edge of his coat fall over the lamp, thereby forming a sort of little tent in which the temperature soon rose and warmed his body. Moreover the Tunit often had scars of burning on his belly, for in his eagerness to harpoon the seal when it eventually appeared, it happened not infrequently that the hunter forgot about the lamp! Another important game was the walrus, and here the legendary strength of the Tunit was a great help, not least in the transport back home; for they had no dogs, but only a short sledge which they pulled themselves. The island Uglit south of Alarnerk was the last domicile of the Tunit; and often they had to go and catch the walrus far away on the sea ice. Therefore, despite their strength, they were tired when they approached Uglit. But then the women only had to appear outside the houses to bring the strength back to the hunters—for the Tunit were very fond of their women. "Tunit were driven away by our forefathers, sometimes after fights. Our forefathers loved to fight, like the white man," related an old Eskimo named Anangoar.

The people emerging from these tales are in perfect agreement with our archaeological Dorset specimens. But they do appear more alive, and more human, although to the present-day Eskimo they are to be grouped together with the white man as the offspring of a dog.

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ARCHAISM AND REVIVAL ON THE SOUTH COAST OF PERU

Dorothy Menzel

Recent studies of late prehistoric pottery styles of the South Coast of Peru have brought to our attention some examples of cultural archaism, phenomena which are of considerable theoretical interest to archaeologists and students of culture change. Archaism is defined as the deliberate attempt by a later people to imitate or revive features of culture of an earlier period. Such attempts may differ widely in motivation and in their general effect on the respective cultural features in question. Present studies have revealed at least two examples of archaism which show marked differences in kind.

Art historians have noted the existence of archaistic and revivalistic movements in so many instances in the Old World that one gets the impression that the phenomenon is not an unusual one. Some of the best-known examples are those of Neo-Babylonian Mesopotamia and Saite Egypt in the sixth century B.C., a period in which art styles and religious ideas of some 2000 to 1300 years earlier were revived. Another well-known example is that of the Hellenistic period in the Mediterranean, in the first century B.C., during which there was an archaistic taste among art collectors leading to the imitation of sixth and fifth century Greek art. During the Ming dynasty in China in the fifteenth century A.D., after a period of Mongol rule, there was an attempt at revivals of bronze art of the Chou and T'ang dynasties, respectively 2000 and 800 years earlier. However, such archaistic revivals are not necessarily confined to art styles, or to only one aspect of culture. The Renaissance in Europe was in part an archaistic movement on an immense scale, affecting first art and literature, then philosophy and science, and finally even political theory. Although these phenomena are known and have to be studied mainly from archaeological material, archaeologists have paid little attention except as they furnish one more dating problem. It is time that we started looking at them as problems in culture history as well.

One of the places in the New World where examples of archaism are most striking is prehistoric Peru. As early as 1925, A. L. Kroeber recognized the fact that the Late Chimu style of the North Coast of Peru revived many features of the earlier Moche style, after a period during which foreign influences had virtually wiped out the local tradition. In the recent studies of South Coast pottery styles of the later periods, it has been possible to study several examples of archaism in very close detail.

One type of archaism present in the late styles of the Ica valley is a form of antiquarianism involving the collection and imitation of antiques. It had a particular vogue during the period of the Inca occupation in the late fifteenth and early sixteenth centuries, but the phenomenon is known from much earlier times on the South Coast as well. The earliest evidence of a taste for the antique has been reported by Lawrence E. Dawson for the later phases of the Nazca styles of about the eighth or ninth centuries A.D. Some 400 years later, antique specimens appear in burials of the Chulpaca style of the Late Intermediate period. Two burials with pottery of this style in the Museum of Anthropology at Berkeley contained also one Nazca vessel each. The Nazca specimens are definitely ancient pieces and not later imitations, and the Nazca style had no traceable influence on the styles of the Late Intermediate period. It is interesting that no attention is paid to the Nazca style in the archaizing activities of later periods. The taste of later collectors seems to have run exclusively to Epigonal and Chulpaca art which developed from a tradition foreign to the South Coast.

The Inca-period vogue of antiquarianism which revives post-Tiahuanacoid styles first appears at a time just preceding the Inca conquest of the area. A grave-lot of this period (Late Soniche) included one jar, in the Epigonal style of about 300–400 years earlier, which is so authentic that it is presumably another reused ancient specimen. Subsequently, during the Inca occupation, collectors' items and later imitations of Epigonal and Chulpaca pottery represent six percent of the pottery found in burials of that time. While one or two vessels may be reused ancient specimens, the majority of the specimens are copies of ancient prototypes with varying degrees of faithfulness. In most cases elements in shape features and design techniques betray the later origin of the vessels. With the end of Inca administrative control in the sixteenth century came an abrupt change in the local style, and the antiquarianism of the Inca period largely seems to have gone out of fashion.

The antiquarianism just described has in common with the examples from the Old World cited earlier that it revives stylistic elements of much earlier times not in the memory of those living. An interest in the antique is traditional in this area. It probably had its origin in the occasional accidental discoveries by the later inhabitants of earlier specimens, perhaps uncovered through the disturbance of earlier burials by natural or human agencies. Such discoveries must have been made at all times, but the antiques clearly proved of greater attraction in some periods than in others. Imitations, in particular, seem to be confined to certain periods. An important distinction of this type of antiquarianism in the Ica valley is that it does not affect the contemporaneous style as a whole but is restricted to isolated specimens.

A second type of archaism occurs in Late Ica pottery in the early Colonial period which differs from the earlier antiquarianism just described in that it revives on a large scale features of one particular style of the recent past. There is very clear evidence that this early Colonial archaism is a nativistic revival, the result of a reaction at Ica against the Inca occupation which preceded it. To date virtually the only nativistic revivals which have come to the attention of anthropologists have been described by ethnologists, such as the Ghost Dance of the American Indians or the Cargo cults of the South Pacific. It is a valuable addition to our knowledge to be able to document such a movement from archaeological evidence as well.

The early Colonial or Tajaraca B pottery of the Ica valley represents a wholesale reversion to the local style which immediately preceded the relatively brief Inca occupation period. Its main distinguishing feature is the fact that no vessel found is free from its stylistic effects, in contrast to the archaism described earlier. This revival coincides with an almost complete loss of features of Inca origin; both such features and local stylistic features which are closely associated with the Inca occupation are pointedly ignored. The emphasis which dominates the revival is the reversion to the Soniche style, the last fully standardized style which preceded the Inca occupation. However, although a few vessels come very close to being perfect replicas of their Soniche prototypes, such close replicas are extremely rare, and the majority of vessels have some characteristic which betrays their later origin. This is due to the fact that changes in the local style independent of Inca influence, which had taken place during the period of Inca occupation and for a brief interval preceding it, persisted, though in somewhat altered form. Traces of such changes are found in combination with classic Soniche features as well as with additional innovations in patterning and design. The result is a distinctive new ware characterized by a number of stylistic anachronisms.

Many of the revivals of classic Soniche features are amazingly faithful. Such accuracy undoubtedly was possible because of the shortness of the intervening period. The Inca occupation lasted about sixty years. There must have been enough older people living at the time of the Spanish conquest who remembered the art of their youth or had close contact with those who did, so that there was direct continuity of artistic experience to supplement the heirloom specimens available as models.

Information on the historical background of the revival, based primarily on archaeological evidence, contributes a good deal to our understanding of the circumstances which led up to this early Colonial reaction at Ica. In the first place, on the basis of studies of pottery styles from a number of South Coast valleys, we can make some inferences about the status of the Ica valley among its neighbors in the period preceding the Inca conquest. Ica clearly had a position of prestige as regards artistry in pottery making. This prestige manifests itself in the amount and kind of stylistic influence which Ica pottery had on that of neighboring valleys. Such influence is discernible to the north at least as far as Chincha, and to the south in the Nazca drainage, at Acari and at Chala, over an area of about 500 square kilometers. The evidence also indicates that the artistic prestige of Ica originates with the Chulpaca style, some 200–300 years before the Inca conquest, and is found with increasing strength through successive Ica style phases up to and including the Inca period. Not only do elements of the Ica styles strongly influence the styles of adjacent valleys, but, in addition, trade pieces or local imitations of Ica ware are found in conspicuous percentages side by side with vessels of the local styles. In contrast, no comparable influences from neighboring valleys are found at Ica, where stylistic change is based predominantly on local development unaffected by outside influences. This is particularly true of the Soniche style which served as the basis for the Colonial revival. Compared with the earlier styles, the Soniche style reaches a high point in homogeneity and standardization, and foreign influence is virtually nonexistent.

Unfortunately, the studies to date are based almost entirely on pottery, and consequently it is not known whether Ica prestige extended also to other forms of artistic expression. However, it is known from historical sources that in terms of political and military power and size of population Ica was not important in the area in which it exerted artistic influence. Early chroniclers agree that in these respects the Chincha and Cañete valleys were more important at the time of the Inca conquest and up until the advent of the Spaniards. Ica prestige, then, was based on artistic achievement rather than political power, and pottery was either the main, or at least an important, expression of that achievement.

The history of succeeding political events, starting with the Inca conquest, is another important aspect of the antecedents which led up to the Ica revival. Inca conquest techniques are known from historical sources and have been described by Rowe in the Handbook of South American Indians.[1] The Inca pursued a policy of enlightened self-interest designed to establish complete political control of a large and well-functioning empire. Their policies were ruthless, uncompromising and sweeping in their aim to establish such control, but they also brought efficient administration and material well-being to the provinces as an important means to this end. A conspicuous aspect of the conquest program was the manipulation of cultural symbols standing for the national self-consciousness of conquered peoples. The Inca attempted to divert these symbols from representing exclusive pride in local tradition to representing pride in membership in the Inca empire. Historic sources give us only sporadic glimpses of the effect of this manipulation on the affected peoples. These sources do show that the policy affected many aspects of culture, including dress, architecture, religion and local leadership, but the information is rather general and selected; it is here that the archaeological data provide important additional information on how the program was carried out. In the first place, pottery studies demonstrate that on the South Coast, at least, this Inca policy was extended with particular emphasis to pottery art as an important symbol of local distinctions. In all the valleys for which studies have been made, the Inca conquest brought fundamental and arbitrary changes to local pottery styles. At Ica, in particular, new categories of pottery vessels became important symbols of new social distinctions and prestige associated with the Inca. The new wares have very little in common with the local tradition and are primarily composed of arbitrary combinations of Inca elements and local innovations. These new types clearly originate at Ica but have a wide distribution, specifically in the former sphere of Ica prestige on the South Coast, and sporadically as far away as the highlands. No comparable prestige ware emanates from the other South Coast valleys, and it is evident that the local prestige pattern continued under the Inca domination, though it was divorced from its former associations. Pottery of the local Ica tradition continued side by side with the new wares, but the evidence is that it was relegated to a position of inferiority in relation to the latter and to Inca pottery. Thus, the sum of historical and archaeological evidence shows that the Inca conquest interfered in a number of ways with local pride and independence, although it did not bring economic or general physical hardship to Ica.

The advent of the Spaniards to Peru in 1532 brought with it the destruction of the Inca administrative system as a functioning unit and the dissolution of local controls. Subsequent civil wars and chaotic conditions prevented effective administrative control by the Spaniards until 1570, when the Spanish administration began to enforce residence by Indians in certain designated towns under the so-called "reduction" policy. Thus, during a period of about forty years, there was a relaxation of controls over dominated peoples, and it was during this period that the Ica revival took place.

The above historical and stylistic observations readily reveal the nature of the Ica pottery revival. It was unquestionably a reaction to the Inca occupation, based on frustration rather than extreme hardship. The primary point of sensitivity at Ica involved local pride in prestige based on pottery artistry of some two to three hundred years' standing. Ica prestige did not diminish in this respect under the Inca, but it became second to foreign prestige wares, such as Inca and North Coast pottery, and a symbol of Inca domination. It is thus not the loss of prestige but rather the alteration of its symbolic associations and status which caused the reaction at Ica. It is no accident that the nativistic reaction manifested itself so emphatically in pottery, since pottery was a major symbol of local pride and independence in pre-Inca times.

It is of considerable interest that no comparable revival occurred in the neighboring South Coast valleys in the instances in which the pottery could be studied in detail. Early Colonial ware at Chincha, Acari and Chala continued without noticeable changes from that of the Inca period. Archaeological evidence shows that pre-Incaic pottery in those valleys had a secondary prestige position in relation to Ica and so would not have been suitable symbols of local pride. This does not preclude the possibility that nativism could have manifested itself in some other form in those valleys. Surface collections in the Pisco and Nazca regions to the immediate north and south of Ica turned up one revivalistic pottery vessel from each valley. The characteristics of these specimens are such that they indicate Ica influence rather than an independent local revival. Their presence in those valleys seems to be the result of a continuation of the historic prestige pattern in the area. It is quite possible that, had South Coast cultures not been destroyed by Spanish domination, Ica influence might have continued to grow in the old pattern and eventually again might have influenced pottery development in the neighboring valleys.

As may be seen, the above examples of stylistic revival, as studied from archaeological data, are capable of revealing a good deal of significant information about the culture history of the area. Aside from pointing to their historical significance, we can also emphasize the special difficulties they create for archaeological interpretation. They produce a situation where degree of similarity cannot be used as a basis on which to infer nearness of time, a type of inference commonly made by all archaeologists and which usually works reasonably well. In such cases only careful data on associations in the ground and on stylistic antecedents make a reasonable chronological arrangement possible.

Berkeley, California.

Notes

1. John Howland Rowe, Inca Culture at the Time of the Spanish Conquest, in *Handbook of South American Indians*, (The Andean civilizations), Julian H. Steward (Ed.). Smithsonian Institution, Bureau of American Ethnology, Bull. 143 : 183–330. Government Printing Office, Washington, 1946; pp. 272, 273.

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THE PRECLASSIC CERAMIC SEQUENCE OF HUAPALCALCO, OF TULANCINGO, HGO

E. Florence Jacobs Muller

The archaeological zone of Huapalcalco is located at Km. 140 on the highway of Mexico City, D.F. to Tuxpan, Ver., and 4 kms. to the north of the city of Tulancingo, Hgo.

It is situated on an ancient shore of a lake which has almost disappeared. All that is left is represented by the lake of Zupitlan. During the rainy season the valley of Tulancingo suffers disastrous floods, as the two rivers which join together a little above the city of Tulancingo, are not sufficient to take care of all the waters which pour down from the hills of the south and western part of the valley.

This region shows signs of very active volcanism in the past. The southern portion of the valley is covered by a lava flow, which is called the "Pedregal of Santiago." On its northwest tip lies the archaeological zone of Zazacuala. At Km. 144 of the highway of Tulancingo to Tuxpan lies one of the two quarries of obsidian to be found in the state of Hidalgo. The obsidian from the quarry of Km. 144 is of a very bad quality. It is full of air bubbles. The color ranges from dull black to gray pearl, and sometimes a nugget of red lacquer shade is found.

Yearly explorations have been made in the Valley of Tulancingo since 1953 as part of the program of the Dept. of Prehispanicos of the "Instituto Nacional de Antropología e Historia" and in charge of Dr. César Lizardi Ramos and the author of this paper.

The zone consists of eight groups of mounds. Starting with the Tulancingo Honey Railroad, groups I and II are located. Crossing the highway in direction of the hills of "La Mesa" and "El Huistli" groups III, IV and V are found. Towards the north, crossing the small gorge which descends from "La Abra" is group VI. On the summit of the hill of "La Mesa" lies group VII and on "Huiztli" group VIII is found. (See Fig. 1.)

The superficial collection of sherds made in November of 1953 indicated that the oldest human settlement was to be found around groups IV and VI.

During the season of 1954–1956 stratigraphic pits were dug. Two types of pits were excavated: the natural in the rubbish heaps located at the edges of the mounds and patios, and the artificial in the mounds themselves. Natural stratigraphy is represented by pits No. 1 to No. 20, and No. 23. As can be seen in the plan No. 16 is located in the terrace east of M-6 and No. 16-A to the southeast of this. No. 18 was excavated in the patio near the road to the southwest of M-6.

The artificial stratigraphic pits were excavated in the buildings of group I and II and pit No. 21 in M-VI, which was dug to one side of the violation made by the treasure hunters. This was a tunnel dug from the outside of the eastern portion of the mound and went toward the center of the mound and in depth it reached the flooring No. 8 (See Fig. 2).

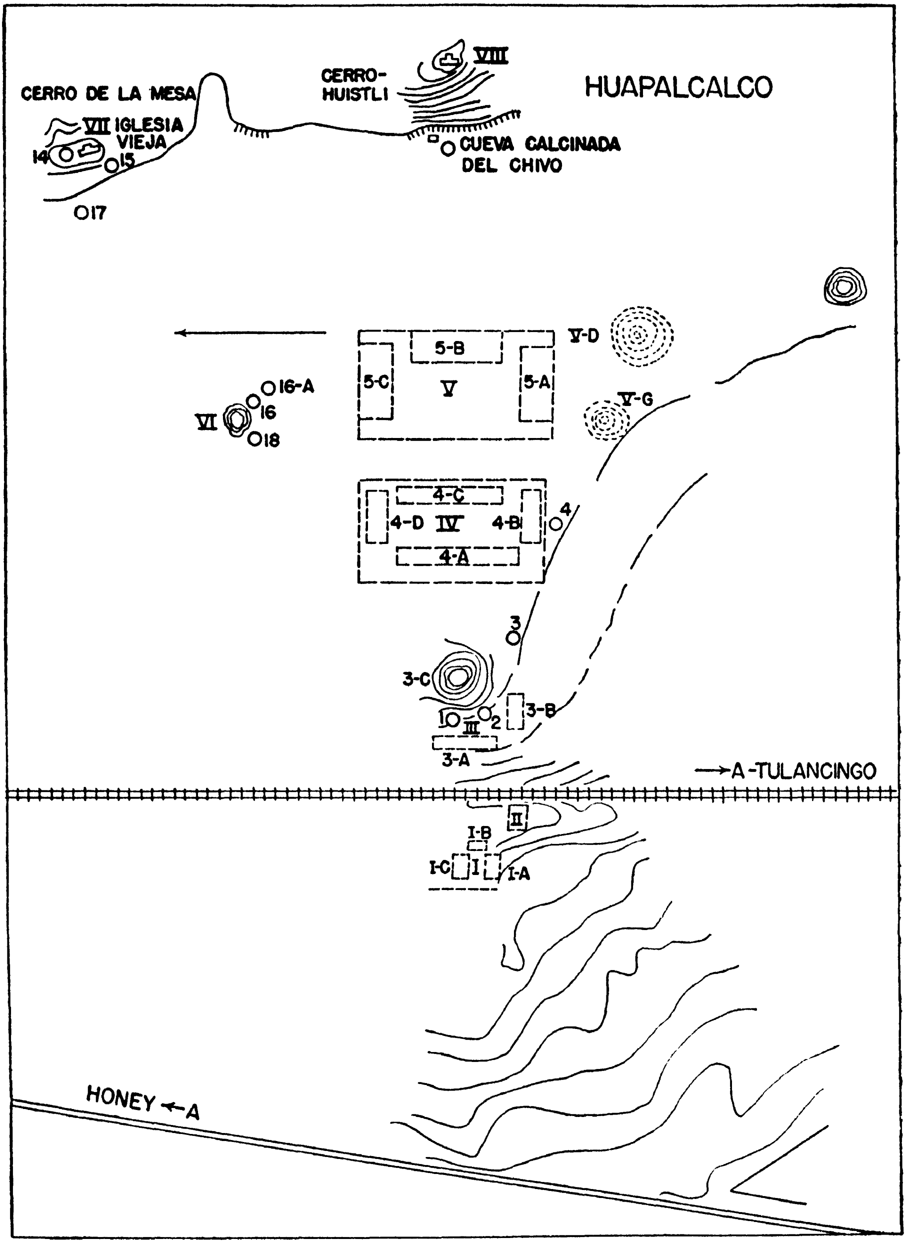
CERRO DE LA MESA
VII IGLESIA VIEJA
14
15
17
CERRO-HUISTLI
VIII
HUAPALCALCO
CUEVA CALCINADA DEL CHIVO
V-D
5-B
5-C
V
5-A
V-G
16-A
16
VI
18
4-C
4-D
IV
4-B
4
4-A
3
3-C
3-B
1
III
2
3-A
→A-TULANCINGO
II
I-B
I-C
I
I-A
HONEY ←A

Fig. 1. Plan of the Archaeological Zone of Huapalcalco, Hgo
Numbers encircled represent the stratigraphic pits

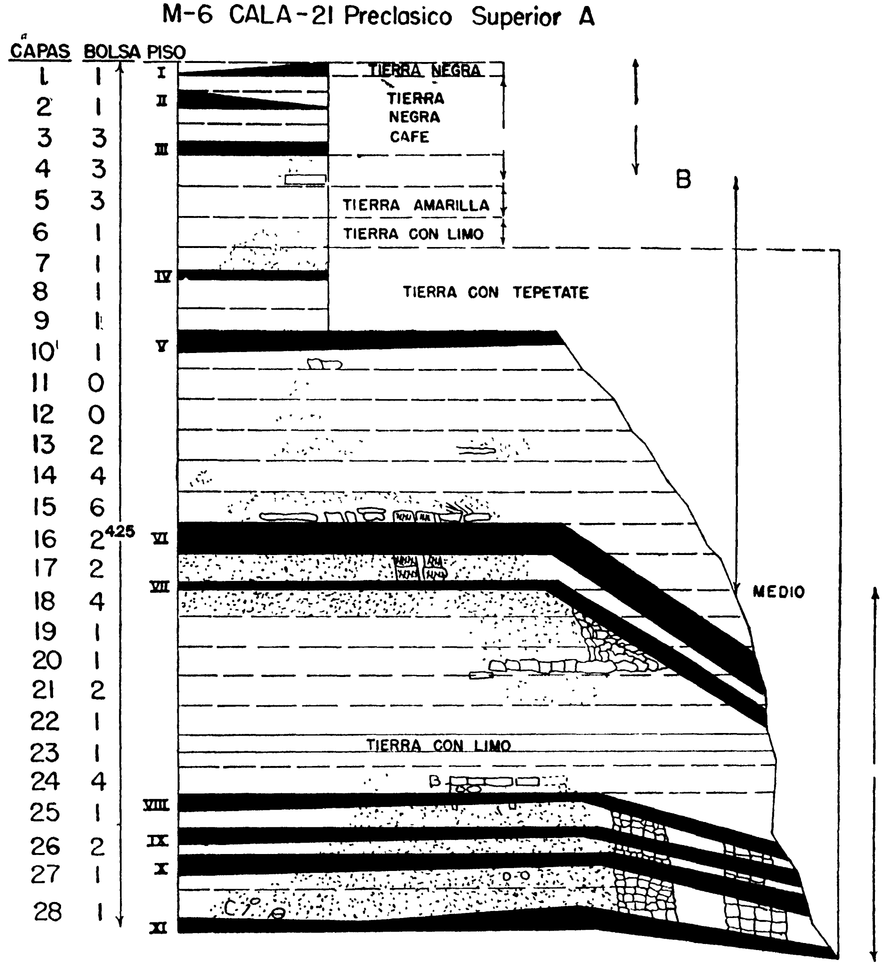
M-6 CALA-21 Preclosico Superior A
CAPAS
BOLSA
PISO
1 1
2 1
3 3
4 3
5 3
6 1
7 1
8 1
9 1
10 1
11 0
12 0
13 2
14 4
15 6
16 2
17 2
18 4
19 1
20 1
21 2
22 1
23 1
24 4
25 1
26 2
27 1
28 1
425
I
II
III
IV
V
VI
VII
VIII
IX
X
XI
TIERRA NEGRA
TIERRA NEGRA CAFE
TIERRA AMARILLA
TIERRA CON LIMO
TIERRA CON TEPETATE
B
MEDIO
TIERRA CON LIMO
B

Fig. 2. Stratigraphic Pit No. 21 of M-6

Floors are indicated by roman numerals, and levels by the arabic. Burials are indicated by capital letters

Pit No. 21 was cut in levels of 15 cms. and reached a depth of 4.25 mts. The first 15 cms. is represented by black dirt. The following 45 cms. is black-brown in color. Starting with level No. 5 a sandy fill, yellow in color, starts to appear. In level No. 6 silt lenses horizontally laminated were found, giving the impression of a floor lake.

From 90 cms. to 4.25 mts. the earth was an orange ocre color due to the large amounts of "toba" or volcanic ashes which it contained.

There is another interruption at level No. 22, where again a silt deposit was found, which probably indicates another rise in the lake level.

THE POTTERY

The archaeological zone of Huapalcalco has given a complete ceramic sequence which corresponds to the three horizons in which the archaeology of Mesoamerica is divided: Preclassic, Classic, and Postclassic. The report on the last two horizons was presented to the Round Table Conference of the Sociedad de Antropología e Historia in September of 1954.

Here we will only discuss the Preclassic Horizon which up to date appears to have three phases: Zupitlan Superior, Zupitlan Intermediate and Zupitlan Inferior(?).

As up to this writing we have only found material which belongs to the Zupitlan Intermediate and Superior, we will only review these two divisions.

Zupitlan Intermediate. To this horizon belongs five wares: Tulancingo Red Brown, Tulancingo Red Yellow, Tulancingo Black Brown, Tulancingo Black and Fine Paste Ware.

Tulancingo Red Brown-Domestic Ware

Paste: Color red brown, has temper of quartz and volcanic rock as well as iron oxide.

Forms: Ollas thicked walled from 1.5 to .8 cms.; spherical bodies, simple or vague necks with simple lips. Few handles which go from lip to shoulder of olla. (See plate 3, figs. 1, 2.)

Decoration:

Slip: Monochrome: Red Brown, Black Brown, Red Yellow Red, and without slip.

Techniques: Textile, basket weave or corncob impressions.

Tulancingo Red Brown ware is only found as a domestic ware and its functional use is as follows:

Storage	77%	
Cooking	23%	100%

There are four ritual wares. Three were made in the valley of Tulancingo. The texture and paste of these wares are the same as the domestic ware. They have the same kind of temper, the only difference lies in the color of the paste and the finish applied. The fourth ware is a fine pasteware, having very little temper and is intrusive.

Forms: Cajetes or bowls straight walled or divergent with concave bottoms with composite silhouette; or convex shaped dishes. All these wares have thick walls with the exception of Tulancingo Black which has two thickness of walls: thick and thin (Fig. 3, figs. 3–7).

Decoration: Ware: Red Yellow
Slip: Monochrome: White. Diagnostic of this period.
White Gray. Diagnostic of this period.
Red
Red Yellow
Dichrome: Red/Red Yellow
Red Ocre/Yellow
Black Cream Red. Diagnostic of this period.

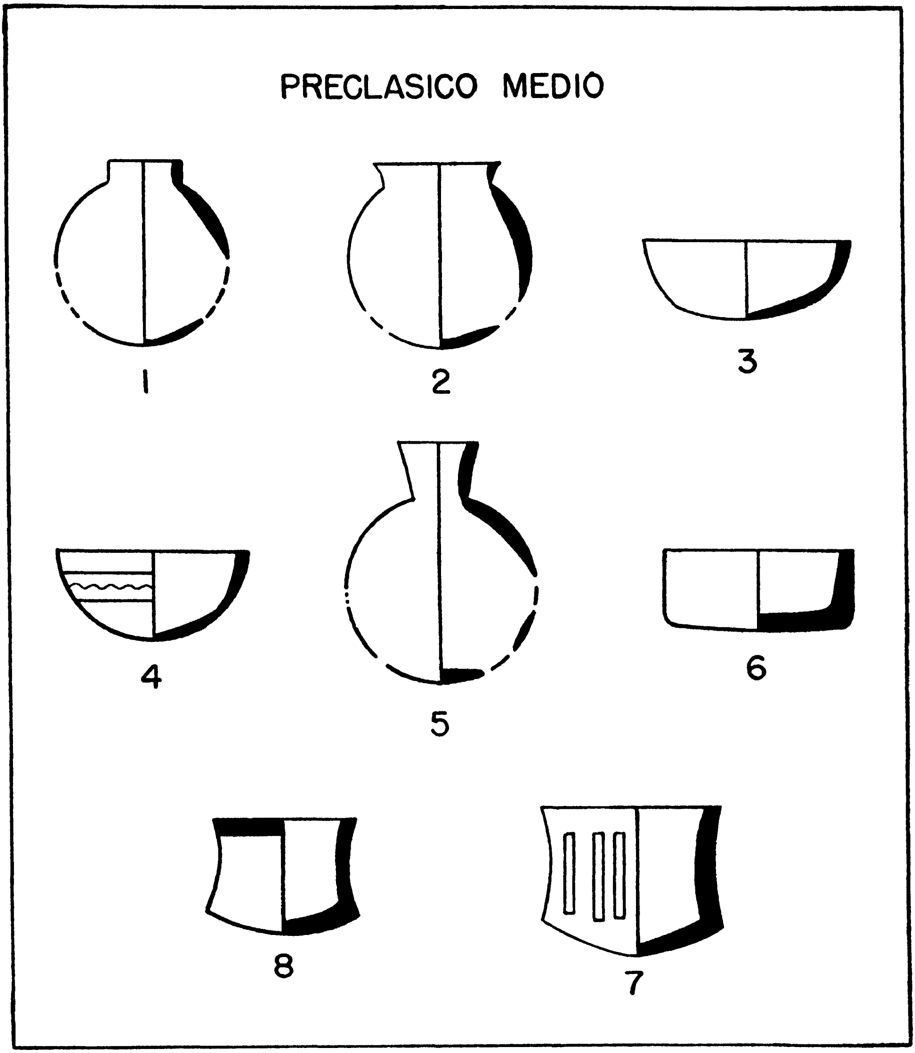

Fig. 3. Forms and Types of Decoration most Frequent in Zupital Intermediate Period

Ware: Black Brown
Slip: Monochrome: Black Brown
Red
White Fugitive or Laca. Diagnostic of this Period.
Dichrome: Red/White
White/Black Brown
Brown/Black Brown

Ware: Black
Slip: Monochrome: Black Smoke

Ware: Fine Paste
Slip: Orange laca
Gray

Techniques: Polished, engraved and incised with running motives of geometric designs: wide bands; horizontal, vertical or wavy.

The percentages of these wares are as follows:

Tulancingo Red Brown domestic	68%
Black Brown ritual	11%
Black Smoke	8%
Red Yellow	9%
Fine Paste	4%

Figurines: Various fragments of heads and bodies of types: C, D, K and O were found as per Valliant's classification for the Valley of Mexico.

Zupitlan Superior Phase B.

Tulancingo Red Brown-Domestic Ware

Paste: Color Red Brown, Coarse texture with temper of quartz and volcanic rock.

Forms: Thick and medium walled ollas; with high, medium and short necks; with simple, flat or turned over lips; semiconvex or flat bottoms. Miniature plates, griddle plates (comales), hand incensers, ear plugs solid type and small discs. (See Fig. 4, figs. 1, 2, 3, 5, 6.)

Decoration:

Slip: Monochrome: Black Brown
Black Smoke
Red brick orange
Red Yellow

Techniques: Stick polish, brush, corrugated and textile impressions.

Tulancingo Red Brown domestic ware percentages as regards its use, is as follows:

Storage	64%	
Cooking	36%	100%

The two ritual wares for this phase is Tulancingo Black Brown and Tulancingo Red Yellow. There is no change in paste or temper.

Forms: Bottles, shallow bottom dishes, cajetes or bowls very shallow in depth, with ornamental supports having the following shapes: globular, teat or animal shaped (see Fig. 4).

Decoration: Ware: Black Brown
Slip: Monochrome: Black Brown
Black Smoke
Polichrome: Red Orange, White/Red

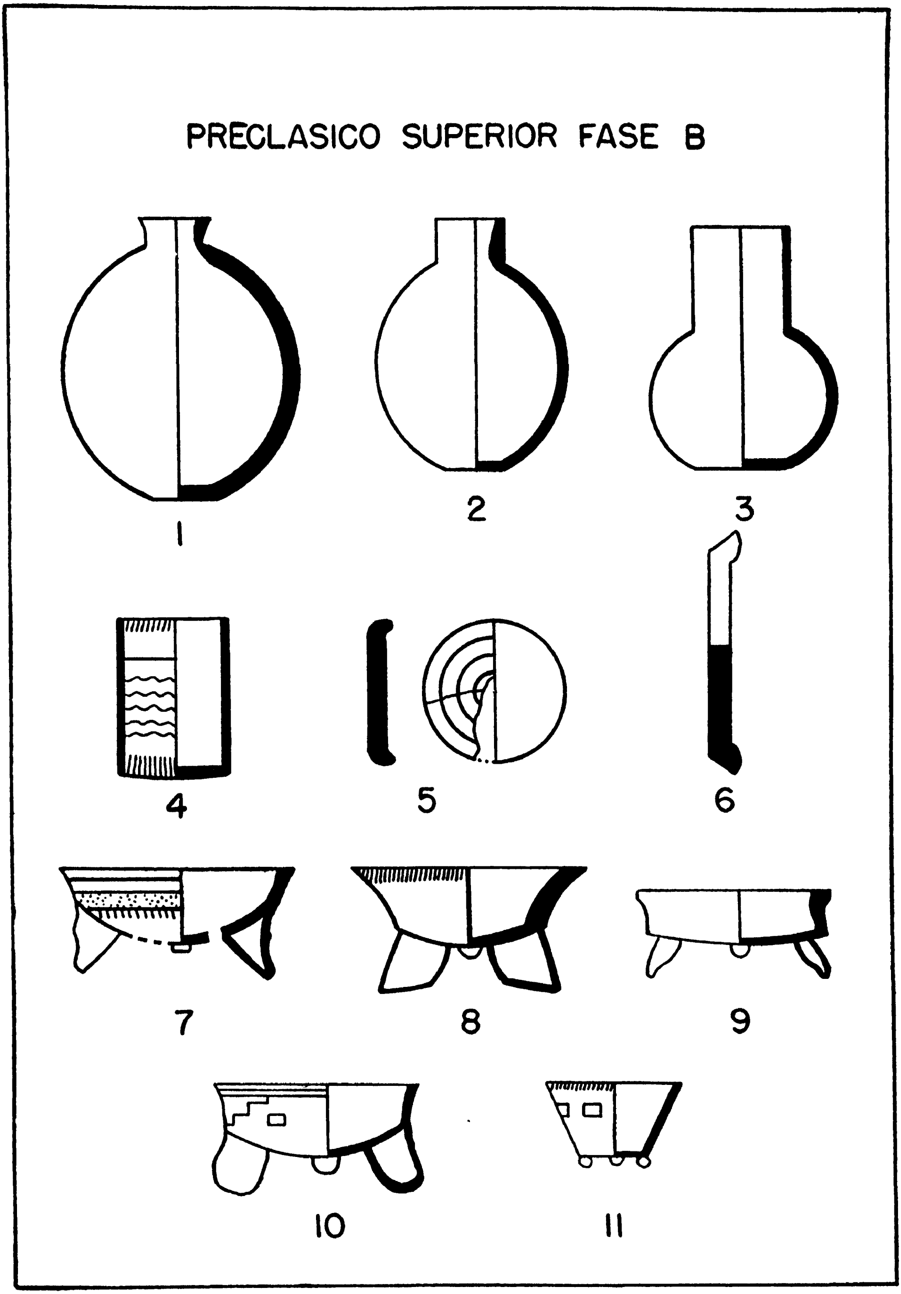
PRECLASICO SUPERIOR FASE B
1
2
3
4
5
6
7
8
9
10
11

Fig. 4. Forms and Types of Decoration Most Frequent in Zupitlan Superior Phase B

Ware Red Yellow
Slip: Monochrome: Red Orange. Diagnostic of period.
Red
Orange
Dichrome: White/Red
Red/Yellow
Red Orange/Red Yellow
Red/Brown. Diagnostic of period.
Red/White
Red/Red Yellow
Polichrome: Red, Black Brown/Red Orange
White, Red Orange, Red Brick/Red Yellow
White, Red/Black Brown
Red Orange, Red, White/Brown

Techniques: Polished, stick polished, incised and lost color, with geometric designs: bands; wide and narrow, horizontal, vertical, wavy, dots, stairs and hooks.

The wares for this phase gave the following percentages:

Tulancingo	Red Brown domestic	75%	
	Black Brown ritual	13%	
	Red Yellow	12%	100%

Zupliptan Superior Phase A. Tulancingo Red Brown continues to be the domestic ware without any change in paste, texture or temper. It has the following percentages with regards to its function:

Storage	54%	
Cooking	46%	100%

The ritual wares are represented by Tulancingo Black Brown and Tulancingo Red Yellow.

Forms: Bowls with vertical walls, flat bottoms with simple lips; and shallow dishes (See Fig. 5).

Decoration: Ware: Black Brown
Slip: Monochrome: Brown
Ware: Red Yellow
Slip: Monochrome: Red
Red Ocre
Red Brown
Dichrome: Red/Red Yellow
Red/Red Orange
Polichrome: Red, Red Ocre/Red Yellow
Black, Red Yellow/Red
Red, Orange/Red Yellow

Techniques: Stick polish, simple or double horizontal fluting, lost color, with geometric motives: wide and narrow bands, horizontal, vertical or wavy; hooks and half moons.

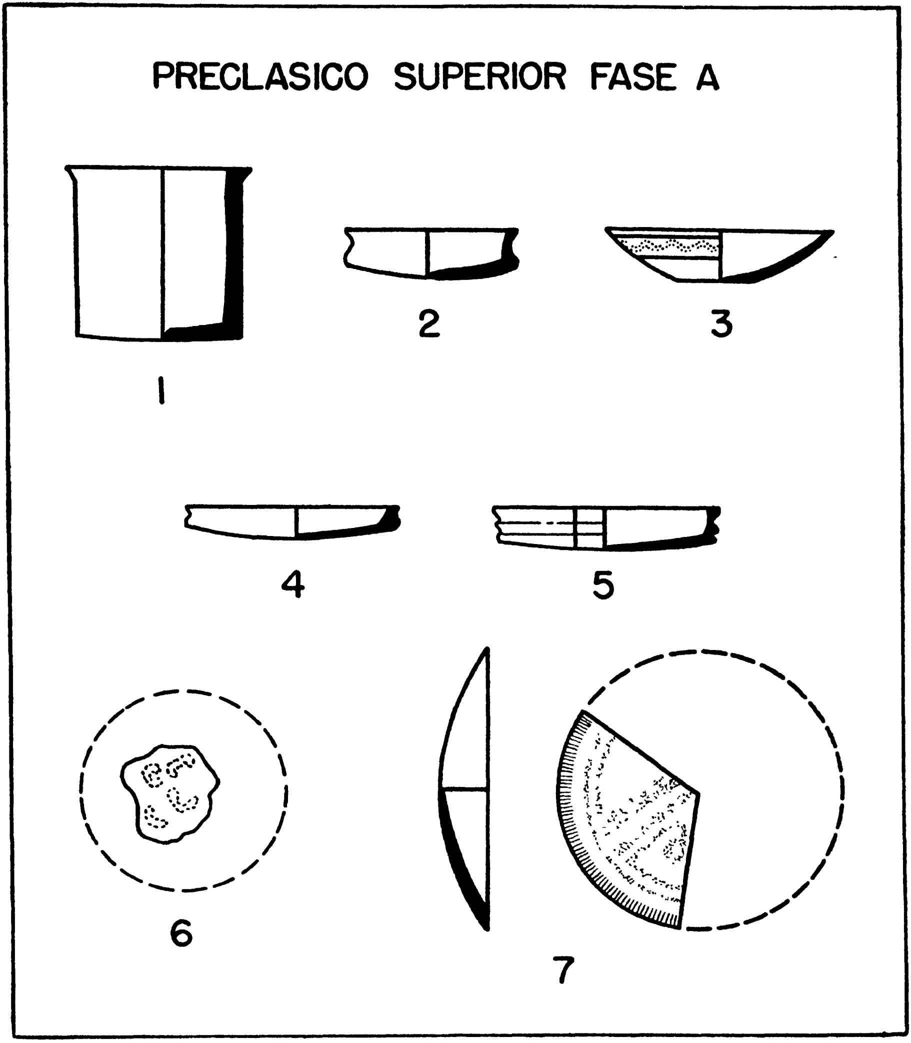
PRECLASICO SUPERIOR FASE A
1
2
3
4
5
6
7

Fig. 5. Forms and Types of Decoration Most Frequent in Zupitlan Superior Phase A

The percentages for this phase are as follows:

Tulancingo	Red Brown domestic	60%	
	Black Brown ritual	26%	
	Red Yellow	13%	99%

GEOGRAPHIC AND CHRONOLOGICAL RELATIONS

Due to the fact that the exploration of M-6 has not been complete, we have only been able to go into detail with the two divisions of the Preclassic; the Intermediate, and Superior with its two phases A and B.

Wares belonging to the Intermediate period began to appear from level No. 17 and continued to increase in percentage up to level No. 28 where the excavation was abandoned because of lack of funds.

The material belonging to the Preclassic Intermediate period indicates that we are dealing with people having the ceramic tradition of the Valley of Mexico. As can be seen in the wares similar to that of Zacatenco, like Black thin ware with incised design, the White Fugitive or Lacas, as well as the Fine Paste of Huapalcalco that is similar to the Orange Ware. The Tulancingo Red Yellow Ware with slip of Red, or Black Cream with Red is the equivalent to Zacatenco's White on Red (Vaillant 1930: 87–88) and figurines' types C, D, K and O as per classification of Vaillant. All these wares and figurines are diagnostic of this period.

But Huapalcalco also received influences or has direct contact with the Olmec tradition. In stratigraphic pit No. 23 was found the rocker stamp technique and also in Tulancingo Black with engraved or excavated motives as well as the Fine paste ware in orange and fine gray, speak of this contact, not only with the valley of Morelos but also sites like Tres Zapotes (Weiant, 1943: 122–123) (R. Piña Chan, 1955a: 26; 1955b: 83–84).

The period Preclassic Superior phase B is found, starting from level No. 28 mixed with material of the former period or the intermediate. It increases in percentage up to level No. 17; from there on up to level No. 6 it reigns alone. To this period belongs the burials and the architectural remains of structure I as well as the floors IV to XI.

The diagnostic markers for this period are: polichrome wares, red/yellow, the ornamental supports, solid ear plugs, and lost paint color technique.

These wares not only show relation with Zacatenco Superior (R. Piña Chan 1952: 37) but also with Ticoman (Vaillant, 1931: 238, 376, 384, 395), Cuicuilco (Noguera, 1939: 220–221) and other sites of the valley of Mexico like Tlapacayo (Barba, 1956: 176, 178), as well as the valley of Morelos (E. F. Jacobs Muller, ms. Tepoztlan); with Tres Zapotes (Drucker, 1943: 115) as well as the site of Pavón of the Gulf of Mexico (Ekholm, 1944: 340–341). Likewise one must not forget the strong influence from Chupicuaro as shown by the high percentage of the polichrome ware found on this site.

The representative wares of Preclassic Superior phase A start with level No. 5 until No. 1. This material is so similar to the potsherds found inside of the Pyramid of the Sun at Teotihuacan, that if one forgot the paste and temper of the wares of Huapalcalco, one would state that this material came from the interior of the Pyramid of the Sun at Teotihuacan (Noguera, 1935: 7–20). Also there are certain techniques and slips which are similar to the wares found in Chimalacatlan and Tepoztlan, Morelos (E. F. Jacobs Muller, Tepoztlan, Ms.).

Mexico, D.F.

TABLE A

PRECLASSIC CERAMIC SEQUENCE

Approx. Date	*Valley of Tulancingo*	*Valley of Zacatenco*	*Mexico Ticoman*	*Coast Tres Zapotes*	*Sites Pánuco*
50 AC	Superior Phase A	Superior	Teot I Ticoman 3	Intermediate	II
190 BC			Ticoman 2		
490 BC	Phase B		Cuicuilco Ticoman 1		
	Intermediate	Middle Copilco		Inferior	I
850 BC					

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THE MIXTECA-PUEBLA CONCEPT IN MESOAMERICAN ARCHEOLOGY: A RE-EXAMINATION

H. B. Nicholson

There is an increasing emphasis in New World archeology on a more precise conceptualization of the vast mass of raw excavational data which has accumulated in recent years. Although a tremendous amount of basic fact-gathering remains to be done, it is recognized that for continuing progress in the field a constant refinement of our methodological and theoretical tools is equally necessary. Occasionally it is also worthwhile to re-examine and, when called for, to reformulate established concepts, particularly where these have been employed somewhat loosely. It is the aim of this brief paper to re-examine and to present suggestions for tightening one such formulation which has had a considerable influence in recent Mesoamerican archeology, the Mixteca-Puebla concept.

Vaillant, in three important studies published between 1938 and 1941 (Vaillant 1938, 1940, 1941) created this construct as a by-product of his attempt to erect a general interpretational scheme for the prehistory of Mesoamerica, with special reference to central Mexico. Boiled down to essentials, Vaillant visualized the development and crystallization of what he variously termed a "culture," "civilization," or "culture complex" in the region of Puebla (especially at Cholula) and the Mixteca of northeastern Oaxaca immediately following the Teotihuacan period, during the "Chichimec" interregnum in the Valley of Mexico. He saw it as diffusing into the Valley, especially at Culhuacan, and providing "the source and inspiration of Aztec civilization" (1941: 83). He also believed that elements of this "culture" were carried throughout Mesoamerica, from Sinaloa in the north to Nicaragua in the south, chiefly by actual migratory movements. So important did he view this impact that he labeled his fifth and final major time division of pre-Hispanic Mesoamerica the "Mixteca-Puebla Period" (1941: Chart 1).

Vaillant never presented a systematic exposition of his concept, but in two brief passages he indicated in general terms its major elements: a carefully defined polytheism, the *tonalpohualli*, 52-year cycle, stylized picture writing, chiefly lineage, formal war, and "characteristic ceremonial practices" (1940: 299; 1941: 84). As sites, areas, and phases which displayed characteristic Mixteca-Puebla influence, apart from late central and southern Mexico in general, he specifically singled out Guasave in Sinaloa, Xochicalco, the Cerro Montoso phase in Veracruz, "the Mexican occupation of Chichen Itza," Santa Rita in British Honduras, Naco in Honduras, and Guatemala, Salvador, and Nicaragua generally. In addition, he felt that "elements of the religion affected tribal communities as far distant as the southeastern United States" (1941: 84).

Applying it to their own findings, Vaillant's Mixteca-Puebla concept was soon accepted by other archeologists. One of the first was Ekholm, who, in his

1942 Guasave report, used it to clarify the source of an important influence in the Aztatlan complex of Sinaloa (Ekholm, 1942: 126–131). In the same year the second Mesa Redonda of the Sociedad Mexicana de Antropología adopted a scheme recognizing four major horizons in Mesoamerica, for the last of which they utilized the label Mixteca-Puebla (Mayas y Olmecas, 1942: 76). Since then the term has passed into common terminological currency. Subsequent to its original formulation, however, the concept has not been the subject of any significant re-analysis. This is all the more surprising in view of a major shift in orientation toward the "Toltec problem" which occurred in the very year that Vaillant's final presentation of the concept appeared and which has since led to the rejection of much of that portion of his scheme which concerns the Teotihuacan-"Toltec" and "Chichimec" periods.

I refer to the Instituto Nacional de Antropología's excavations at Tula, Hidalgo, under the direction of Acosta (beginning in 1940), and the first Mesa Redonda of the Sociedad Mexicana de Antropología, 1941, where it was almost unanimously agreed, after some spirited debate, to effect a final divorce between Teotihuacan and the Toltecs of the traditions. This re-orientation resulted in a recognition that Vaillant's "Chichimec" period (typified ceramically by Mazapan, Coyotlatelco, and Culhuacan-Aztec I) falls almost wholly within the newly defined Toltec period, during which Tula played a dominant political and cultural role in central Mexico. Since Vaillant had classified "Mexican" Chichen Itza as Mixteca-Puebla, the nearly identical parent style of Tula necessarily also fits within his concept. But the picture revealed by the Tula excavations is quite distinct from that drawn by Vaillant, i.e., of a Mixteca-Puebla movement into the Valley of Mexico during a kind of Chichimec time of troubles. Instead, if his chronology on this diffusion is accepted, it would have occurred some time during the Toltec period, in conjunction with the development of the "Mixteca-Puebla" Toltec style itself further to the north. Vaillant's hypothesis of a Pueblan origin for "Aztec civilization" is also greatly weakened by this new alignment, for the essentially Toltec background of the latter is constantly receiving more confirmation.

Do these difficulties caused by our somewhat clearer understanding of the period between the end of Teotihuacan and the rise of Tenochtitlan force the conclusion that Vaillant's Mixteca-Puebla notion has lost its conceptual utility? I think not, but I also believe that a certain amount of reformulation is necessary. The remainder of this paper will be devoted to a consideration of the kind of reformulation which seems to be required by the evidence.

As noted above, Vaillant interchangeably employed the terms "culture," "civilization," "culture complex," and "period" for his concept. Later students added the term "horizon." This terminological variance has led to both ambiguity and confusion. The first two labels seem much too broad to be conceptually useful. The third perhaps has more justification but still appears poorly applicable to the type of data out of which Vaillant erected his construct. The last two describe the concept in terms of a temporal framework; a brief comment will be made on this below.

Analysis reveals that what Vaillant and his followers really have in mind when they employ the term Mixteca-Puebla is, above all, a distinct *style*. Thus when Vaillant speaks of "Aztec civilization" entering the Valley of Mexico at Culhuacan, he actually means that a style of ceramic decoration, out of which evolved the later dominant Valley pottery tradition (Aztec II–IV), seemingly first appears at this site (as Aztec I). Vaillant's other elements, listed above, are not particularly useful criteria, being too widespread temporally and spatially. For example, most, conceivably all, of these traits may have been present, at least to some degree, in both the Classic period Teotihuacan and Monte Alban configurations. Phrased in essentially stylistic terms, however, the Mixteca-Puebla concept can still serve a useful purpose, particularly as a chronologic marker.

What are some of the leading features of the style which lend it distinctiveness? Within the brief compass of this paper, a thorough analysis and definition is impossible, but certain diagnostics can be outlined in a very preliminary way. Perhaps the best touchstone for a definition of the developed style is the Codex Borgia, which, considering its iconographic complexity, esthetic sophistication, and stylistic near-identity to the decorative devices of the local polychrome wares, was very likely painted in Cholula itself. Above all, the style at its best, as in this superb *bilderhandschrift*, is characterized by an almost geometric precision in delineation. Symbols are standardized and rarely so highly conventionalized that their original models cannot be ascertained. Colors are numerous, vivid, and play an important symbolic role in themselves. In general, there is much that is akin to modern caricature and cartooning of the Disney type, with bold exaggeration of prominent features.

These generalities, however, are much less important in distinguishing the style from others in Mesoamerica than certain specific ways of representing various symbols. The presence of even one of these symbols or a characteristic grouping is often enough in itself to define the presence of the style. Among the most highly distinctive individual symbols are: solar and lunar disks, celestial and terrestrial bands, the Venus or bright star symbol, skulls and skeletons (with double-outlined bones), jade or *chalchihuitl*, water, fire and flame, heart, war (*atl-tlachinolli*, shield, arrows, and banner), mountain or place, "downy feather ball," flower (many variants), stylized eyes as stars, stepped fret (*xicalcoliuhqui*), sliced spiral shell (*ehecacozcatl*), and the twenty *tonalpohualli* signs. One of the most frequent and diagnostic symbol groups is the row of alternating skulls and crossed bones (often combined with hearts, severed hands, etc.). Zoomorphic forms are quite distinctive and easily recognizable, particularly serpents (frequently feathered, *quetzalcoatl*, or sectioned, *xiuhcoatl*), jaguars, deer, rabbits, and spiders. The many deities depicted are highly individualized and usually accompanied by special, clearly distinguishable insignia.

The general style can be broken down into a number of regional and temporal variants. The Toltec sub-style is one of the most divergent of these and appears to lack many of the basic elements listed above. Most surviving Toltec relief sculptures and wall paintings (the ceramics rarely display representational or symbolic motifs) seem to deal with predominantly secular themes, although supernaturalistic features are commonly intermixed. If more strictly religious depictions were available, especially pictorial codices, similarities to the general style might well be increased (the rock paintings of Ixtapantongo, State of Mexico (Villagra, 1954), probably provide a fair idea of how a sheet from a Toltec religious manuscript might have appeared). Another notable sub-style is what can be called the Valley of Mexico Aztec style, although its influence extended considerably beyond that range in the wake of the military conquests of the Triple Alliance. It is best typified by the Codex Borbonicus and most of the carved monuments unearthed in Mexico City. Although very close in both spirit and formal detail to the Cholulteca (= Codex Borgia) sub-style, it is marked throughout by greater realism. A third important sub-style is the Mixtec style proper, well-known from the large number of both pre- and post-Conquest codices which have been preserved from this region. It is extremely close to Cholulteca; certain minor but significant differences, however, probably justify its being distinguished. A number of other sub-styles could be delimited, notably that represented by the Codices Fejervary-Mayer and Laud, which perhaps originated in Veracruz (Cuetlaxtlan? The two codices sent by Cortes to Spain?), but space forbids further discussion.

Not only is the Mixteca-Puebla concept best defined in stylistic terms, it is an obvious candidate for one of the most significant recent concepts in New World archeology, the "horizon style." This useful construct, which originated in Peruvian prehistory, was given its first explicit formulation by Kroeber (1944: 108), who defined it as a style ". . . showing definably distinct features some of which extend over a large area, so that its relations with other, more local styles serve to place these in relative time, according as the relations are of priority, consociation, or subsequence." The ideal horizon style is characterized by three principal features: (1) narrow temporal distribution; (2) broad spatial distribution; (3) stylistic complexity and uniqueness. In terms of Willey's "space-time systematics," horizon styles function as ". . . horizontal stringers by which the upright columns of specialized regional development are tied together in the time chart" (Willey, 1945: 55).

Does the Mixteca-Puebla style qualify? Although it falls somewhat short of the ideal, it appears to satisfy the requirements well enough to be conceptually utilized as such. Perhaps its weakest aspect is its rather broad temporal range (in some cases apparently throughout most of the Post-Classic). Stylistically, in spite of numerous temporal and regional variants, it certainly possesses enough complexity and uniqueness to qualify. Its strongest aspect is probably its broad, though quite gappy, spatial distribution.

This latter has yet to be worked out in detail, but some of the most obvious and striking occurrences are worth noting. Apart from its heartland in the areas from which it takes its name and the immediately adjacent regions, especially the Valley of Mexico, it has been located: in almost classic form in the Aztatlan complex of Sinaloa; sporadically elsewhere throughout northwestern and western Mexico; in a distinct regional variant in the Huaxteca (particularly in stone sculpture, shell-work, and wall paintings); throughout the Veracruz littoral in styles often very close to Cholulteca ("Cerro Montoso," Cempoala, Isla de Sacrificios, Cerro de las Mesas Upper I–II, etc.); in Yucatan as the Toltec substyle at Chichen Itza and, in a later variant, as a clearly discernible influence on the wall paintings of Tulum; in the Santa Rita wall paintings, British Honduras (a particularly striking fusion of late Maya and Cholultecoid styles); somewhat weakly and sporadically in the ceramics of Chiapas, Guatemala (where the Cotzumalhuapan or "Pipil" sculptural style also displays certain generalized elements reminiscent of Mixteca-Puebla), and Salvador; and, possibly, as a pale reflection in certain varieties of Nicoya Polychrome in Nicaragua and western Costa Rica. In addition, wherever Plumbate or X Fine Orange is found throughout Mesoamerica, various Mixteca-Puebloid motifs occasionally appear. A thorough check of all Mesoamerican archeological literature would doubtless fill in a number of gaps; further excavation, many more.

The temporal range of the style is bound up with the problem of the time and place of its origin. As of now, Vaillant's hypothesis of earliest appearance in Puebla and/or the Mixteca still seems to be supported by the best evidence;

certainly, as he justifiably stressed, it reached its greatest elaboration there. Ceramically, one of its earliest occurrences is in the "policroma laca" and black-on-orange wares of Cholulteca I–Altar de los Craneos, falling apparently near the base of the Early Post-Classic (coeval with Mazapan, Coyotlatelco, Culhuacan–Aztec I, etc.; Noguera 1937; 1954). The formative stages through which it passed, however, have not yet been clearly revealed, either at Cholula itself or elsewhere. Sculpturally, some of the motifs of the Xochicalcan style definitely foreshadow the developed Mixteca-Puebla style, particularly calendric symbols (I would hesitate to follow Vaillant, however, in classifying it as a Mixteca-Puebla sub-style). It seems likely that Xochicalco, in this as in other respects, may have served as a bridge between the older Teotihuacan–Monte Alban tradition and the newer Mixteca-Puebla stylistic age.

On the basis of present evidence, the following developmental hypothesis can be suggested: it is probable that, as both the Teotihuacan and Monte Alban traditions were sputtering out, a new stylistic synthesis was taking place (in which Xochicalco may have played an important role) somewhere to the east and south of the Valley of Mexico, possibly centered in Cholula. Meanwhile a parallel process of synthesis was developing further to the north, with Tula as a center. The two evolving traditions must have exerted considerable influence upon each other, particularly the southern upon the northern, which became in a sense a sub-style of it, although preserving a strong individuality. The two traditions seem to have met in the Valley of Mexico, where the Chalco region, particularly, displays striking southern ceramic affiliations. The creators of the southern synthesis, the Mixteca-Puebla style *par excellence*, can perhaps be identified, as Jiménez Moreno has suggested (1942: 128–129), with the Olmeca-Xicalanca of the ethno-historic traditions (Historia Tolteca-Chichimeca, Muñoz Camargo, Chimalpahin, Ixtlilxochitl, *et al.*), who may have been the masters of a political empire rivaling and contending with that of Tula. With the break-up of this latter center, an outpouring of migrants, "civilized" Toltecs as well as "barbaric" Chichimeca, evidently overran the southern region. Far from obliterating its stylistic traditions, however, these newcomers appear to have readily accepted them, the Toltec groups probably fusing their own well-developed and similar stylistic canons with those they encountered. The southern tradition, therefore, continued with little basic change, as evolved Cholulteca and Mixtec, eventually strongly influencing the formation of a new Valley of Mexico synthesis, Aztec. All three were flourishing at the time of the Conquest. During both the Toltec and post-Toltec periods, waves of Mixteca-Puebla stylistic influence spread widely throughout Mesoamerica, some echoes perhaps reaching the southeast United States in the "Southern Cult" efflorescence. Although the extensiveness of this diffusion might seem to justify labeling this final Mesoamerican horizon Mixteca-Puebla, the term now coming strongly into use, Post-Classic, is unquestionably preferable, if for no other reason than the fact that the style had such varying influence in different regions, some apparently being affected little if at all.

This tentative reconstruction is only an attempt to modernize somewhat Vaillant's original stimulating hypothesis and, like his, will undoubtedly be significantly modified by further analysis and excavation. Certainly one of the most important tasks for the future is a more refined sequential breakdown of the basic style into successive stages of development. This would probably eventually result in the formulation of at least two major sub-stages; these might be labeled, respectively, Mixteca-Puebla Horizon Style A (= Toltec period) and Horizon Style B (= post-Toltec period; "evolved Cholulteca"). A promising minor lead in this direction, focusing on a single important stylistic element, the ray device of the solar disk, has already been briefly initiated by both Andrews (1943: 75–76) and Caso (1956: 173–174). A careful tracing of both the immediate antecedents of the style and its earliest formational stages is another important desideratum.

In summary: Vaillant's Mixteca-Puebla concept has been subjected to a brief critical analysis, necessitated particularly by the revision of important parts of the overall scheme in conjunction with which it was formulated. It has been suggested that its reformulation in essentially stylistic terms would best preserve its conceptual utility. An attempt was made to define briefly the salient features of the style and to sketch preliminarily its spatial-temporal distribution. Its candidacy as an horizon style was also put forward, and it was felt that it fitted the specifications well enough to qualify in a broad sense. Lastly, further research designed to clarify its sequential development was urged, which, if successful, would greatly increase its value as a chronologic indicator.

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EL PATIO MAS ANTIGUO DE MESOAMERICA

César Lizardi Ramos

El Montículo 6 de Huapalcalco, Tulancingo, Estado de Hidalgo, República Mexicana, se levanta sobre una terraza que mide 120 m. de norte a sur y 35 de oriente a poniente. Su planta es, aproximadamente, de 26.5 m. de norte a sur y 22 de este a oeste, en tanto que su altura era, antes que se iniciasen las excavaciones, de 10 m. por el lado poniente y 5 m. por el oriental.

Florencia Jacobs Muller y yo ejecutamos en él los primeros trabajos en 1955, cuando iniciamos una cala en la cumbre, por el lado septentrional, cala que llegó a una profundidad de 3 m. y en la cual fuimos levantando capas de 0.15 m. de espesor en una superficie de 1 m. por 0.5 m. En total quitamos 20 capas.

Durante diciembre de 1955 hicimos la segunda serie de excavaciones, como la primera, bajo el patrocinio del Instituto Nacional de Antropología e Historia y su Dirección de Monumentos Prehispánicos y con la cooperación de algunos vecinos de Tulancingo.

La cala hecha en mayo de 1955 atravesó cinco pisos, el más alto de los cuales era de aplanado de cal sobre tezontle. La cerámica y demás objetos recogidos han sido estudiados por Florencia Jacobs Muller, quien expone sucintamente sus conclusiones en el trabajo "The Preclassic Ceramic Sequence of Huapalcalco, Tulancingo, Hidalgo", sometido a este V Congreso Internacional de Ciencias Antropológicas y Etnológicas.

En diciembre de 1955, temerosos de que la gente del lugar destruyera el monumento en busca de "tesoros ocultos", acordamos hacer una disección total de Montículo cortando capas de 0.15 m. de espesor hasta llegar a la roca estéril y recoger todos los datos y objetos arqueológicos que hubiere.

Lo primero que hicimos fue abrir una trinchera en la falda noreste del Montículo, de 6.70 m. de largo por 1 de anchura. Se empezó la trinchera al pie del Montículo en el lado dicho y se continuó hacia arriba, por la falda correspondiente, hasta llegar a la altura del Piso VI.

La excavación puso al descubierto dos construcciones, correspondientes a dos épocas, una adentro y otra afuera (Lám. 1) separadas entre sí por un relleno de 0.90 m. de espesor, hecho con piedras chicas y tierra suelta. Esas construcciones son: el Talud I y el Talud II.

Se trata, en rigor, de muros de revestimiento que en la parte descubierta presentaban, abajo, una hilada de piedras grandes, algo labradas por el lado de afuera e inmediatamente arriba, una hilada de piedras boludas acomodadas, y pegadas con lodo. De la segunda hilada para arriba las piedras usadas tienden a tener cuadrada la cara exterior, y se asientan sobre un núcleo de tierra. Por su aspecto hacen recordar el Cono de Cuicuilco y los otros dos taludes encontrados en el lado noreste durante la quinta temporada de excavaciones (mayo de 1956).

Un piso de piedras chicas, aplanadas con un revocado de lodo, cubría la plaza o terraza que se extiende al oriente del Montículo y que pronto empieza a subir hacia la falda occidental del Cerro de la Mesa.

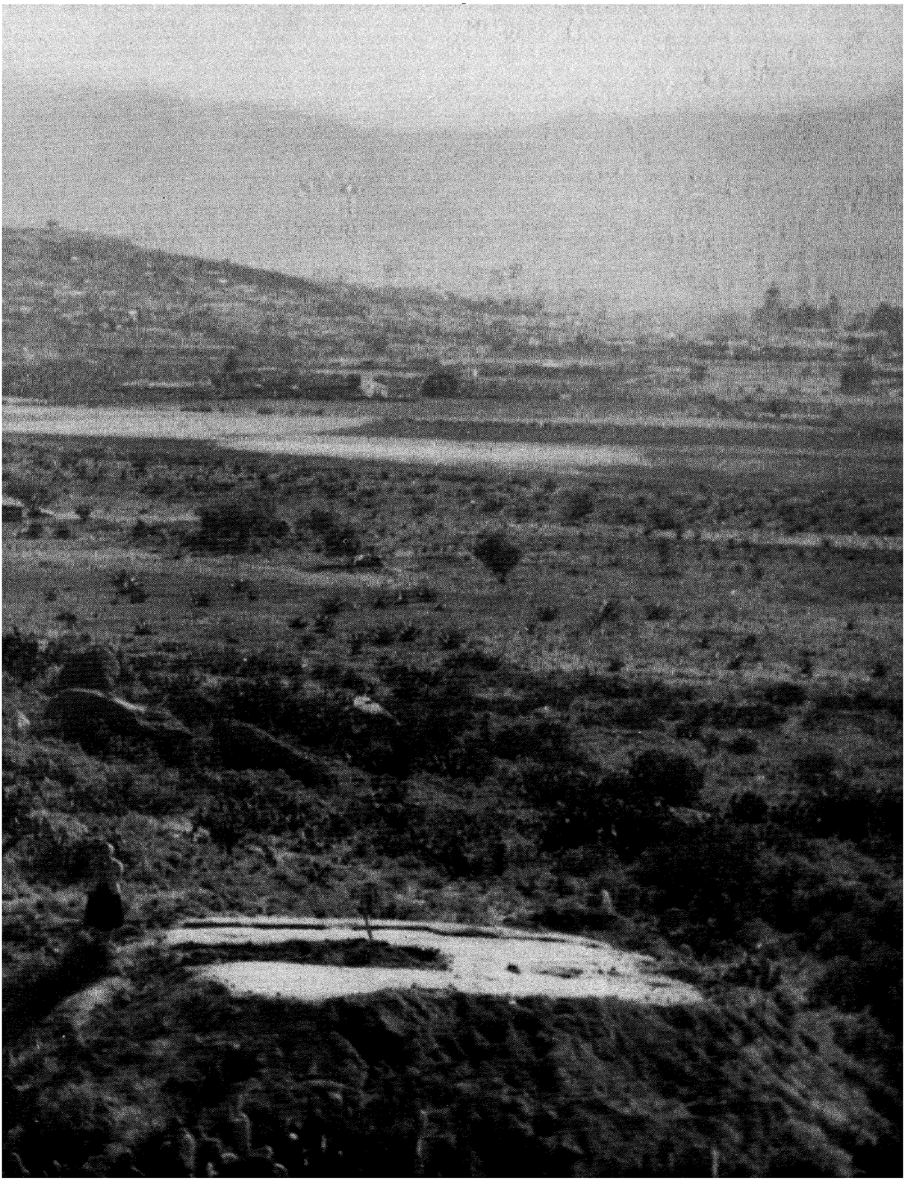


Foto 1

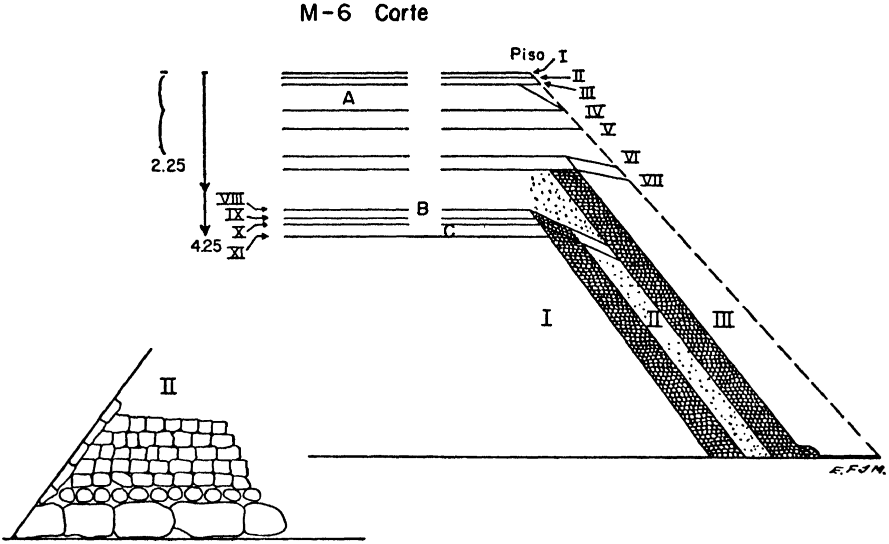


Lámina 1

Mientras se excavaba en la falda oriental del Montículo se inició el trabajo en la cumbre levantando capas de 0.15 m. de espesor. El piso más superficial, o I, se dividió en cuadrados de 1 m. por lado, señalados con letras, como se ve en el croquis (Lám. 2).

El Piso I era de tezontle y grava con aplanado de cal y descansaba sobre una capa de tierra negra, de 0.05 m. de grueso.

El Piso II era de tierra apisonada.

El Piso III se encontró 0.20 m. más abajo y era de barro negro. Inmediatamente abajo de él y en el cuadro "S" hallamos un entierro primario, de un adulto del sexo masculino, en posición de feto en útero y en decúbito lateral izquierdo. Sobre los restos había una capa de piedras sueltas y abajo otra, de piedras más pequeñas (Lám. 3). La ofrenda consistía en una cazuela tricroma y una vasija-efigie en figura de pato, estilo Chupícuaro ambas. Este Entierro "A" había sido perturbado por una violación que, a lo que sabemos, fué hecha durante la noche del día en que iniciamos nuestras excavaciones en el Valle de Tulancingo (9 de abril de 1954). Parece que los violadores temían que nos adelantáramos a ellos en el hallazgo del "tesoro". La excavación fué hecha en la cumbre por el lado oriental y formó una trinchera como de 2 m. de ancho, 4 de largo (en sentido oriente-poniente) y 2.25 de profundidad. Posteriormente hallamos huellas de otras dos violaciones, al parecer, anteriores a la ya mencionada. Una de ellas, en forma de socavón, comienza por el lado poniente, 4.25 m. abajo de la cumbre original y luego se desvía hacia el norte, donde acaba. La otra está en el lado noreste del Montículo, allí donde se adosa a él el Anexo Norte.

El Piso IV quedaba 0.60 m. abajo del inmediatamente superior y era de barro negro cubierto con una capa de lodo alisado. El grueso de este piso era de 0.065 m. Descansaba sobre una capa de tierra apisonada en que había fragmentos de carbón vegetal. Esa capa, que era de 0.30 m. de grueso, no entregó tiestos.

El Piso V era también de barro negro y tenía 0.07 m. de grueso; yacía 0.20 m. abajo del anterior.

El Piso VI quedaba 0.90 m. abajo del V, es de barro negro y está formado de un número de capas que varía entre 8 y 14, con un espesor total de 0.07 a 0.11 m. Al ponerle al descubierto de todo a todo habíamos cortado 1.45 m. de la cumbre del Montículo.

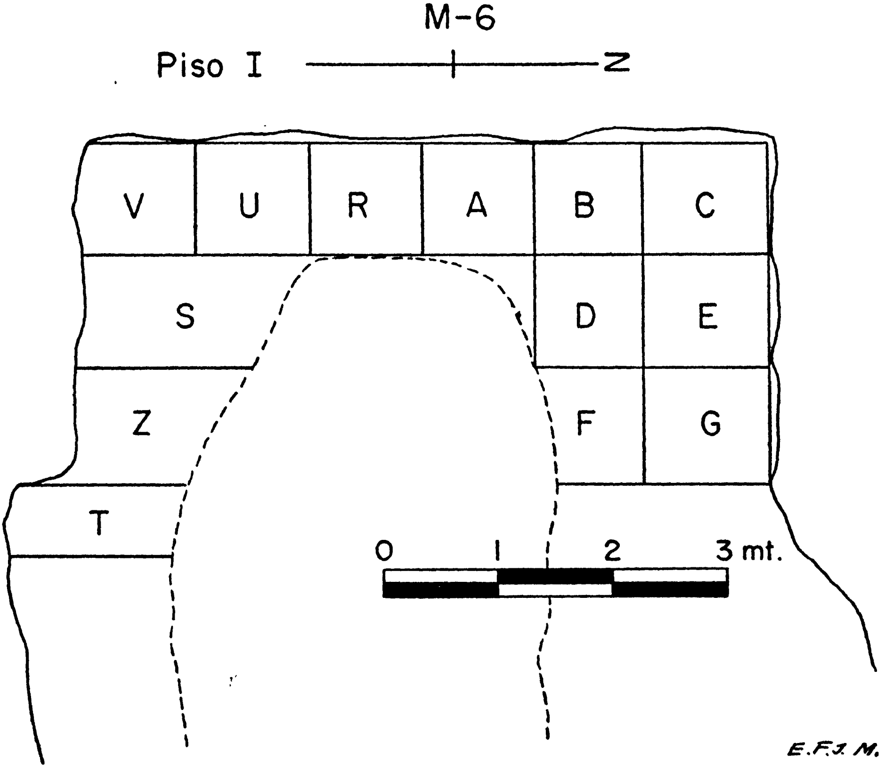
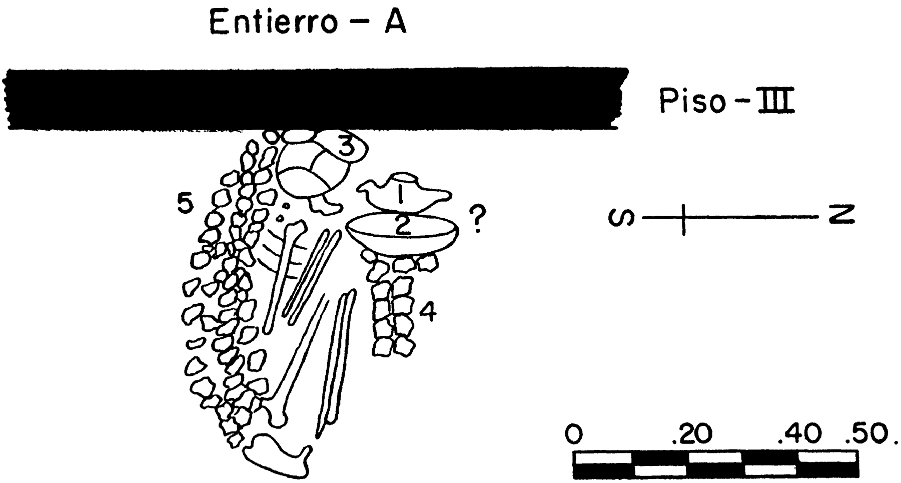


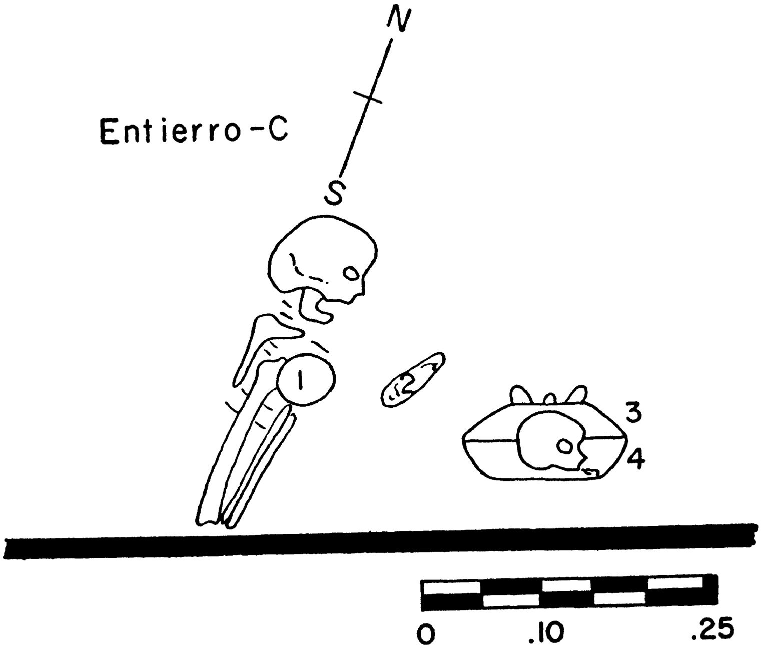
Lámina 2

Este Piso forma el Patio a que se refiere el trabajo aquí sometido. Lo que aun queda de él mide 12 m. de norte a sur y 6.5 de oriente a poniente. La violación a que me he referido más extensamente lo perfora en su parte oriental. La zona central del Patio es un cuadrado de 4.5 m. por lado; tiene un nivel inferior en 0.10 m. al resto del piso. (Láms. 4 y 5 y Foto 1).

La rama septentrional, o sea la parte del piso que yace al norte del cuadrado hundido, mide 3.75 m. de norte a sur y 6.5 de oriente a poniente. Por este último lado tiene una prolongación más ancha que ella, que mide 3 m. de norte a sur y 2.5 de oriente a poniente.

La rama meridional, incompleta, que yace al lado sur del cuadrado hundido, mide 1.25 m. de norte a sur y 5.5 de oriente a poniente. Lleva también una


Entierro - A
Piso - III
3
1
5
2
?
4
S
N
0
.20
.40
.50.


N
Entierro-C
S
1
2
3
4
0
.10
.25

E.F. J.M.

Lámina 3

prolongación occidental, que mide 2.25 m. de norte a sur y 0.75 de oriente a poniente. (Lám. 5).

En cada una de esas prolongaciones hay un brasero en forma de cuenco, hecho del mismo material que el piso y formando parte de éste. Sus dimensiones son: diámetro interior, 0.50 m.; diámetro exterior, 0.80 m.; altura sobre la plataforma, 0.12 m.

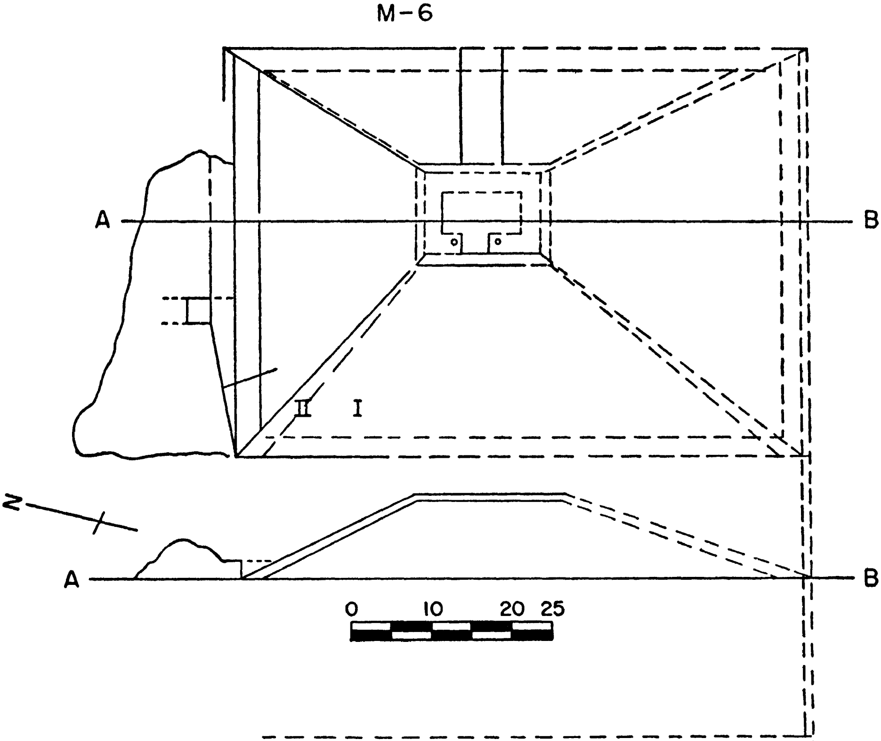


Lámina 4

Dentro de los dos braseros, situados, como se comprende por la descripción anterior, en el lado occidental del Patio, había huellas de fuego, piedritas calcinadas, fragmentos de barro cocido y residuos de alguna resina quemada.

Entre ambas prolongaciones hay uno como vestíbulo, hundido, que se continúa sin desnivel apreciable, con el centro del Patio. Este espacio o vestíbulo mide 3 m. de norte a sur y 2 m. de oriente a poniente. Dos depresionas a manera de canales le cruzan de este a oeste. Cada una tiene 1.80 m. de largo y 0.10 m. de anchura, es decir, en el sentido norte-sur. La distancia entre cada canal y el ensanchamiento o plataforma respectivo es de 0.50 m.

En el lado norte de este Patio hallamos dos hiladas de piedras chicas, unidas con barro y cubiertas en la cara meridional por un aplanado de barro pulido con vestigios de pintura roja en líneas onduladas. Encima de esos restos arquitectónicos o arranque de talud, había unos fragmentos de morillos y bajareque, todo quemado y un adobe, de 0.65 por 0.35 m.

El eje mayor del Patio tiene una inclinación de 17 grados noroeste.

El hallazgo de este Patio, que nos pareció el antecedente de los muchos que se encuentran en Mesoamérica, nos movió a renunciar al proyecto de disecar por completo el Montículo 6, pues nos pareció que lo debido era conservar los vestigios arquitectónicos que habíamos puesto al descubierto y que eran anteriores a los más antiguos que se conocían en el Altiplano de México y acaso contemporáneos del Cono de Cuicuilco, Tlalpan, señalado como construido en la fase Superior del Horizonte Preclásico, o Arcaico.

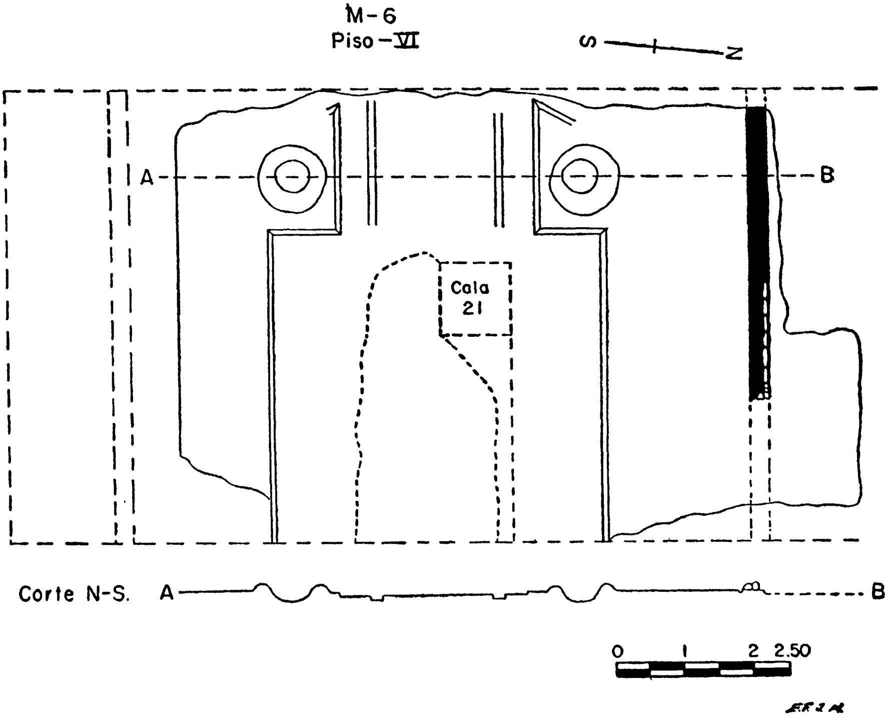


Lámina 5

Suspendimos el trabajo y no volvimos a él hasta mayo de este año, en que pusimos en práctica otro plan, consistente en ahondar la violación de la parte oriental.

Lo primero de importancia que hallamos en esa temporada, la quinta que hemos hecho en el Valle de Tulancingo, fué, al lado norte de la violación, un par de piedras y al oriente de ellas, unos fragmentos de bajareque y de morillos carbonizados. Continuando la excavación encontramos otros cinco pisos, cuya descripción es como sigue:

El Piso VII yace 0.35 m., en promedio, abajo del VI, es de barro bruñido, consta de cinco capas en la parte examinada y tiene un espesor de 0.04 m. Parece que en su borde oriental está el principio de un talud descendente, que seguimos en una extensión de 1.25 m. Está hecho de piedras grandes en el revestimiento y de piedras medianas, tierra suelta y tepalcates, adentro. A una profundidad total de 3.35 m. dimos con un depósito aluvial gris amarillento, de 0.10 m. de espesor. Suponemos que en alguna ocasión hubo inundaciones y que hasta el nivel de ese limo llegó el agua de la Laguna que ocupaba la parte más baja del hoy Valle de Tulancingo.

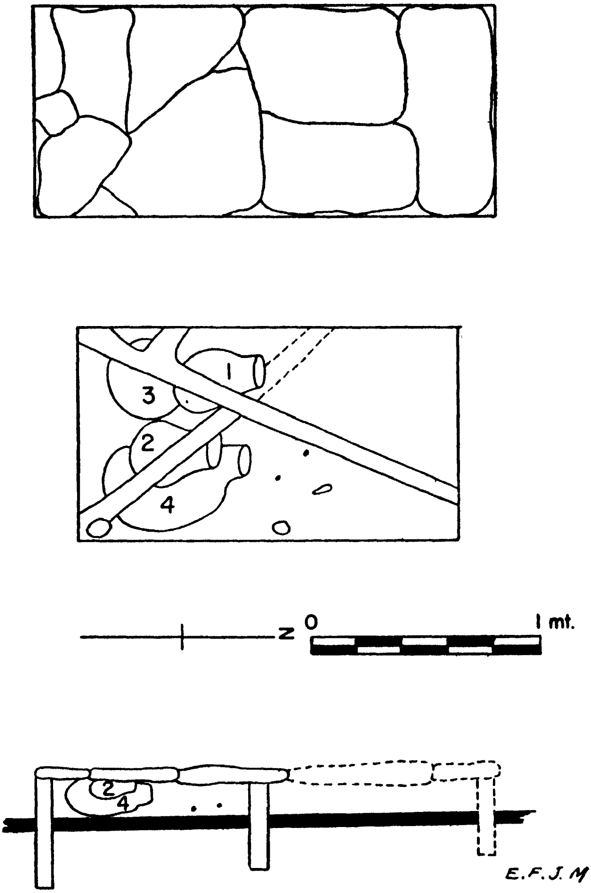


Lámina 6

El Piso VIII fué encontrado 1.14 m. abajo del VII, es de barro negro, bruñido y tiene 0.07 m. de grueso. En él hallamos el Entierro B, que creemos puede uno considerar como una fase rudimentaria de tumba, pues en él se usaron lajas para cubrir los restos, de los cuales encontramos nada más un canino y una falange, pues la violación que he mencionado dispersó lo demás.

Sólo hallamos cinco lajas, unos fragmentos carbonizados de los morillos que tal vez formaban el sostén de aquellas, y dos hoyos para postes, de 0.07 m. de diámetro y 0.47 m. de profundidad. Pensamos que la disposición de este Entierro es la que representamos en la Lámina 6.

Suponemos que los vándalos que hicieron la violación en abril de 1954 extrajeron dos lajas, que arrojaron luego por el lado oriental, ladera abajo. Nosotros las recogimos, y colocadas con las otras cinco formaron un rectángulo de 2 m. de largo por 0.85 de ancho.

Encontramos muchos fragmentos de carbón vegetal abajo y encima de las lajas y suponemos que esos vestigios de fuego indican suficientemente la extensión del Entierro, el cual pudo haber tenido dimensiones semejantes a las del rectángulo de que hablo en el párrafo inmediatamente anterior.

La cerámica recogida en este nivel, abundante por cierto, es descrita por Florencia J. Muller en su trabajo sometido a este Congreso.

El Piso IX yace 0.15 m. abajo del anterior, es de tezontle y lodo y tiene 0.07 m. de grueso. En su lado oriental hallamos restos del Talud I y un metro al oriente de éste, el Talud II, denominaciones éstas que considero estrictamente provisionales mientras no terminemos la excavación del Montículo.

El Piso X está 0.15 m. abajo del IX, es de tezontle y lodo y tiene 0.07 m. de espesor. En la parte oriental de lo descubierto hallamos tres conchas con restos de pigmento rojo.

El Piso XI yace 0.40 m. abajo del anterior, es de lodo apisonado y mide 0.03 m. de grueso. Por su borde oriental parece continuarse con una como rampa que seguimos en una extensión de 3.30 m.

A 0.50 m. del ángulo noroccidental de la excavación y 0.10 m. arriba de este piso yacía el Entierro Doble C formado por restos óseos de un infante y de un adolescente, de sexo no averiguado.

El C 1 contenía la parte superior del esqueleto, que se encontraba en decúbito izquierdo. Los huesos, muy fragmentados, parecen calcinados y presentan tanto por fuera como por dentro, irregulares manchas azules. Sobre el hombro derecho había un fragmento de vasija. Unos 0.20 m. al oriente estaba una mancha de ceniza blanca y 0.10 m. más al oriente yacía el Esqueleto C 2, reducido a un cráneo de niño y pedazos escasísimos de huesos largos. Todo esto se hallaba entre dos cazuelas, completamente rotas. Los restos C 2, calcinados al parecer, como los otros, presentan manchas azules.

Los huesos, dientes y muelas del C 1 son más grandes que los del C 2. Además, los fragmentos de mandíbulas que encontramos de éste último contienen dientes no brotados y en algún caso, dientes que iban a ser permanentes y que estaban debajo de los de leche.

Todos los incisivos de los dos niños tienen forma de pala.

La profundidad del Piso XI contada desde el nivel original, o sea el del Patio I, es de 4.25 m.

Es de suponer que los restos de este Entierro Doble son de niños inmolados, mayormente en atención a que no se encontraron los esqueletos completos, sino una parte mínima de ellos. Por supuesto que la calificación que parece convenir más a este Entierro es la de secundario.

Hay que recordar que los aztecas solían inmolar niños de diferentes maneras, a los dioses de la lluvia y que aun escogían a pequeños de diversas edades, según era la del maíz en el momento en que se hacía la inmolación. Es posible, pues, que los restos recogidos del Entierro C, Piso XI, hayan pertenecido a infantes inmolados a los dioses de la lluvia.

En la misma zona de Huapalcalco, en el cantil con que termina por el oeste el Cerro de la Mesa, existe una cueva que los del lugar llaman Cueva del Chivo. En su interior, Florencia Jacobs Muller encontró durante la primera temporada de excavaciones (abril de 1954) dos pisos y sobre el superior, cerámica Azteca II–III, cenizas, bolas de copal, huesos calcinados de niños y fragmentos de cuchillos. El hallazgo se hizo 0.50 m. abajo del suelo de hoy.

Nos inclinamos también a creer que esos restos indican un sacrificio de infantes en la cueva, sacrificio que era frecuente entre los aztecas y que estaba dedicado al Dios de la Lluvia.

CRONOLOGÍA

La estratigrafía del Montículo 6 de Huapalcalco, estudiada por F. Jacobs Muller y expuesta en el trabajo sometido a este Congreso bajo el nombre de "The Preclassic Ceramic Sequence of Huapalcalco, Tulancingo, Hidalgo", permite sacar las conclusiones siguientes:

Los Pisos I, II y III pertenecen al Preclásico Superior, Fase A, correspondiente a Teotihuacan I de la Cuenca de México, que en tiempo absoluto puede calcularse entre 50 años después de nuestra era y 190 antes de ella.

Los Pisos IV a XI corresponden al Horizonte Preclásico Superior, Fase B, que puede uno colocar entre los años 190 y 490 antes de nuestra era.

A pesar de que no hemos encontrado restos de construcciones del Preclásico Medio en Huapalcalco, sí existe en el Montículo 6 alfarería de ese período, el cual puede uno situar entre los años 490 y 850 antes de nuestra era. Esa alfarería está representada por figuritas tipos C, D, K y O, cerámicas blanca, roja, roja-amarilla, laca blanca fugaz, naranja-laca.

Durante la misma 5a. Temporada se excavó el Montículo 6 por el norte y el noreste y se pusieron al descubierto dos taludes de piedra, uno interior, o I y otro exterior, o II, que podrían ser continuación de los hallados por el lado oriental, cosa que se comprobará o no al continuar las excavaciones. Del I sólo se excavó un fragmento: el superior; del segundo se excavó hasta el piso, o sea una altura total de 2.80 m.

Inmediatamente atrás del Talud I hallamos un fogón y detrás del II, dos fogones, a niveles diferentes, todos limitados por piedras brutas.

Al oriente de esos taludes se encuentra un Anexo, representado por una superposición de pisos, tal vez seis, el más superficial de los cuales tiene un aplanado de cal y queda 2.80 m. arriba del piso de tierra apisonada sobre el cual se desplanta el Talud II.

Sobre el Piso superior del Anexo hay un montón de escombros, entre los cuales hallamos algunas piedras labradas. Se puso al descubierto ese Piso superior en una extensión de 6 m. de oriente a poniente y 0.80 m. de norte a sur. Parte de su borde sur está cubierto por una hilada de piedras labradas. Ese borde queda a 1 m. del Talud II y como es muy irregular puede uno sospechar que haya habido allí una violación y que originalmente ese piso se apoyara en el Talud dicho.

Mexico City, Mexico.

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CULTURAL UNITY AND DIVERSIFICATION IN PERUVIAN ARCHAEOLOGY

John Howland Rowe

Peruvian archaeology is still in a state where even a few years' accumulation of new discoveries can make necessary a major revision of our interpretations of time and space relationships and of their cultural significance. The last four years have been especially productive of new information, and it is time for another attempt at synthesis.

The basis for all general schemes of relative chronology in the Andean area is the concept of horizon styles, now used also in a number of other American areas. The term is Kroeber's, although the concept itself was used earlier by Max Uhle. Horizon styles are, of course, styles which have a very wide distribution, in contrast to local styles which may be found only in a single valley or other small district. In general terms, a situation in which a single style is held in common over a large area reflects some sort of cultural unity, while the existence of many different local styles reflects a pattern of cultural diversity. Closer study of the horizon styles and of their relationships to the local ones which separate them in time should provide us with some clues to the processes of cultural unification and diversification which the archaeological record reflects.

Dates in years are still scarce in Peruvian archaeology, so we must still talk about time in terms of named units in a known relative order. Style names have been most generally used for this purpose, but this usage makes it almost impossible to avoid confusing style and time. We need a terminology that will permit us to distinguish in discussion between the time span covered by a given style phase and the changes that took place in the style itself. The need is not particularly obvious when a single sequence is under discussion, but it becomes imperative as soon as an attempt is made to compare and correlate several sequences. The simplest solution is to restrict our present style terms to their stylistic meanings only and provide an additional set of terms for time periods. The so-called "functional-developmental" schemes which became fashionable in the Peruvian field in 1946 are not satisfactory for our purpose because they introduce another confusion, a confusion of time and cultural process. Cultural process should be a goal of our investigations, not something that we assume at the moment we try to put pottery styles in chronological order.

Before 1946 we had a rather loose method of talking about time alone in Kroeber's distinction between Early, Middle and Late Periods, the Middle Period being that characterized by the Tiahuanaco horizon style. My proposal is to revive this terminology and expand it to take care of the additions that have been made to the Peruvian culture sequence since it was originally devised. The terms suggested are: Initial Period, Early Horizon, Early Intermediate Period, Middle Horizon, Late Intermediate Period, and Late Horizon. The Initial Period covers the time from the beginnings of pottery making to the introduction of incised designs. The Early Horizon is a period, probably of some centuries' duration, in which pottery was fired under such conditions that it was incompletely oxidized and generally dark in color, with a series of characteristic decorative techniques including incision and pattern burnish. It was in this period that the Chavían art style, used as a horizon marker by Bennett and Willey, spread over a large part of northern and central Peru. The Early Intermediate Period is marked on the coast by the rise of such strikingly original local styles as Moche and Nasca. The Middle Horizon is the period of the spread of the Tiahuanaco horizon style. The Late Intermediate Period is again dominated by local styles like Chimu, Chancay, and Ica. The Late Horizon is marked by the spread of the Inca horizon style which influenced but did not replace the local styles of the Late Intermediate Period.

As it becomes possible to make more precise correlations, we will have to equate this chronological scheme with the sequence in some particular Peruvian valley, so that it can be used to state time equivalences elsewhere. One of the central coast valleys will probably prove ultimately to be the most convenient reference point.

With this much clarification of terminology, we can turn to a discussion of the tendencies toward unity or diversification that can be traced in the successive periods of Peruvian archaeology. Remains attributable to the Initial Period have so far been reported only in the Virú and Chicama valleys, so our discussion will have to begin with the Early Horizon.

The assignment of cultures to an Early Horizon date presents many difficulties, which, in themselves, have important implications for our problem. There is no single artistic style characteristic of all Early Horizon cultures, and the technical features of pottery manufacture and decoration which we stressed in explaining the terminology for time may very well have lasted in some areas into the Early Intermediate Period. The first Peruvian area in which a chronological sequence long enough to extend into the Early Horizon was established was the north coast. The Cupisnique style of that area can be assigned to an Early Horizon date by definition and used as a reference point for comparisons. There are sufficient similarities between Cupisnique pottery and the pottery of La Copa near Cajamarca, Chavín, and the shellmound site at Ancón so that all four lots of material must be considered approximately contemporary. Rare pieces in all of them are decorated with designs in the Chavín style. There are also important differences, however, indicating that we are dealing with a family of closely related pottery styles rather than a single style covering all of northern Peru. The stirrup spout, for example, is a common feature of Cupisnique pottery. It occurs less commonly at La Copa and is excessively rare at Chavín and Ancón. There are differences in architecture and architectural decoration also. The stone sculpture of Chavín is well known, and there is a related but not identical style of stone sculpture at La Copa. No comparable sculpture has been found at Cupisnique or Ancón type sites. Thus, even within the restricted area where the Chavín art style is found, there was a high degree of local cultural diversity in Early Horizon times.

The south coast pottery style datable to the Early Horizon is the style of Paracas (Tello's Paracas Cavernas). This style has long been thought to date to the Early Intermediate Period because some Paracas vessels are decorated by negative painting, and this technique is not found on the north coast until after the end of the Early Horizon. However, Engel and Lanning have recently found a Paracas sherd in Early Horizon refuse at Ancón, so an earlier dating is indicated. A large number of Paracas sites are now known from the valleys of Pisco, Ica, and Nasca, and from intermediate fishing stations along the desert coast.

Resemblances between the Paracas culture and the contemporary cultures of central and northern Peru are, on the whole, slight. There are some similarities in pottery, especially plain ware, and Kroeber has called attention to suggestions of Chavín influence in some Paracas designs. The major contrasts of the Early Intermediate Period were, however, already established: the north coast was a region of monumental ceremonial centers and extended burial, while the south coast was characterized by dense, urban type settlements with small public buildings, and the dead were buried in seated mummy bundles.

There are a number of cultures in the south highland region which may be early enough to fall into the Early Horizon, but we have no direct evidence of trade pieces by which a firm date can be assigned them. The highland cultures most likely to be of Early Horizon date are Classic Chanapata and Qaluyu, found respectively in Cuzco and the northern Titicaca basin. Both are known mainly from pottery. Chanapata pottery shows some interesting resemblances to Chavín and Ancón pottery but lacks the Chavín art style. Qaluyu pottery is very distinctive.

The Early Horizon thus appears to have been a time of marked cultural diversity in the Andean area. The pottery styles of the period share at most certain technical features and decorative techniques. We have indications of one major regional grouping, that characterized by the Chavín art style in central and northern Peru, but even within this grouping the cultures were by no means uniform.

The Early Intermediate Period was a time of intensified regional diversification. Regional differences were probably sharper in this period than in any other in the whole time span of Peruvian archaeology. There was no cultural break; the Early Intermediate cultures developed directly out of their Early Horizon predecessors. In some areas, such as the north and south coasts, the regional cultures of the Early Intermediate Period marked a peak of artistic originality and skill, but there are indications that artistic excellence was not characteristic of all cultures of this period.

As we have noted, the Middle Horizon is the time of the spread of the Tiahuanaco horizon style. Recent detailed studies of Tiahuanaco and Tiahuanaco-like pottery, made by Wallace and Lanning at the University of California, indicate that the Tiahuanaco horizon style consists of three distinct regional styles which are artistically closely related. One of these is the style of Tiahuanaco proper, with its center in the southern Lake Titicaca basin and extensions all over central Bolivia and northern Chile. Another is the style of Huari, named for its presumed center of dispersal near Ayacucho in the southern Peruvian highlands. It is found throughout the highlands from Sicuani to Cajamarca and occurs sporadically on the coast. The third is the Pachacamac style, named for the site where it was discovered, which is also one of the major centers of its occurrence. It is at present known only from the coast but may eventually turn out to have a highland origin. These three regional styles appear to be roughly contemporary, but so many of the known pieces lack archaeological associations that detailed cross-dating is extraordinarily difficult. There are some suggestions that Huari influences reached the central and south coasts before the appearance of the Pachacamac style, and sites like

Pacheco in the Nasca valley may represent actual Huari settlements. Huari influences are associated with the latest phases of the regional styles which flourished in the Early Intermediate Period on the coast.

The origins of the Tiahuanaco, Huari, and Pachacamac styles must be sought in the Early Intermediate Period. The direct ancestor of the Tiahuanaco style seems to have been the style of Qeya (Bennett's Early Tiahuanaco), but many of the features of classic Tiahuanaco design are found more fully developed in the Pucara style which flourished in the northern Titicaca basin and seems to have been approximately contemporary with Qeya. The immediate antecedents of the Huari style are not known, but there was clearly a strong element of Nasca influence in its development. The origins of the Pachacamac style are entirely unknown, but its relationship to Huari is particularly close. Presumably the common artistic features which tie together the Tiahuanaco, Huari, and Pachacamac styles had their origin in some single center. On the basis of the evidence now available, Pucara is the best candidate.

Too little work has been done on the Tiahuanaco style of Bolivia to permit a discussion of the speed and nature of its spread. The Huari style seems to have spread rapidly, and the archaeological evidence suggests that it spread by military conquest. The argument for this interpretation is an analogy with the spread of the Inca style, which was spread by a historically recorded conquest movement. Huari sherds are identical wherever found and they are clearly intrusive into the local sequences in most of the areas where they occur. On similar grounds, the spread of the Pachacamac style on the coast may represent military expansion. It should be emphasized, however, that there is no evidence for a single "Tiahuanaco Empire" comparable in size to the later Inca Empire. If the major styles of the Middle Horizon can be equated with political units, each must represent a separate state.

The spread of the three components of the Tiahuanaco horizon style produced a partial cultural unification of the area it affected. On the coast, where the major agent of unification was the Pachacamac culture, the effect of the process was to spread features of southern origin, such as the seated mummy bundle, urban settlement patterns, and the double spout and bridge in pottery, throughout the central and north coast valleys. On the central and south coast the Pachacamac occupation completely swamped the earlier local cultures; the Maranga and Nasca traditions disappeared without a trace. On the north coast, the Moche tradition was broken down and heavily influenced but not completely destroyed. For the Peruvian highlands, we have some evidence of the effect of the Huari occupation at Cuzco and Cajamarca. In both areas, the local styles came under heavy Huari influence but did not completely disappear. Comparable evidence is not available for Bolivia.

In the Late Intermediate Period, local diversification was again the rule, presumably because the political power of the Middle Horizon states had broken up. Where the local traditions had not been completely swept away, as on the north coast, Late Intermediate Period culture was in large part a continuation of Early Intermediate Period culture. In this particular case, the Chimu culture of the Late Intermediate Period is remarkable for an obviously conscious attempt to revive the artistic canons of the Early Intermediate Period at the expense of Huari and Pachacamac influences. On the central and south coast, there was no local tradition left to revive, but after a period of artistic simplification (the Epigonal phase), new and distinctive local styles were developed out of the common Pachacamac base. Different as they became in their later phases, the Chancay and Ica styles had a common origin in the art of Pachacamac.

The Early Inca culture of the Late Intermediate Period was one of those arising from a mixture of Huari influences with elements of an older local tradition. It gave rise to the Late Inca culture of Late Horizon times by a process of internal development, with some influences from contemporary local cultures which are difficult to trace in detail in the present state of our knowledge. It was Late Inca culture which was spread by the Inca conquerors to the furthest corners of their great empire.

Inca policy was oriented toward gradual cultural unification of the empire, as we know from historical traditions. The effects of this policy can be seen in the archaeological remains in three ways. First, public buildings, metal work, and pottery in the purest Inca style are found throughout Inca territory at strategic points. Second, Inca artistic and technological influences can be traced in the provincial styles, being generally strongest in the finest pieces. Third, the ease of communication in the empire encouraged trade and the influence of one provincial style on another. For example, north coast pottery begins to appear in Ica valley graves in the period of Inca occupation. The Spanish invasion came before the unification process was complete, and many of the provincial styles survived into the early Colonial Period, but the work of the Incas was never undone. The common stamp of Inca culture remains on the native population of the Andean area today, from Ecuador to northwestern Argentina.

We may conclude, then, that, although the three Horizons do represent periods of relative cultural unity, the degree and nature of the unity varied very considerably. The unity was least in the Early Horizon and greatest in the Late Horizon, although in no case was complete cultural uniformity achieved in the period covered by the archaeological record.

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THE GULF COASTAL PLAIN IN NORTH AMERICAN PREHISTORY

W. H. Sears

That part of the Coastal Plain of the Gulf of Mexico extending from eastern Texas and southeastern Oklahoma through the northern half of the west coast of Florida is characterized by a reasonably uniform environment. It was also characterized by a rather uniform culture or cultural tradition for a lengthy portion of its prehistory. This paper is intended to define the environmental and cultural features of this region, and to demonstrate their coincidence through a specific span of time, five to ten centuries. This definition will then serve as that of a prehistoric culture area with a definable duration in time.

That such a culture area exists has been suggested explicitly by Willey and Phillips, and by myself. Its existence has been implicit for some time in the syntheses of the prehistory of the area worked out by Ford, Willey, Krieger, and others. Ford, in his "Measurements of Some Prehistoric Design Developments in the Southeastern States," has carried out the bulk of the synthesis, although some of his problems were different.

If a serviceable definition can be arrived at, one which will effectively indicate the separateness of this culture area from the various adjacent and interacting areas which at times expand at the expense of the Coastal Plain area, such as the Mississippian, we may proceed with some attempts at estimating the position and significance of this prehistoric culture area in American prehistory.

The mechanisms or concepts useful as tools in this effort at definition are the tradition and horizon style, as these are applied to ceramics, plus some information from ethnohistorical sources. We may quote Willey for the definitions of tradition and horizon style as they are to be used here. "A pottery tradition comprises a line, or number of lines, of pottery development through time within the confine of a certain technique or decorative constant. In successive time periods through which the history of ceramic development can be traced, certain styles arose within the tradition. Transmission of some of these styles during particular periods resulted in the formation of a horizon style; . . . The distinction between a horizon style and a pottery tradition . . . are opposable concepts in archaeological reconstruction." (1945: 53, cf Phillips and Willey, 1953: 626.)

Physiographically, the area is a flat plain, cut up by a great many slowly moving streams, with frequent swamps. Probably of some importance in southeastern cultural development is the fact that most of these streams have their sources in the Appalachians or their foothills. Soils are usually sandy, and not too fertile. The northern boundary of the area, the fall line, coincides rather well with vegetation and cultural dividing lines.

As Kroeber has suggested, vegetation is particularly useful in definition of North American culture areas, and is probably the most useful single factor. For this area, forest cover is typically a thin stand of pine and oak with a heavy growth of gum, cypress, and magnolia in the bottomlands and swamps. Cane, an item of considerable importance in the aboriginal economy, is common in most of the bottoms.

It is interesting to note that the Livingston and Shreve map of the distribution of fifteen southeastern deciduous trees, reproduced in Kroeber, does particularly well in cutting off the area south of Tampa Bay on the Florida peninsula, otherwise following quite well the physiographic boundaries. A map of modern crop types (Klimm, Starkey, and Hall) demonstrates, perhaps with some relevance to the aboriginal economic situation, that the humid subtropical crops belt is coincident with, and helps to define, our area. Reflecting as it does temperature, soil, and moisture conditions, it helps establish the northern boundary of the area near the Savannah River, a boundary not definable in terms of physiography or forest cover.

A number of lines of evidence are available for definition and documentation of the cultural unity within this area through time. Archaeology, with data of several sorts, offers, necessarily, the primary evidence. Since it appears that the culture tradition persisted, with some late loss of cohesiveness, into the historic period in part of the area, we may also consider some ethnohistorical evidence.

Pottery of this area possesses a number of distinctive characteristics, definable as traditions and horizon styles, which are of especial importance here. We might emphasize that the "type" concept begins to lose utility at this point, and that we must go through, or past it, to styles, modes, or traditions. This poses a problem, since normal publication in terms of types, whether "lumped" or "split," often obscures precisely the information we need. To look at it the other way round, "types" are designed to handle chronological problems in a limited areal and cultural context. More widespread, and perhaps more tenuous relationships, must be handled in terms of the various elements described or counted as unit complexes within types. These elements would fall within Rouse's definition of "modes," more often included in descriptions of artifacts as classes than when description of a complex is in terms of the more abstract "types."

A set of vessel forms, ranging from high-shouldered bowls and small jars to cylindrical beakers, constitute one area-wide tradition, extending through the entire area for most of the total time span. The tradition is perhaps best visualized as a single variable style, one with a more or less normal amount of internal change. Going along with it is an emphasis, a diffuse tradition which we will break down further on, on plain pottery and on incised, punctated, and plastic decoration. The only appendages which are part of this vessel-form tradition are tetrapod feet, and they appear as part of it only early, thus constituting a horizon style. The tetrapods appear at the very beginning of the tradition, the Deptford-Tchefuncte level, time G in the Central Mississippi Valley Archaeological Survey time scale which we will use here, and give way to flat bases, disc or square shaped, during the Marksville–Santa Rosa–Swift Creek period. The basic vessel-form tradition, with the flat base, then continues on in time throughout the area, so rare in its appearance elsewhere that it is probably an indicator of Coastal Plain contact when found in other regions.

A negative tradition, if I may be permitted such, is the lack of handles in any form in this area. This would be relevant only after time D when they become important to the north in the Mississippian tradition. There are of course exceptions to this, to be noted below.

Ford has traced the fortunes of a number of traditions and styles through this area. For reasons to be noted in individual instances, Ford's traditions have been altered, broken up, or omitted in some cases. Generally, this was necessary because, as noted, his problems were different. Nevertheless, the basic synthesis is taken directly from Ford's "Measurements of Some Prehistoric Design Developments in Southeastern United States."

As far as possible, we will take these traditions up in the same order as that used by Ford. First then is the "Punctated, Nail-Punched and Pinched" tradition. In Louisiana, this tradition makes its appearance with the type Tammany Pinched at time G, then continues through Churupa Punctated and Rhinehart Punctated to time B. In Florida, Santa Rosa Punctated with a beginning around E–F gives way to Carabelle Punctated and Tucker Ridge Pinched which carry the tradition into the B–C period. In east Texas, the style appears in the types Weches Fingernail Impressed and in Dunkin Incised. In agreement with Ford, the Davis site is placed at a CD–CB level as a unit.

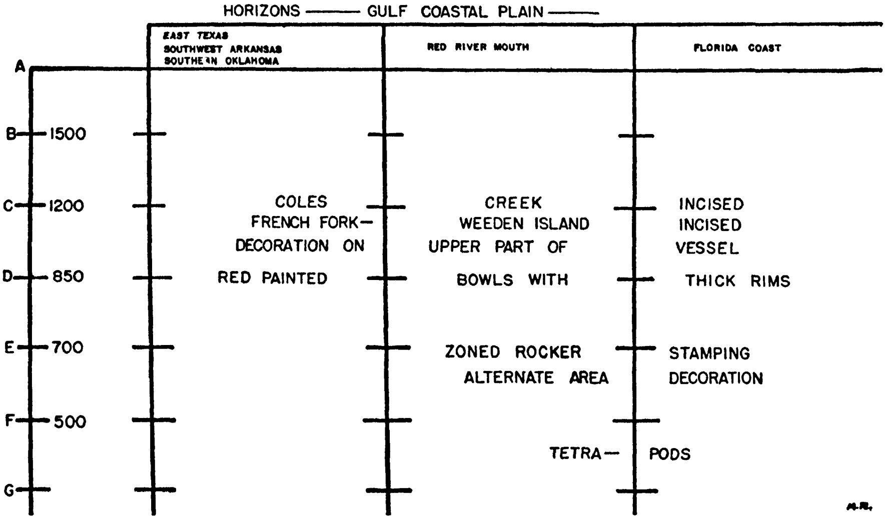
HORIZONS —— GULF COASTAL PLAIN ——
EAST TEXAS
SOUTHWEST ARKANSAS
SOUTHERN OKLAHOMA
RED RIVER MOUTH
FLORIDA COAST
A
B 1500
C 1200
D 850
E 700
F 500
G
COLES
FRENCH FORK–
DECORATION ON
RED PAINTED
CREEK
WEEDEN ISLAND
UPPER PART OF
BOWLS WITH
INCISED
INCISED
VESSEL
THICK RIMS
ZONED ROCKER
ALTERNATE AREA
STAMPING
DECORATION
TETRA–
PODS
M.R.

Fig. 1. Horizons—Gulf Coastal Plain

"Arrangements of Straight Incised Lines," including line-filled triangles, parallel vertical or oblique lines, and cross-hatching, have an interesting distribution. We are excluding here the Hopewellian rim cross-hatching which may or may not be an area style, however useful it may be as an horizon style in eastern United States as a whole. In Louisiana, the tradition extends through the entire time scale, A to G, Tchefuncte to Manchac Incised. In Florida, as with the punctate tradition, temporal range is more limited, running from DE to BC, Keith and Carabelle Incised. In East Texas the tradition is expressed in Davis Incised.

One of the most useful horizon styles is that represented by the types Weeden Island Incised and Punctated, French Fork Incised, and Crockett Curvilinear Incised. This is the most rigidly uniform of the decorative styles we have been considering and, at the same time, the one with the most restricted temporal range, E to CB in all areas from Texas to Tampa.

The earlier Marksville and Yokene Incised types may be representative of a distinct style, perhaps related to the Florida type Basin Bayou Incised. This style may then constitute a useful horizon style too, but with our poor understanding of the spatial and temporal distribution of the Florida type, it is impossible to be certain.

Such types incised with multiple fine line scrolls and swirls as Fatherland Incised and Indian Pass Incised are still a third style. It seems doubtful that this is as uniform a style as is desirable, however. Possibly it represents a backwash of the French Fork–Weeden Island Incised style after modification by Mississippian concepts.

Another useful horizon style is rocker stamping. This can hardly be considered a Gulf Coastal Plain style, of course, but it seems to mark off the earlier horizons here as elsewhere. An exception to its value as an horizon style, when used alone, is its extremely long usage in Louisiana, G to C, Tchefuncte Stamped to Chevalier Stamped.

Related to this style of course is the "Design motifs with roughened background" tradition. I hesitate to attempt a great deal with it here, however, for several reasons. First, it is most clearly an eastern United States wide horizon style in the Hopewellian horizon where the roughening is most commonly the rocker stamping just noted. Secondly, Ford has included Weeden Island Incised, French Fork Incised, and so on, here. Undoubtedly they do participate in this tradition to some degree. The amount of their participation is, however, at least in Florida, a function of time. In Weeden Island Incised it appears that the alternate area treatment is part of the style only at the early, immediately post-Hopewellian level.

I do not see any particular utility in the "Paddle Stamped Tradition" as a unit, at least not for our problem. Check Stamping and cord marking are probably at least independent traditions, and possibly have a number of centers of independent or near independent origin, as well as tremendous space-time distributions. In the case of the complicated stamping, it might with considerable justification be considered an independent tradition, as it will be here, and until very late is not necessarily even paddle stamped. The stamp is carefully applied decoration, and I doubt if the tool was a paddle.

A tradition, limited to the eastern one-half of the area, running from beginning to end of our time scale, from the Deptford series of check and simple stamps through into curvilinear complicated stamps, may be recognized. This is a continuous stamping tradition, using the Coastal Plain series of vessel forms all the way through, with only a change in stamp style at the Deptford–Early Swift Creek break. This tradition serves well to demonstrate the internal unity of the eastern half of the area. This is particularly true in view of the fact that the complicated stamping, after its appearance at roughly time F, participates with the incised and punctated traditions in vessel form styles, as yet indefinable styles in rim forms, and perhaps others. Through most of the eastern part of the Coastal Plain, the complicated stamped pottery appears consistently as part of assemblages with ceramics in the incised and punctated traditions. There is here, clearly, considering complicated stamping *in toto* as a tradition, a lack of coincidence with physiographic and biological boundaries as well as interaction between two readily distinguishable traditions. In these respects, complicated stamping resembles the western horizontal incised line tradition which there runs from time E to time A.

One more tradition that we might consider, one of limited utility as an horizon style, is red painted pottery. Actually this is more recognizable, or at least useful as an horizon style, if we combine two modes and restrict the definition to red painted bowls, with the red paint most often in the interior, with thickened rims. Thus restricted, a definite horizon is discernible, limited to the general E–C range.

There are a number of other features of Coastal Plain ceramics whose utility as traditions and/or horizon styles should be considered. These might include thickened bowl rims, apparent great importance of bowls at certain levels, bowl rims worked out to four points, and pre-cut kill holes in mortuary pottery. Unfortunately, while we know of the presence of these styles, their importance in particular areas and periods is virtually unknown due to lack of field work, or is obscured through normal publication in sherd counts by types.

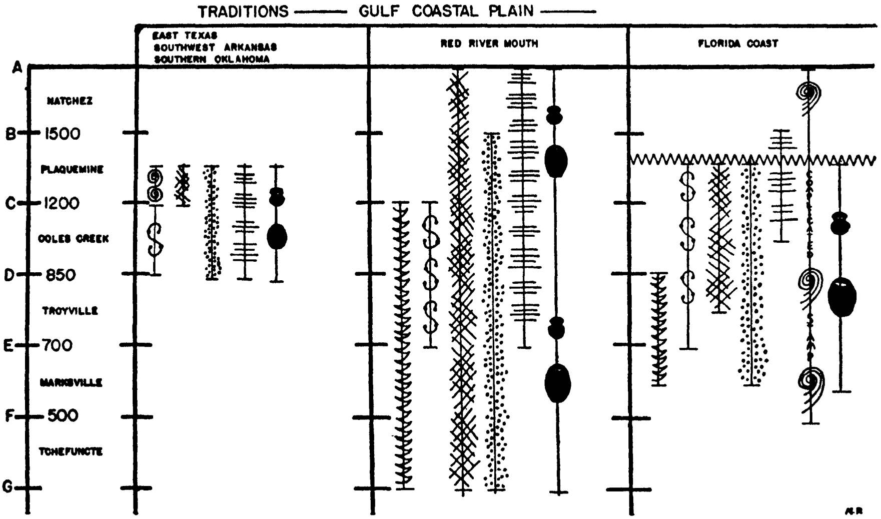

Fig. 2. Traditions—Gulf Coastal Plain

The data we do have is sufficient, I believe, to allow perception of the culture area at least to the level where its real existence and significance may be adequately tested. That part of it having to do with ceramics, discussed so far, is summarized on two charts, each with the total area divided into three regions and into time periods A–G. It would have been desirable to enter the central Florida West Coast and the Mobile Bay area separately, the West Coast being an area where the Coastal Plain styles are watered down and attenuated with distance south. The Mobile Bay area is probably of extreme importance in that it is the contact area between the eastern and western divisions of the traditions and styles, divisions which maintain their distinctiveness throughout the period of overall culture-area uniformity. Unfortunately, field work in these sections has not, to date, provided the sort of data we need here. What information is available is sufficient only to make the assumption fairly safe that they are actually parts of the total area, on the terms on which the charts are constructed. The first chart is intended to emphasize traditions, persistence through time. The second is broken down to emphasize horizon styles, the horizontal threads of the fabric.

I would like to consider the use of temple mounds and community plans as horizon markers, particularly the level of temple mound introduction which may be, as Stephen Williams has suggested, especially early in this area. Unfortunately, the available information is entirely inadequate, although perhaps suggestive, especially for the eastern part of the area. This is even more true of community plans, available in sufficient detail only for the Greenhouse Site and Kolomoki.

Two negative items may, I think, be considered as area-wide traditions. First is the point that definable dwelling structures seem to be lacking in this area in all periods, although structures are present on or in mounds. Scattered postholes and refuse pits are, however, frequently observed. This may indicate the existence of a common type of lightly built structure such as those used by the modern Seminole or in many parts of the tropical forest, in which two, four, or six sturdy posts support a structure of considerable size. The second negative item is the comparative scarcity of projectile points, other stone tools, and bone implements. It seems at least possible that stone and bone were replaced, as raw materials, by cane which is adequate or more than adequate for most purposes filled by bone and stone elsewhere.

Another horizon marker for the area, limited, I suspect, to the main E to B–C period, is the common social structure I have discussed at some length elsewhere. The point is that a heavily class structural social system, ancestral to that of the historic Natchez, was characteristic of the area. Retainer sacrifice, one of the indicators of the social system, might also be mentioned separately as a characteristic of this region. I do not know of evidence for it elsewhere in eastern United States excepting possibly in the Hopewell cultures of the midwest. The retention of skulls, arm bones, and leg bones, presumably as trophies and eventually buried in mounds, is another area-wide tradition.

Recognition of this prehistoric culture area as a distinctive entity will allow us, I believe, to formulate multiple culture or multiple area problems in eastern United States with a greater degree of exactness. For example, as Ford's studies have already indicated, the Coast Plain is the most highly probable source for many of the Mississippian incised decorations.

It is rather clear that the Gulf Coastal Plain tradition, particularly as evinced in ceramics, takes form first in the Louisiana area around the mouth of the Red River, and that it lasts longest there in recognizable form, facts of some historical importance.

To the west, the post-Alto-focus cultures are so heavily Mississippian influenced as to exclude them from the area tradition. To the east, Fort Walton, at least, and Safety Harbor possibly, are representative of cultural replacement.

A major area problem, clearly, is that of relationships between Coastal Plain culture, both as a unit and by segments, and Middle Mississippian culture. As noted, the Coastal Plain may well have been the source for many of the incised Mississippian pottery styles. On the other hand, there is a distinct tendency for developing Mississippian culture to overwhelm, by influence or by actual population expansion, Coastal Plain cultures. This phenomenon becomes pronounced in the B–C period. Examples are the Fulton Aspect, apparently heavily influenced; clear influence, especially evident in vessel forms, in Plaquemine and Natchez levels; and the replacement of Weeden Island by Fort Walton in Florida.

To sum up, it is possible to see that a culture existed in the fifth through the thirteenth centuries, at least, which occupied the Coastal Plain of the Gulf of Mexico from East Texas and south Oklahoma and Arkansas to the vicinity of Tampa Bay in Florida. Although, as obviously predictable, this culture received elements from and interacted with other North American cultures at various points in its history, it maintained its essential unity until swamped by Mississippian culture. Recognition of this area as one which both originated and transmitted culture traits and complexes will, I believe, make formulation and solution of numerous historical problems in the southeast more feasible.

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THE PALEO-INDIAN CULTURE SUCCESSION IN THE CENTRAL HIGH PLAINS OF TEXAS AND NEW MEXICO

E. H. Sellards and Glen L. Evans

The region here discussed is part of the High Plains of western Texas and eastern New Mexico. The Paleo-Indian localities in this area that will be referred to are as follows: Lipscomb, Miami, Plainview, Lubbock and Midland in Texas and several localities of the Clovis-Portales region in New Mexico, including San Jon, Blackwater No. 1, Barber, Elida, Milnesand and Tatum. The position of these localities is indicated on the accompanying sketch map (Fig. 1). The Paleo-Indian cultural complexes within this area in order of age, as now known, are Llano, Folsom, Plainview and Portales. A Sandia projectile point has been obtained. This, however, was a surface find. Hence the exact place of the Sandia culture in the stratigraphic succession at this place remains undetermined. The Plainview culture complex is placed as post-Folsom and pre-Portales on the basis of radiocarbon age determinations. Eden and Milnesand points are present in the Portales complex. Angostura-like projectile points have been found here. Unfortunately, there is as yet question as to their place in the section.

In these hunting cultures the projectile points are the most distinctive artifacts of each culture. In the accompanying Figure 3, two projectile points each of the Portales and Folsom cultures are illustrated and one each of the Plainview and Llano cultures. Additional illustrations of Paleo-Indian artifacts of this region may be seen by consulting the references given at the close of this paper.

Of the several localities listed, the most revealing is Blackwater No. 1 in New Mexico. During the 1930's Dr. E. B. Howard and associates made extensive excavations and collections at several localities in and adjacent to an ancient waterway known as Blackwater Draw which crosses the plains in Roosevelt County, New Mexico and continues to Lubbock, Texas and thence southeastward. A large area in New Mexico, adjacent to this draw and to the towns of Clovis and Portales, has come to be known appropriately as the Clovis-Portales region and has often been referred to as the Clovis locality. Inasmuch as artifacts have now been obtained at many places in this region it seems advisable that each separate locality within the area from which artifacts have been obtained should be separately named. As here used, Blackwater No. 1 is the locality, now enlarged in area, from which Howard and associates obtained most of the artifacts collected from the Clovis-Portales region, New Mexico in the 1930's. The locality is about one mile from Blackwater Draw with which it is connected by a minor drainageway, now largely blocked by windblown sand. During the wet cycles of the Paleo-Indian period, this was a springhead or lake in or at the side of the small draw. During dry cycles, with lowered ground water level, there was no longer a flowing spring or stream here but a more or less dry basin subject to wind and other forms of erosion. Successive wet and dry cycles, with resulting successive periods

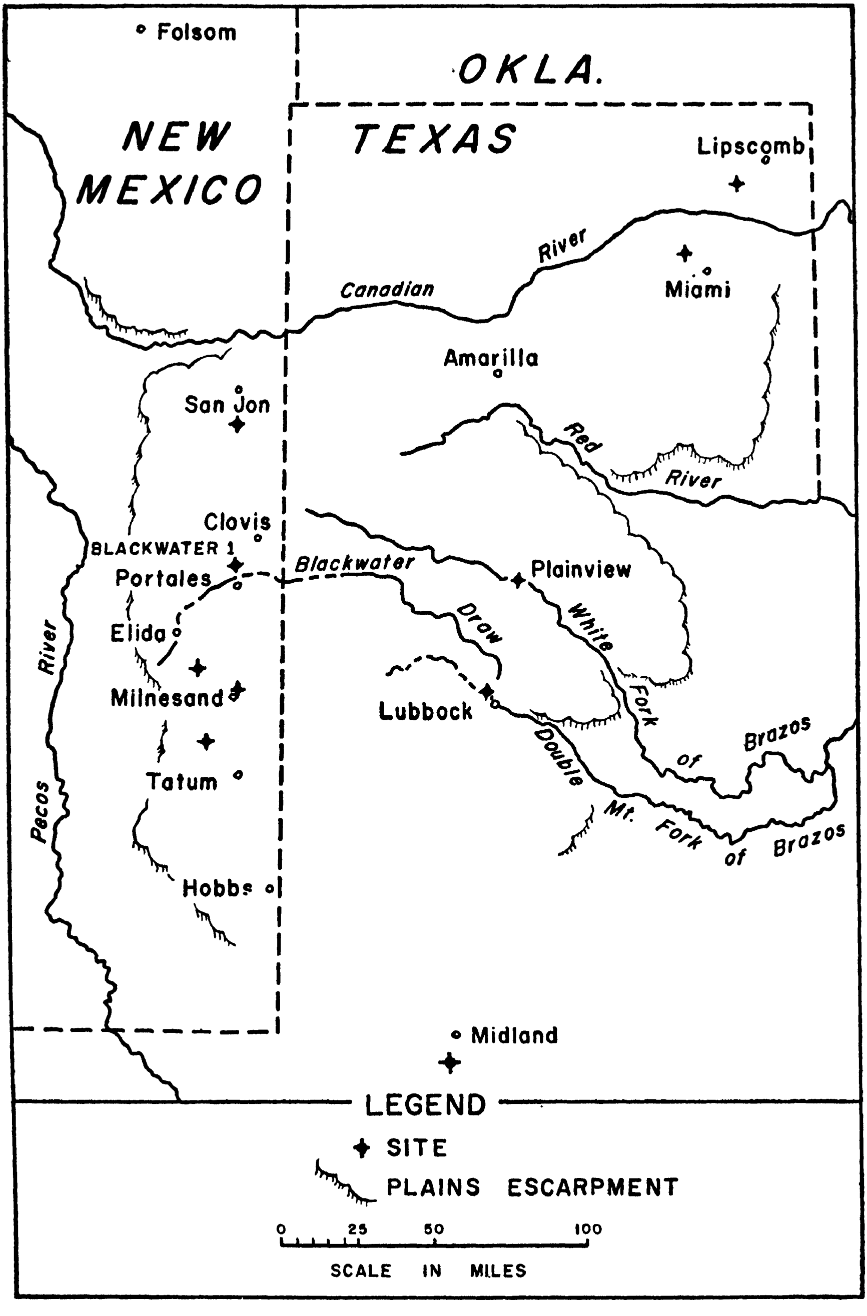
Folsom
OKLA.
NEW MEXICO
TEXAS
Lipscomb
River
Canadian
Miami
Amarilla
San Jon
Red
River
Clovis
BLACKWATER 1
Portales
Blackwater
Plainview
Draw
White
Elida
Milnesand
Lubbock
Fork
of
Brazos
Tatum
Double
Mt.
Fork
of
Brazos
Pecos
River
Hobbs
Midland
LEGEND
SITE
PLAINS ESCARPMENT
0
25
50
100
SCALE IN MILES

Fig. 1. The High Plains of Western Texas and Eastern New Mexico

of deposition and erosion are recorded by unconformities and disconformities in the several units of deposition found in the springhead.

The discovery of artifacts at what is now known as Blackwater locality No. 1 resulted from mining operations for gravel in the early 1930's. In moving overburden to reach gravel the extensive fossil bone deposits that characterize this locality were cut into, and with these bones, were found artifacts. The earliest publication on this discovery known to the writers is that of Dr. E. B. Howard, 1933. In approximately a quarter century since this discovery, mining operations for gravel at this locality have increased unbelievably. The earliest removal of the overburden was by means of scrapers. This method of mining was unsatisfactory because the ground water level was at that time very close to the top level of the gravel. This produced the undesirable condition of water in the gravel pits. However, one operator, Mr. Sam Sanders, conceived the idea of utilizing this underground water in mining. Allowing the water to accumulate in the pit, he used first the dredge-line method and later, when sufficient depth of water was available, built a floating dredge and initiated hydraulic mining which is now underway on a very large scale.

The gravel deposit being mined, so far as known, contains no distinctive fossils and hence the age of the formation is unknown. The gravel deposit is not confined to the springhead but is regional in extent. Overlying the gravel are the depositional units which contain artifacts of the Paleo-Indian and later cultures. Of deposits later than Paleo-Indian age there are at this locality at least two artifact-bearing units of deposition, Archaic and late prehistoric. It is possible that not all artifact-bearing units of the springhead deposits have yet been discovered.

A question of major importance in this study is at what geologic time did the small draw erode to such depth as to cut into the water bearing gravel deposit and thus form this springhead basin of deposition. This date is not yet definitely known. However, the large mammals, horse, camel, bison and elephant of the basal stratum, of the springhead deposits are found also at Midland, Texas. The age of the Midland deposit has been found by radiocarbon test to be 12,500 ± 1200 years.[1] Another check on the age of the basal stratum of this locality is as follows: The Folsom horizon at Lubbock has been determined as age near 10,000 BP. At Blackwater the Folsom horizon is the second stratum above the basal stratum, indicating a considerable time interval between the two units of deposition. On the basis of these conditions an estimate of 12,000 years for the age of the basal stratum of this springhead deposit would seem to be the minimum that can be reasonably made. The origin of the springhead basin, or valley, must therefore have been as early at least as late glacial time, or possibly earlier as there is no certainty that the oldest of the depositional units of the springhead valley have as yet been exposed and recognized. It is quite certain that at the time of the formation of the springhead the climate was moist and this small draw, now dry or nearly so, was during wet cycles, a flowing tributary to Blackwater Draw.

Four units of deposition are clearly recognized in the Paleo-Indian series of the springhead deposits. The sediments of the basal stratum consist largely of a gray to yellowish sand 1 or 2 feet thick. Cross bedding, indicating stream flow, is present. This phase of deposition evidently represents a time when the spring, probably supplemented by rainfall, flowed freely. It is probable that the flow of the spring, plus surface run off, was greater during the deposition of this stratum than at any subsequent time.

The culture complex of the basal stratum is the Llano complex. The artifacts of this complex include at this locality bone implements in considerable numbers, flint projectile points; hammer stones mostly of granite; scrapers; knivers and other chipped stone implements. This culture occurs also at the Miami locality; there represented by three projectile points and one scraper associated with fossil elephant. The principal projectile point of this culture is the Clovis point. Identical or closely related points occur widely distributed in North America. In the great plains region this point has been found at elephant hunting sites in Arizona, Colorado, Nebraska, New Mexico and Texas. An elephant and associated artifacts, collected by Cotter and associates in 1937 were from this stratum; as was also a considerable part of an elephant skeleton with artifacts obtained by the Texas Memorial Museum in 1950. Clovis points vary greatly in size. The largest found at Blackwater has a maximum width of 37 mm. The length is not known as the basal part only was found. The smallest point known in the Llano complex in this region is 36 mm long by 16 mm

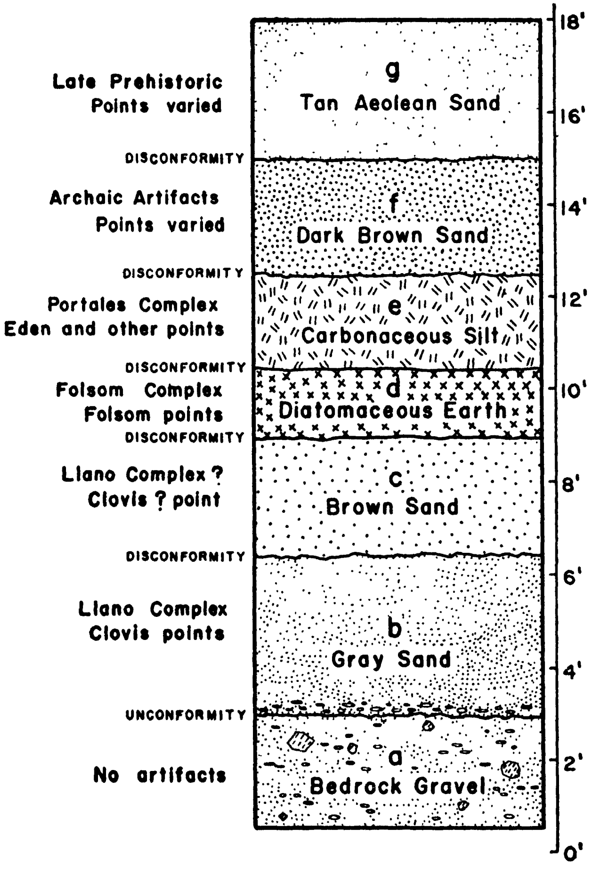
18'
Late Prehistoric
Points varied
g
Tan Aeolean Sand
16'
DISCONFORMITY
Archaic Artifacts
Points varied
f
Dark Brown Sand
14'
DISCONFORMITY
Portales Complex
Eden and other points
e
Carbonaceous Silt
12'
DISCONFORMITY
Folsom Complex
Folsom points
d
Diatomaceous Earth
10'
DISCONFORMITY
Llano Complex ?
Clovis ? point
c
Brown Sand
8'
DISCONFORMITY
6'
Llano Complex
Clovis points
b
Gray Sand
4'
UNCONFORMITY
No artifacts
a
Bedrock Gravel
2'
0'

Fig. 2. Strata and Artifact Types

width. The largest complete point, found at the Miami locality, is about 117 mm long, 30 mm wide. In the course of mining operations the marginal phase of this stratum has been seen and collected from in the east and northeast parts of the mined area. At the east margin the formation thickens and becomes conglomeratic with inclusion of caliche pebbles washed in from the adjacent higher land. The marginal phase has at some places a thickness up to 5 or 6 feet. It also is in some degree calichified. The west margin of this stratum has not yet been uncovered in mining.

The second stratum of the Paleo-Indian series is well exposed at the east side of the Blackwater pit. This stratum at this place consists of well rounded clear sand grains. Although on the average only about one or two feet thick the stratum can be traced from the east margin of the valley westward as far as mining has extended. Near the east margin of the valley this sand stratum, there firmly indurated, reaches a thickness of two or three feet. The rapid thinning from the east margin westward caused the stratum to be designated in earlier publications as the brown sand wedge. As now understood it is not a wedge but a stratum, the west margin of which has not been uncovered. Fossils known from this stratum are large bison, elephant, wolf and turtle. Until recently no distinctive artifact was known from this stratum. However, in August 1956 a projectile point and some flakes were obtained in this stratum in association with the skeleton of an elephant. The projectile point, is suggestive of, but is not certainly of the Clovis fluted type.

The third unit of deposition is the Folsom stratum. During Folsom time the springhead valley was largely if not entirely a clear water lake affording conditions favorable for the growth of diatoms in such abundance as to form a stratum one or two feet thick consisting of the siliceous residue of these minute plants. The length of time during which conditions favorable to growth of algae persisted can as yet be no more than roughly estimated. At the Lubbock locality where similar conditions prevailed a radiocarbon test from the basal three inches of the Folsom stratum gave age determination 9883 ± 350 years[2] BP. A sample representing more or less mid-Folsom gave age 9300 ± 200.[3] From these tests it appears that a reasonable estimate of the time required for deposition of the Folsom stratum would be 1000 years more or less.

The climatic conditions of this millennium or so must have been such as to support vigorous growth of plant and animal life. Otherwise the great herds of bison that came to this lake could not have lived on the plains. Sand dune formation prevailed in this region previous to Folsom time. This is indicated by the considerable number of Folsom artifacts, especially projectile points, found on the older sand dunes. Hence, there exists the probability of a dry cycle accompanied by dune formation preceding the Folsom more favorable cycle. If so, the small draw may have been somewhat blocked during Folsom time by pre-Folsom wind blown sand, thus accounting for the lake during this period of time. The climatic conditions during Folsom time must have been favorable to growth of diatoms generally on the plains as the Folsom stratum at the Lubbock locality also has diatomite deposits.

The Folsom localities of the High Plains region referred to in this discussion are in order from east to southwest Lipscomb, Lubbock, Blackwater No. 1, Elida and Tatum. Lipscomb is a hunting locality containing fossil bison and Folsom artifacts (Schultz, 1943). The Lubbock locality which in stratigraphy is similar but not wholly identical to Blackwater is an oxbow lake in the same drainage system as Blackwater Draw. Certain points from the Midland locality have been identified as unfluted Folsom (Wendorf and others, 1955). Elida and Tatum are Folsom localities in the sand dune region. Neither have as yet been fully described.

The Folsom culture complex has the distinction of having been the first Paleo-Indian complex in America to receive general recognition. The discovery locality was a hunting site near Folsom, New Mexico (See sketch map, Fig. V), the date of discovery was 1926. Projectile points of this complex vary greatly in workmanship. Fluting, normally done with precision, is sometimes omitted or very imperfectly executed. One can readily conjecture that under necessities of the hunt, points at times were put into use unfinished or were made hurriedly of inferior rock. Examples of imperfectly fluted points and points fluted on one side only are recorded at several localities so that the occurrence is by no means exceptional. Of a collection of several points or parts of points obtained in place in the Folsom horizon at the Blackwater locality among fossil bison bones, two are normally fluted, one is fully fluted on one side, and very imperfectly so on the reverse; one is totally unfluted; the remaining two are entirely unfluted on one side and very imperfectly fluted on the reverse. From this record it would seem that in a hunt any rock that could be used was acceptable.

Folsom points vary greatly in size. The largest point obtained by the authors is 26 mm wide. The tip is broken and the exact length cannot be obtained but is estimated to have been 73 mm. A point 23 mm wide, base broken, is estimated to have been 65 mm long. A complete point from near Portales, is 63 mm long, width 22 mm.

Of the small points, one, base and tip lacking, fluted one side, reverse side not fluted, is 14 mm wide. Another point, base and tip lacking, fluted both sides, has width 12 mm. The smallest point obtained, maximum width 11 mm, length 22 mm, is fluted both sides from base to tip.

The six points cited, three large and three small, represent extremes in size, there being but few points as large or as small as these. The great number of Folsom points found are intermediate in size.

The Plainview culture complex, which on the basis of radiocarbon age determination should immediately follow the Folsom, is known at Blackwater No. 1 by only a few artifacts. It is probable that the absence of a well developed Plainview depositional stratum in that part of the springhead that has been mined is due to erosion in post-Plainview and pre-Portales time. If this supposition is true it is reasonable to expect that as mining is continued, at least a remnant of a Plainview stratum will be found resting on the Folsom stratum. Two broken points found near the east margin of the valley are clearly Plainview. These were obtained on the screen but obviously came from the Lake deposits. The exact place in the section, however, cannot be determined. Another broken Plainview base was found on the dump having been thrown out by the mining operations. The type locality of the Plainview complex, Plainview, Texas is only about 100 miles from Blackwater locality and Plainview points are found throughout this entire plains region. Under these conditions it is reasonable to believe that deposits representing the Plainview depositional period were deposited in this springhead valley. Under the mining conditions necessary to the locality finding such deposits if they exist is a matter of chance. The age of the Plainview culture complex as determined from a sample of snails collected from the bonebed and determined by the Lamont Laboratory, Columbia University is 9170 ± 500 BP.

The fourth well recognized stratum of the Paleo-Indian series at Blackwater No. 1 locality, the Portales unit, consists of a dark colored silt stratum varying in thickness from 1 to 2.5 feet. While the three underlying Paleo-Indian strata present a marginal phase differing in some respects from that farther out in the valley, this stratum as seen on the east margin of the valley remains essentially unchanged other than thinning. The depositional conditions of the valley during this interval of time appear to have been as follows: water, probably shallow, with heavy growth of plants, either floating or rooted underwater. Erosion, probably by wind action, during post Paleo-Indian time has removed this stratum at many places in the valley. The largest collection of fossils and of artifacts from the formation were obtained from the westernmost part of the

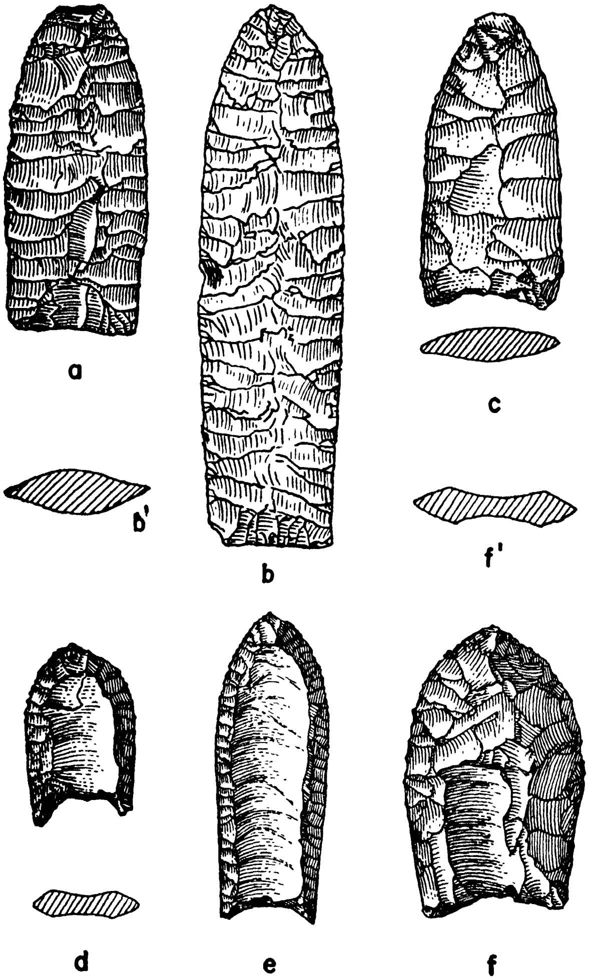

Fig. 3. Projectile Points

mined area. The extreme western area of the formation, however, has not yet been reached in mining.

A radiocarbon test made from a sample taken from near the top of this stratum gave age determination 6230 ± 150 BP.[4] As previously noted, basal Folsom was found to be age 9883 ± 350. The length of time of accumulation of the third and fourth units of deposition plus whatever period of time intervenes between the two, may therefore be estimated at 3650 years.

Noting that radiocarbon dating involves considerable possible error, it may be said that while these time estimates represent the best record of age that is now available, subsequent measurements may differ greatly.

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Notes

1. The sample on which this age determination is based was collected in a co-operative project carried on between the Museum of New Mexico and the Texas Memorial Museum, financed by a grant from the Wenner-Gren Foundation. The radiocarbon test was made by the Lamont Laboratory, Columbia University, New York.
2. Texas Memorial Museum sample 892–329. Test made by Libby, 1950.
3. Texas Memorial Museum No. 892–102. Test made by Lamont Laboratory, 1955.
4. Texas Memorial Museum sample No. 937–779a. Age determination made by the Humble Oil and Refining Company, Houston, Texas.

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Description of Figures

FIGURE 1. Sketch map of North Western Texas and Eastern New Mexico, showing location of certain Paleo-Indian sites as follows: Lipscomb, Miami, Plainview, Lubbock and Midland in Texas, and San Jon, Blackwater, Barber, Elida, Milnesand and Tatum in New Mexico.

FIGURE 2. Geologic section at Blackwater locality No. 1, New Mexico. a, gravel and sand bedrock underlying the artifact bearing formations; b to e inclusive, recognizable depositional units containing Paleo-Indian artifacts; f and g, Archaic and later deposits.

FIGURE 3. Projectile points representative of Paleo-Indian culture complexes as follows: Portales complex; a, slightly shouldered point; b, not shouldered point. Plainview complex; c, Plainview point somewhat below average size. Folsom complex; d and e, two Folsom points, both deeply fluted. Llano complex; f, Clovis point, fluted both sides as indicated by cross section f. Catalog record, Texas Memorial Museum, numbers as follows: a and b, 937–22 and 32; c, 725–1; d, 892–130; e and f, 937–481 and 584.

WESTERN SIBERIAN ARCHEOLOGY, AN INTERPRETATIVE SUMMARY

Demitri B. Shimkin

Western Siberia is a vast alluvial plain of Quaternary age, bounded by the Urals, the Kazakh Uplands and the Altai, the trans-Yenisei plateaus, and the Kara Sea. The plains of the Pyasina-Khatanga system to the northeast and of the Pechora to the west are its physiographic extensions.[1] (See Fig. 1.) Characterized by a gentle gradient, with moraines and remnant bluffs alone rising to elevations of 100–200 meters above sea level; by a predominance of impermeable soils (permafrost north of 60°N, clays and hardpan to the south); and by a close balance of precipitation and evaporation, the Western Siberian plain is a region of shifting streams, and of ephemeral lakes, swamps, forests and meadows, compartmented by the major arteries of the Ob-Irtysh system.[2] Movement and settlement alike have followed these arteries for more than four thousand years.

Western Siberia has few resources accessible to a simple technology. Except in the moraine areas, even stone is rare, while bog iron is the sole metallic ore. The floral and faunal resources were extirpated by the ice and waters of the Riss glaciation, and the subsequent immigrant biota have been subjected to the stresses of a harsh, variable environment.[3] Thus, the confinement of rotting vegetation by thick ice on the streams leads annually to the wholesale asphyxiation of fish, especially in the Central Ob', wherever deep holes and springs do not provide refuge.[4] The fauna of greatest economic importance include the sturgeon and whitefish of the genera Coregonus and Stenodus, above all on the lower Ob'; the ringed and bearded seals, walrus, and white whale, closely associated with the Kara Sea ice packs; the reindeer, Arctic fox, and summer-visiting birds of the tundra, north of the Arctic circle. In the forests, down to 55°N, moose, Siberian roebuck and bear provide meat, with squirrel and sable being the cash crops of the drier, and beaver, of the wetter, lands.[5] The instability of mammal populations in Western Siberia, especially such specialized feeders as the Arctic fox and squirrel, must be stressed.[6] The forest steppe, long a zone of animal husbandry and agriculture, is unique as a firm subsistence base.

Western Siberian weather reflects, summer and winter alike, the interaction of warm, wet Atlantic and cold, dry Arctic air masses. An intensification and northward displacement of the Atlantic circulation brings about a warmer and drier climate in the southern zones; a wetter and warmer one, north of the Arctic circle. A lessening of that circulation has converse effects.[7] Given the precarious ecological conditions of Western Siberia, these climatic fluctuations have led to profound environmental cycles since the Würm glaciation, a period characterized by extensive flooding and by the displacement of subarctic biota (mammoth, woolly rhinoceros, and giant elk) to the present forest-steppe.[8] During this and the succeeding Boreal period temperate forests covered Kazakhstan and the Central Asiatic lowlands to the Uzboy (40°N).[9] In Western Siberia, the environment was greatly impoverished as late as 2000 B.C. Such pre-ceramic and Neolithic sites as Andreyevskoye Ozero and Tatarskiy Bor,

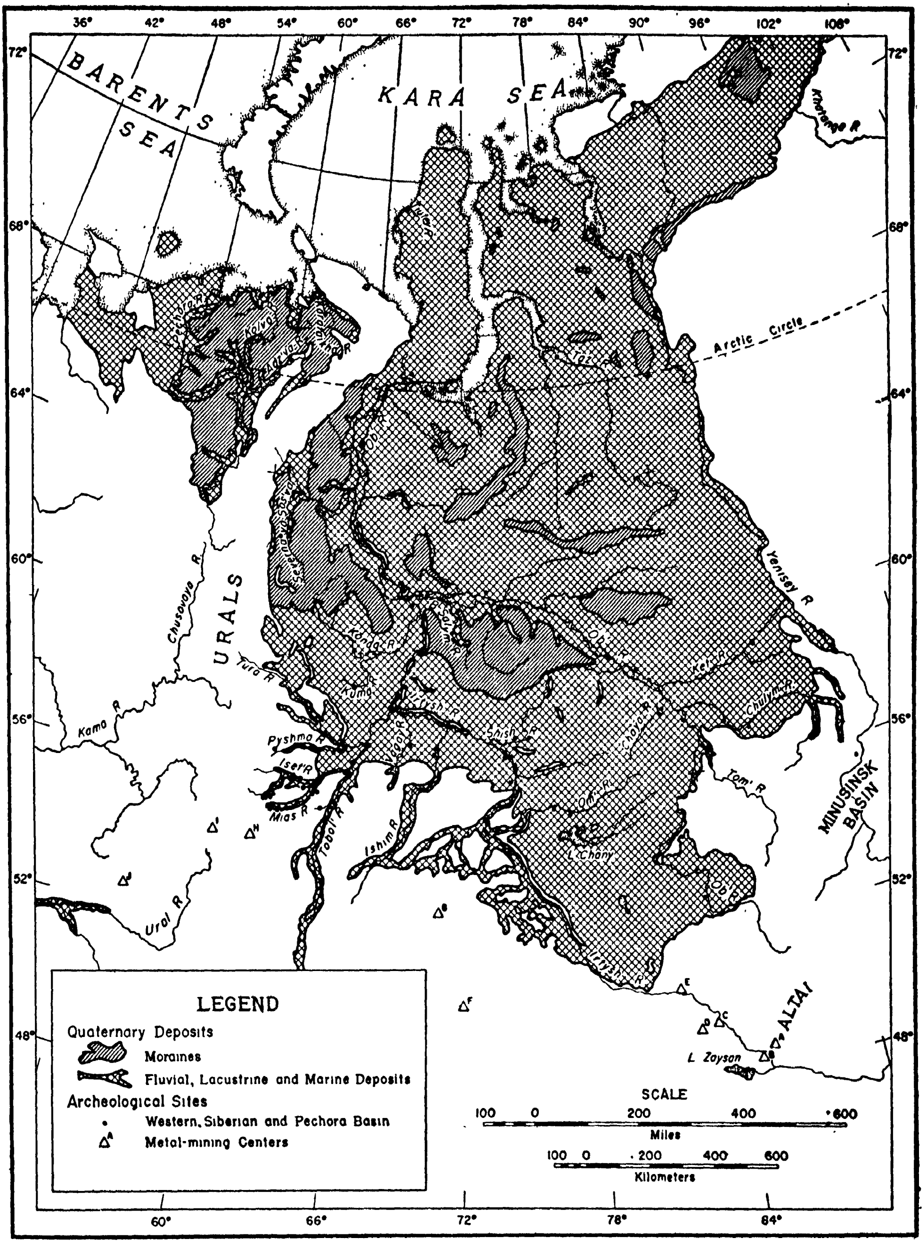
36°
42°
48°
54°
60°
66°
72°
78°
84°
90°
96°
102°
108°
72°
68°
64°
60°
56°
52°
48°
BARENTS SEA
KARA SEA
Khatanga R
Arctic Circle
Pechora R
Kolva R
Adz'va R
Usa R
Taz R
Ob R
Severnaya Sos'va R
URALS
Chusovaya R
Kama R
Tura R
Konda R
Salym R
Kuma R
Irtysh R
Pyshma R
Iset' R
Mias R
Tobol' R
Ishim R
Vagai R
Shish R
Om' R
Ob R
Ket R
Chulym R
Yenisey R
Tom' R
L. Chany
MINUSINSK BASIN
ALTAI
L. Zaysan
Ural R
A
B
C
D
E
F
G
H
I
J
LEGEND
Quaternary Deposits
Moraines
Fluvial, Lacustrine and Marine Deposits
Archeological Sites
Western Siberian and Pechora Basin
Metal-mining Centers
SCALE
100 0 200 400 600
Miles
100 0 200 400 600
Kilometers
60°
66°
72°
78°
84°

Fig. 1. Western Siberian Physiography and Archeological Sites

which were deposited on coarse sands underlying sand dunes and below present water levels, boasted of innumerable fish scales but almost no mammal or bird bones. Conditions were probably better in the moraine uplands of the Sever'naya Sos'va, which was also drier then than today (Sortynya I). In contrast, the early Pechora sites were all located well above present water levels. (For sources on sites see Figure 2.)

By 1500–1200 B.C., grasslands and a rich ungulate fauna had been reestablished in Western Siberia. In fact, the northern margin of the steppe was perhaps 150 miles further north than now. At Tomsk, Kurgan and Alakal', remains of this period and slightly later are associated with black earth (chernozem) overlying earlier sandy and clayey formations. Today these regions are characterized by leached (degraded) chernozems, by podzols or by flood plain soils.[10] Also, horses, cattle, and later sheep and swine were bred as far north as Mys (57° 10′N) during the Middle Bronze Age. Further east, at the same latitude, the terminal Neolithic burial at Ust'Kurenga enclosed a necklace of wild-boar tusks, signifying the presence of deciduous trees. Walnut in Bronze-Age pollen at Gorbunovo in the Central Urals and Arkhangel'sk on the White Sea also indicates a considerable northward shift of mixed forest in Western Siberia.[11] The timber line was then about 70°N, a displacement of 200 miles from the present limit.[12]

Over the next millennium, the climate became still warmer yet somewhat wetter. At Chuvashskiy Mys (58° 10′N) the soil was clayey in 700–900 B.C., but had become chernozem by 500–300 B.C., a 300-mile extension of the present limit. At this site, barley and oats were cultivated, and horses raised. Yet the hoes were antler from moose or reindeer, and fish rather than livestock were the base of subsistence. Also, at Chudaki, to the southwest, black earth, horses, cattle and sheep denoted a grasslands ecology, but wild boar, roebuck, moose and beaver marked the proximity of wet forests.

The Early Iron Age, near the beginning of the Christian era, had a climate much like today's. At Ust' Polui, near the mouth of the Ob', reindeer and fish were basic foods, but walrus, Arctic fox, water fowl, squirrel, and sable were also hunted. Dogs served for transport and as sacrificial animals. To the west, in the Pechora Basin, the old culture had largely disappeared, and the sacrificial site of Khaybede Peder, with its numerous reindeer skulls, testified to a tundra environment. Finally, the presence of sheep, as well as cattle and horses, at Bol'shoy Log, near present-day Omsk, indicates conditions no harsher than the present.

Climatic indicators are almost lacking in data on the first millennium A.D. However, a horse bit and representations of cattle and rams at Barsov Gorodok (61° 15′N) indicate warm and dry conditions. So too do the remains of pine apparently associated with the terminal phases of Ust' Polui. Finally, it is clear that a climate somewhat warmer than today's characterized the period of A.D. 900–1300. On the Yamal peninsula, at Tiutey-sale, walrus and seal were primary, reindeer and Arctic fox, secondary, economic species. Zelenaya Gorka, near Ust' Polui, had both reindeer and Arctic fox. Yet horses, cattle, sheep and swine were raised as far north as Samarovo (60° 58′N) and Isker (50° 06′N), while both wild boar and aurochs testify to mixed forests near the latter site.

This then is the ecological background of Western Siberian archeology. What do we know of its content?

A considerable amount has been done on this subject. A. V. Adrianov (1892), Heikel (1894), Kuznetsov (1899), Martin (Arne, 1935), Plotnikov (1901), Slovtsov (1892), and Spitsyn (1906) laid the foundations. The greatest contributions have, however, been made by Soviet scholars of the past quarter-century, especially V. S. Adrianov (1936), Boch (1937), Chernetsov (1935; 1947a, b; 1949, 1953), Dmitriyev (1939, 1948, 1951), Gryaznov (1949, 1952), Levasheva (1950), Moshinskaya (1953), and Salnikov (1947, 1951, 1952). About a thousand sites have been reported; the general nature and relative chronology of Western Siberian culture have been established. Nevertheless, the entire area north and east of the Ob' and Ket rivers remains unknown, while scientific excavations elsewhere have been limited perhaps to two dozen sites.

The absolute chronology of Western Siberian sites has been built up by typological comparisons with the Kama Basin, the Urals, north Kazakhstan, the Altai, and the Minusinsk Basin, reinforced by a few Middle Eastern and Chinese trade objects. Serious uncertainties remain for the period A.D. 100–600, while dates for the Neolithic and pre-ceramic periods are at most approximate. Within these limits of confidence, the developmental sequence of Western Siberia may be summarized as follows:

1. Apart from one Upper Paleolithic campsite at Tomsk, the few known pre-ceramic sites in Western Siberia relate to the Kel'teminar groups of cultures of Central Asia, Kazakhstan and the southern Urals, which are characterized by finely retouched prismatic blades, often set into bone sickle-knives; single-tanged arrow points, slate knives and, in the later phases, shallow, incised, pottery bowls.[13] Of the four sites reported, Chudatskaya Gora is on the upper Ob'; the others are on western branches of the Tobol'. One site on the Ad'zva river in the Pechora Basin may also relate to this complex. A conservative dating (Third Millennium B.C.)[14] appears justified by the finding of a Kitoi-type compound fish hook at Chudatskaya Gora, and by the fine workmanship of the Andreyevskoye Ozero points.

2. The late Neolithic was marked by a major flow of settlement, clearly originating from the Central Urals (especially Poludenka) in the west,[15] and the Minusinsk Basin (especially Yarki) in the east.[16] The earliest focus of western settlement appears to have been at Andreyevskoye Ozero (Kozlova Pereyma), which alone retained prismatic blades, accompanied albeit with cord-impressed pottery. Later sites extended to the Adz'va and Shchuchya rivers in the north, and to Yekaterininskoye in the east. Common features of the culture included rectangular semi-subterranean houses; pots of truncated-egg shape, with over-all pit, punctate, and impressed-comb decorations organized in horizontal bands; tools of retouched flint and of slate, including lanceolate arrowpoints and semilunar knives; polished adze-heads of rectangular cross-section, and polished, hafted, semilunar scrapers. The eastern complex is known only from burials, at Tomsk, Chudatskaya Gora, and Ust' Kurenga. It is characterized by wide, flat-bottomed pots with over-all pit decoration; by chipped and retouched as well as polished ax-adze heads; sharing lanceolate points and the polished scrapers with the west. East–west trade along the Ob'-Severnaya Sos'va is indicated by the corresponding distribution of ground, two-lugged ax-heads, an old Eastern Siberian type appearing late in Europe. The dating of these sites from about 2000 to 1200 B.C. rests upon correspondences with the Minusinsk Basin, Central Urals, and even North Russia.[17]

Fig. 2. West Siberian and Allied Sites; A Selective Tabulation (Key to Map)

River and site group	Location		Periods represented							References	
	N ° ′	E ° ′	Pre-Ceramic (3d Millennium B.C.)	Late Neolithic (Ca. 2000–1200 B.C.)	Bronze		Iron			Site No. (Talitskaya, 1953)	Other
					Middle (1200–800 B.C.)	Late (800–300 B.C.)	Early (300 B.C.–300 A.D.)	Middle (300–900 A.D.)	Late (900–1300 A.D.)		
I. WESTERN SIBERIAN AND PECHORA BASIN SITES (IN DOWN-RIVER SEQUENCES)											
Ket											
Ob'-Yenisey Canal	59 20	89 20	—	+	—	—	—	—	—	—	Chernetsov *et al.*, 1953, p. 11.
Chulym											
Cherdatskoye	56 50	87 00	—	—	—	—	—	+	—	95	—
Mitrofanovka	57 08	85 50	—	—	—	—	(+)	—	—	76	—
Tom'											
Shelomok Gorodok	56 23	84 50	—	—	—	—	—	—	+	118	Levasheva, 1950.
Basandayka	56 26	85 00	—	—	—	—	+	—	+	113	Chernotsov *et al*, 1953, p. 223.
Tomsk	56 28	84 58	—	+	+	+	(−)	+	—	107–9	Adrianov, 1892; Komarova, 1952.
Toyanov Gorodok	56 28	84 50	—	—	—	—	—	—	+	105	Levasheva, 1950.
Arkhiyereyskaya											
Zaimka	56 35	84 55	—	—	—	—	(+)	+	—	103	Kuznetsov, 1899.
Chernilishchikova	56 42	84 44	—	—	—	—	—	—	+	102	Levasheva, 1950.
Ob' (above Ket)											
Blizhniye Yelbany	52 45	83 45	—	—	(+)	+	+	+	(−)	—	Gryaznov, 1947, 1949, 1952.
Chudatskaya Gora	53 25	83 15	+	+	?	?	+	(−)	(−)	—	Kiselev, 1951, p. 22; Komarova, 1952, p. 13, Chernetsov *et al*, 1953, pp. 223–4.
Molchanovo	57 25	83 40	—	—	—	—	—	—	+	75	Sinyayev, 1950.
Chaya											
Podgornoye, Kulayka	57 42	82 30	—	—	—	—	+	—	—	69–70	Chernetsov *et al*, 1953, p. 127.
Kolyvan'	57 50	82 40	—	—	—	+	—	—	—	73	—
Lake Chany											
Chinyaikha	54 55	77 40	—	—	—	—	—	—	+	—	Levasheva, 1950.
Om'											
Osintsovo	55 27	77 25	—	—	—	(+)	—	—	—	749	—
Tontur (Voznesenskoye)	55 35	76 25	—	—	—	—	—	—	+	733	Levasheva, 1950.
Bol'shoy Log	55 00	73 30	—	—	—	?	(+)	(+)	(+)	727	Chernetsov *et al*, 1953, pp. 101, 184, 224.
Shish											
Ust' Kurenga	57 25	75 40	—	+	—	—	—	—	—	200	Chernetsov *et al*, 1953, pp. 34–6.
Imshegal	57 25	74 45	—	—	—	—	—	(+)	(−)	199	—
Irtysh (above Ishim)											
Balandino	54 25	74 20	—	—	+	—	—	—	—	244	—
Omskoye Selishche	54 55	73 15	—	+	+	—	—	—	—	240–2	Chernetsov, 1947a.
Omsk	54 57	73 25	—	—	+	—	—	—	—	238	—
Kokonovka	55 10	73 15	—	—	—	—	(+)	—	—	233	Chernetsov *et al*, p. 101.
Nadezhdinskiy	55 15	73 15	—	—	—	—	—	+	—	231–2	—
Takmyk	56 15	74 35	—	—	—	—	—	—	(+)	224	—
Murlinskiye Yurty	56 40	74 40	—	—	—	—	+	—	—	222	Chernetsov *et al*, 1953, pp. 150–6.
Tara (vicinity)	56 50	74 20	—	—	—	—	+	+	+	217	—
Yekaterininskoye, Bezymyannoye	56 50	74 30	—	+	?	+	—	—	(+)	208–15	Chernetsov, 1947a.
Krasno-ozerka	57 00	74 20	—	—	(+)	—	—	—	(+)	206–7	Chernetsov *et al*, 1953, p. 58.
Ishim											
Kargaly	57 02	70 32	—	—	+	(+)	—	—	—	690	Chernetsov *et al*, 1953, p. 52.
Serebryanka	57 10	70 35	—	—	—	(+)	—	—	—	687	—
Vagay											
Istyatskiye Yurty	57 15	69 08	—	—	—	—	+	—	—	188	Heikel, 1894; Chernetsov *et al*, 1953, pp. 162–71.

Tobol											
Verkhne Alabuzhskoye	54 35	64 55	?	+	—	—	—	—	—	373	—
Klyuchevskaya	55 05	65 05	—	—	+	—	—	—	—	—	Heikel, 1894, pp. 57–8.
Chudaki	55 22	64 37	—	—	—	+	—	—	—	358	Sal'nikov, 1947.
Kurgan	55 25	65 18	—	—	+	+	?	+	—	347	Heikel, 1894, pp. 101–6; Dmitriyev, 1948.
Paderinskoye	55 35	65 38	—	—	—	+	—	—	—	339	Dmitriyev, 1948.
Tomilova (Yalutorovsk)	56 39	66 19	—	—	+	+	+	—	—	290	Heikel, 1894, pp. 91–4.
Borovaya	57 10	67 00	—	—	—	+	(+)	+	?	281–4	Heikel, 1894, Pl. XIII, Nos. 1–4.
Mias											
Pivkino	55 15	62 22	—	—	—	+	—	—	—	—	Sal'nikov, 1947.
Alakul'	55 15	62 30	—	—	+	—	—	—	—	—	Sal'nikov, 1952.
Tatarskiy Bor	55 45	64 20	+	+	—	—	—	—	—	559	Sal'nikov, 1951, p. 97.
Baklanskoye	55 50	64 23	—	—	+	(+)	—	—	—	557–8	Sal'nikov, 1951, p. 99.
Sinara-Karabolka											
Novo-Burinskoye	55 50	61 30	—	+	+	—	—	—	—	—	Sal'nikov, 1951, pp. 97, 110.
Bol'shaya Kyzylova	55 52	61 45	—	—	—	—	+	—	—	621	—
Iset'											
Maksimova	56 10	63 15	—	—	—	+	—	—	—	587–8	—
Zamarayevskoye, Barminskoye	56 08	63 30	—	+	+	(+)	—	—	+	582–6	Sal'nikov, 1951, p. 98 ff.
Peschano-Tavolzhanskoye	55 50	63 50	—	—	—	+	—	—	—	572	Dmitriyev, 1948.
Ust Miasskoye	56 05	64 20	—	?	+	?	+	—	—	555	Tolstov and Zhdanko, 1952, pp. 193–6; Sal'nikov, 1951, p. 97.
Lipnyagova	56 08	64 30	—	—	—	(+)	—	—	—	567	—
Krasnogorsk	56 25	65 55	—	—	+	+	—	—	—	536	Chernetsov *et al*, 1953, pp. 44–5.
Pyshma											
Kashinskoye	56 50	62 10	—	—	—	(+)	—	—	—	452	—
Chernorechenskiy (Baldinka)	56 55	65 15	—	—	—	—	+	—	—	449–51	Heikel, 1894, Pl. XXII.
Andreyevskoye Ozero	57 00	65 35	+	+	+	+	—	—	—	443–4	Dmitriyev, 1939; Chernetsov *et al*, 1953, pp. 25–30, 53.
Andryushin Gorodok	57 05	65 40	—	—	—	+	—	—	—	441	Moshinskaya and Chernetsov, 1953.
Tura											
Lipchinskoye	57 25	64 50	—	+	—	—	—	—	—	481	Sal'nikov, 1951, p. 99; Chernetsov *et al*, 1953, pp. 36–40.
Mys	57 10	65 35	—	(+)	+	+	—	—	—	471–3	Sal'nikov, 1951, pp. 98–9.
Irtysh (from Ishim to Ob')											
Novonikol'sk	57 47	71 05	—	—	—	—	(+)	—	?	192–4	—
Isker	58 06	68 40	—	—	—	(+)	+	(−)	+	179	Levasheva, 1950.
Ivanovka	58 10	68 20	—	—	—	+	—	—	—	175–7	Chernetsov *et al*, 1953, p. 50.
Savina	58 10	68 05	—	—	—	—	+	—	—	174	Chernetsov *et al*, 1953, pp. 217–18.
Tobol'sk (Chuvashskiy Mys)	58 08	68 12	—	—	+	+	+	+	+	171–3	Heikel, 1894; Chernetsov, *et al*, 1953, pp. 189–220.
Suzgun	58 20	68 20	—	—	—	+	—	—	+	167–8	Chernetsov *et al*, 1953, pp. 48–51.
Tyukova	58 25	68 28	—	—	—	—	+	—	—	166	Chernetsov *et al*, 1953, pp. 135, 217.
Durynina	58 30	68 28	—	—	—	—	+	—	—	164	—
Nefedovo	58 35	68 45	—	—	—	—	+	—	—	162	—
Samarovo	60 58	69 04	—	—	—	+	+	(−)	+	152	Heikel, 1894, Pl. XXII; Levasheva, 1950; Chernetsov *et al*, 1953, p. 53.
Kuma											
Laytamak	58 45	66 00	—	—	—	+	—	—	—	—	Chernetsov *et al*, 1953, p. 51.
Konda											
Orautur	60 50	63 40	—	—	—	—	(+)	—	—	259	Boch, 1937.
Leushi	59 38	65 46	—	—	—	+	—	—	—	—	Boch, 1937.
Ob' (from Ket to Irtysh)											
Kondisovskiy	58 56	81 33	—	—	—	—	—	—	+	—	Heikel, 1894, Pl. XXIII, No. 1.
Barsof Gorodok	61 15	73 20	—	—	—	—	—	+	+	37–9	Arne, 1935

Fig. 2. West Siberian and Allied Sites; A Selective Tabulation (Key to Map)—*continued*

River and site group	Location		Periods represented							References	
	N ° ′	E ° ′	Pre-Ceramic (3d Millennium B.C.)	Late Neolithic (Ca. 2000–1200 B.C.)	Bronze		Iron			Site No. (Talitskaya, 1953)	Other
					Middle (1200–800 B.C.)	Late (800–300 B.C.)	Early (300 B.C.–300 A.D.)	Middle (300–900 A.D.)	Late (900–1300 A.D.)		
Bol'shoy Salym											
Leng Ponk	61 03	70 45	—	—	—	—	—	+	—	33	Chernetsov *et al*, 1953, p. 133.
Kintusovskiy	60 10	71 25	—	—	—	—	+	+	(−)	35–6	Chernetsov *et al*, 1953, p. 133.
Ozero Chagyr	59 40	72 20	—	—	—	—	—	+	(−)	—	Chernetsov *et al*, 1953, p. 133.
Ob' (from Irtysh to mouth)											
Vozh Pay	62 55	65 30	—	—	—	—	+	(−)	+	23–6	Chernetsov *et al*, pp. 114–16.
Yurt Vezhakarskikh	63 25	65 30	—	—	—	—	—	—	+	20	—
Kushevat	65 00	65 25	—	—	—	+	—	—	—	17	Chernetsov *et al*, 1953, p. 64.
Salekhard	66 32	66 38	—	—	+	+	—	—	—	7–9	Chernetsov *et al*, 1953, pp. 179–88.
Ust' Poluy	66 35	66 40	—	—	—	—	+	—	—	4	Chernetsov *et al*, pp. 72–148; Adrianov, 1936.
Zelenaya Gorka	66 37	66 42	—	—	—	+	+	—	+	1–3	Chernetsov, 1949.
Severnaya Sos'va											
Nyaksimvol'	62 25	61 00	—	+	—	—	+	—	—	148	Chernetsov *et al*, 1953, pp. 14, 116.
Khulyum-sunt	62 47	61 40	—	+	—	—	—	—	—	147	Chernetsov *et al*, 1953, pp. 18–21.
Sartynya	63 26	63 05	—	+	(−)	+	—	—	—	135	Boch, 1937.
Toboldiny (Us-Nël)	63 08	63 25	—	—	—	—	—	(+)	?	133	Chernetsov *et al*, 1953, p. 231.
Berezovo (Sorovoy Mys)	63 55	65 15	—	—	—	+	+	(−)	(+)	127–8	Heikel, 1894, Pl. XX, Nos. 1–2.
Lyapin											
Saranpaul' (Chës-tyy-yag)	64 12	60 55	—	+	—	—	+	—	(+)	140–5	Boch, 1937; Chernetsov *et al*, 1953, p. 113.
Synya (exact site unknown)	65 0	64 0	—	—	—	—	—	—	+	16	—
Shchuchya	67 0	68 30	—	(+)	—	—	—	—	—	—	Chernetsov *et al*, 1953, p. 24.
Tiutey											
Tiutey-sale	71 21	67 32	—	—	—	+	—	—	+	—	Chernetsov, 1935; *Ibid.*, 1953, p. 65.
Taz											
Zimovya Mamayëva	67 20	79 10	—	—	—	+	—	—	—	—	Chernetsov *et al*, 1953, p. 48.
Karataikha	68 20	62 00	—	—	—	+	—	—	—	—	Foss, 1952, pp. 138–45, 254, Nos. 89–93.
Khaybede Peder	67 50	60 00	—	—	—	+	+	+	+	—	Foss, *loci cit.*, Nos. 86–8; Chernov, 1951.
Adz'va	67 00	60 00	?	+	+	+	—	—	—	—	Foss, *loci cit.*, No. 96; Chernov, 1954.
Kolva	67 20	58 00	—	(+)	+	+	—	—	—	—	Foss, *loci cit.*, Nos. 101–15.
Chernaya	68 30	57 00	—	—	—	+	—	—	—	—	Foss, *loci cit.*, Nos. 80–4.
Kui	67 40	53 30	—		—	+	—	—	—	—	Foss, *loci cit.*, Nos. 74–9.
II. METAL-MINING CENTERS: SOURCES OF WEST SIBERIAN COPPER, TIN AND GOLD											
Irtysh Group											
Karchiga (A)	48 35	85 20	—	?	(+)	(+)	(−)	—	—	—	Chernikov, 1949, p. 38.
Ashibulak (B)	48 20	84 59	—	?	(+)	(+)	(−)	—	—	—	Chernikov, 1949, p. 36.
Krykchuruk (C)	49 30	83 20	—	?	(+)	(+)	(−)	—	—	—	Chernikov, 1949, p. 26.
Sentash (D)	49 19	82 30	—	?	(+)	(+)	(−)	—	—	—	Chernikov, 1949, p. 38.
Ubinskoye (E)	50 17	81 44	—	?	(+)	(+)	(−)	—	—	—	Chernikov, 1949, p. 10.
Stalinsky Rudnik (F)	52 25	72 00	—	—	+	+	—	—	—	—	Chernikov, 1954.
Stepnyak (G)	52 50	70 50	—	—	+	+	—	—	—	—	Chernikov, 1954.
Southeast Urals Group											
Uvel'ka (Placers) (H)	54 30	61 10	—	(−)	(+)	?	?	?	?	375–87	Sal'nikov, 1951, p. 127.
Toguzak (Placers) (J)	54 30	60 00	—	(−)	(+)	?	?	?	?	390–6	Sal'nikov, 1951, p. 127.
Southwest Urals Group											
Kargaly (K)	52 40	56 00	—	(−)	+	?	?	?	?	—	Sal'nikov, 1951, pp. 126–7.

3. The Bronze-Age settlements of Western Siberia may be divided into two groups, the older (1200–800 B.C.), which reflect late Andronovo loans and are largely limited to the southern regions of the area; and the younger (800–300 B.C.), which manifest broad contacts with the Minusinsk and Kama Basins as well as the steppe, and which reach north to the Kara Sea.[18] Steppe immigrants blended with the Neolithic populations, judging from the stability of house forms, and the gradual transition in pottery. Though flat bottoms, geometric ornament and square, lugged, dishes predominated, decoration by pits and comb-marks remained. Hoe agriculture, cloth, and domesticated horses, cattle and sheep were introduced. Cast bronze metal wares (probably imported in exchange for agricultural products) included sickles, socketed adze-heads, single-edged, hafted knives, lanceolate spear points, small mirrors with back loops, and buttons. Stone work included traditional large chipped knives, polished scrapers and adzes, as well as new pestles, querns, and whetstones. Burial rituals became elaborate and diversified, with burnt offerings, horse sacrifices, birch coverings and conical mounds a frequent combination.

The younger period was distinguished by the rise of compact, generally circular settlements, located on commanding heights and protected by earth walls and moats. Only in the Yamal and Pechora regions could nomads' campsites be found. Everywhere, the character of the pottery changed; pot-bellied bowls decorated only on the lip, neck and shoulder became the fashion. Individual designs, such as inverted triangles of round pits, were found the length of the Ob', from Blizhniye Yelbany to Zelenaya Gorka. Local differences, such as the northern preference for small, stamped decorations, can also be noted. Bone, especially ungulate metatarsals, became the basic material for everyday implements, especially lanceolate, hafted arrowpoints, collared harpoons; fish knives and spoons. Local types of bronze celts (socketed adze-heads) developed and spread. New types of bronze imports included three-sided, tanged, socketed arrow-points; pick-axes of Tagar type; daggers; horse bits and cheek pieces; tubular beads, ring-and-spiral earrings, figure-eight buckles, and sculptures of animals in static pose. Copper cauldrons with a circular stand and loop handles were another innovation, as were a few iron objects: a loop-handled iron knife (Tagar), a Scythic dagger, and an iron bit. From Ananino came perforated, pottery disks, presumable solar images; from the steppe, ram-headed sacrificial stones.

4. The long Iron Age of Western Siberia (300 B.C.–A.D. 1300) comprises three distinct phases: the first, strongly represented in the west up to the Arctic, was distinguished by a meeting of traits introduced from the Kama (Pyanobor culture) and from the far north; the second, essentially an eastern complex, had intimate relations with the Turkic cultures, lesser ones with the Lomatovskaya culture of the west, and intensifying trade with the Middle East.[19] In the third phase, Western Siberia was submerged by the surrounding cultures, becoming a fur supplier, market and vassal of Ugrians, Bulgars, and Tatars.

Two sites, Chuvashskiy Mys (or Potchevash) on the Irtysh, and Ust' Polui on the Lower Ob', were cultural foci in the first phase of the Iron Age. Chuvashskiy Mys continues older traditions in its pottery, bone work, and burial rituals. New borrowings included hafted knives and socketted spearpoints of iron, glass beads, double-disk and disk-and-comb brooches of bronze. Rectangular plaques with bas-relief representations of men and beasts show a virtuosity in bronze, and also disclose the local use of parkas, breeches and boots. The finds at Ust' Polui exhibit comparable skill, in articles such as miniature picks, "eared" celts, plaques and sculptured images of many kinds. Bone work included both armor, and ornamented combs and spoons. The pottery at Ust' Polui was provided with high cylindrical bases, a feature probably borrowed from the Minusinsk Basin.[20] Animal gear included dog harnesses and halter parts for a decoy reindeer.

The growing trade and cultural syncretism of the early centuries of the Christian era are exemplified by the Istyatskiy hoard. It contained a variety of objects, especially conical gilded helmets, perhaps of Sarmatian origin, a Greco-Bactrian silver-gilt bas-relief of Diana, and many mirrors, one Chinese (of Han age). A provincial impress was lent to many of these artifacts by their adornment by crudely scratched representations of beavers, grouse, a bullock rider, and a monster bird with a Turkic deity's crown,[21] to mention but a few. A Khorezmian silver vase, Sassanian and Roman coins, and Baltic amber buttons have been found in other Western Siberian sites.

The second phase of the Iron Age (A.D. 300–900) was especially developed at Tomsk and Barsov Gorodok on the middle Ob', and distinguished by great conservatism in pottery, some diminution in bone work, by increasing variety but quantitative scarcity in iron implements, and by a vigorous naturalistic style applied to metal belt-ornaments, and cast, anthropomorphic animal sculptures. Forging began to replace casting; iron hoe-blades, awls, chisels, shears, and strike-a-lights are found, albeit sparingly. Personal adornment faithfully reflected the styles depicted in the grave images of Turkic princes in Mongolia;[22] Chinese coins at Tomsk further reflect eastern ties.

After the tenth century, in the Late Iron Age, large fortified settlements developed in localities commanding trade routes.[23] House types became varied, ranging from circular pit-huts in the Arctic to above-ground wattle structures in the south. Iron, locally smelted from bog ores, grew common. Old types of ornaments deteriorated into crude conventionalism; imports included copper-wire, filigree, and musical ball-and-chain pendants of western types. The pottery of the period was highly variable, often exhibiting archaicisms, such as over-all decoration. In the north, it apparently disappeared. During this period, the resources of Western Siberia were increasingly exploited; numerous hoards of coins and imported brass vessels testify to accumulated wealth. But Western Siberian culture was no longer vital, and subsequent innovations were to come from succeeding waves of immigrants.

Let me come to some general conclusions. In my view, the harsh environment of Western Siberia long delayed settlement. Once population growth had begun, however, both trade and the northward spread of grasslands promoted further development. By perhaps 500 B.C., the area had attained a cultural identity which it was to maintain for some 1500 years. Metal work achieved artistic heights, despite a lack of local resources. Reindeer domestication, evidenced earliest in the Altai,[24] was probably transformed into reindeer pastoralism in the vicinity of Ust' Polui in the centuries when the northward retreat of the tundra no longer permitted a sedentary, mixed economy at a strategic juncture of water, tundra and forest. With the advent of colder climate after the tenth century A.D., reindeer pastoralism diffused, via the Samoyed, to the Ugrians, and Zyryan in the west; to the Yukagir and, perhaps, Chukchian peoples, in the east.[25] Moreover, it is clear, from linguistic and ethnological data, that Western Siberia reflects Samoyedic culture history. In the southeast, the Kettic peoples too unquestionably played significant roles, while the Ugrians were both late and minor actors.[26]

Finally, this examination of one region of northern Eurasia brings out ecological peculiarities, broad contacts with the greater cultures, local innovations, advances and retrogressions. In light of these facts, the undeniable cultural similarities which extend from Norway to Newfoundland must be interpreted with great caution and reserve.

Bureau of the Census,
Washington, D.C.

Notes

1. Mironov, *et al.*, 1951.
2. Suslov, 1947, pp. 5–79; Borisov, 1948, pp. 117–118, 138–164, maps.
3. See Krasheninnikov, 1954, esp. pp. 226 ff., and Berg, 1949, pp. 1243–1244, 1262–1264.
4. Berezovskiy, 1931.
5. Suslov, 1947, *loc. cit.*
6. Pereleshin, 1943, and Kiris, 1944.
7. For a general exposition of paleoclimatology in the light of the North Atlantic heat-moisture balance see Ewing and Donn, 1956.
8. Suslov, *op. cit.*, p. 10; Talitskaya, 1953, No. 109.
9. Krasheninnikov, 1954, p. 228; Tolstov, 1954, p. 246.
10. Suslov, 1947, Map 5; see also Sal'nikov, 1951, pp. 120–126.
11. Bryusov, 1952, pp. 149–163; Foss, 1952, pp. 24–29.
12. Suslov, 1947, p. 11; Foss, 1952, p. 25.
13. Tolstov, 1946, pp. 61–65; 1954, pp. 239–246; Formozov, 1951.
14. For a basic discussion of Ural neolithic chronology see Bryusov, 1953.
15. On Poludenka see Bader, 1949; Raushenbakh, 1952.
16. On Yarki see Komarova, 1947.
17. For other typological comparisons with the Urals and North Russian Neolithic see Bryusov, 1952, pp. 149–163; Dmitriyev, 1951; and Foss, 1952, esp. pp. 194–203.
18. The chronology of Bronze-Age sites in Western Siberia has been established through comparisons with data in Chernetsov, 1947b; Chernikov, 1949, 1954; Dmitriyev, 1948; Kiselev, 1951; Liberov, 1954; Sal'nikov, 1951, 1954; and Zbruyeva, 1952.
19. Crucial sources for Western Siberian Iron-Age chronology are Gryaznov, 1949, 1952; Kiselev, 1951; Levasheva, 1950; Smirnov, 1949, 1952; Spitsyn, 1902; and Talitskiy, 1951.
20. In the Minusinsk Basin this feature was common throughout the Tagar period; in East Russia it appeared only—and then rarely—in middle Pyanobor (Koshibeyev).
21. Kyzlasov, 1949.
22. Compare the ornaments in Adrianov, 1892, and Arne, 1935 with Yevtyukhova, 1952.
23. For a brilliant summary of the Late Iron Age in Western Siberia see Levasheva, 1950; Arne, 1935, and Chernetsov, 1935 are also useful.
24. Kyzlasov, 1952.
25. On the diffusion of the reindeer complex to the Ugrians and Zyryan see Donner, 1927 and Wichmann, 1902. The data for the Yukagir and Chukchian peoples I hope to present in a future study. Tungusic reindeer breeding appears to derive directly from the most ancient Altai-Sayan domestication, which in turn was a transfer from bullock-breeding rather than horse culture. Lapp domestication was of local origin, probably derived from Old Norse.
26. This view rests upon the content of the proto-Samoyedic vocabulary, especially the presence of pre-Orkhon Turkic loan words (Donner, 1924); on the geographical distribution of Samoyedic, including the isolated southern languages (Dolgikh, 1952); and on the persistence of numerous resemblances in the living cultures of the Samoyed, especially the Tavgi, with Chuvashskiy Mys and later Western Siberian archeology (Popov, 1948). The Ugrians (Ostyak, Vogul) share many of these resemblances but

their late arrival in this area is testified to by the absence of Turkic loan words earlier than Tatar (Paasonen, 1902), and by toponymic and cultural evidence linking these groups closely to East Russia (Kannisto, 1927 a, b; Talitskiy, 1951, especially pp. 64–88). The evidence from Scythic-Alanic loan words in Ugrian lacks weight, since these loans are also found in Mordvin (Bouda, 1939). Thus the Chernetsov, 1946–Gryaznov, 1952 thesis must be rejected.

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Abbreviations:

FUF	*Finnisch-Ugrische Forschungen*, Helsinki.
KS	*Kratkiye Soobshcheniya Instituta Istoriyi Material'noy Kul'tury*, Akademiya Nauk, Moscow.
MIA	*Materialy i Issledovaniya po Arkheologiyi S.S.S.R.*, Akademiya Nauk, Moscow.
SA	*Sovetskaya Arkheologiya*, Moscow.
SE	*Sovetskaya Etnografiya*, Moscow.

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SECTION VI

PHYSICAL ANTHROPOLOGY

PHYSICAL AND PSYCHOLOGICAL FACTORS IN CULTURE GROWTH

J. Lawrence Angel

With the appearance of modern man, from the Würm glaciation onwards, culture has grown or evolved materially at a curiously accelerating rate. Why? Culture in large part consists in action-binding symbols which (1) enable a society to control its environment and (2) enable the individual to play a satisfying rôle in this control. It is tempting to deduce that such symbol systems must add to themselves largely in ratio to expansion of population and consequent diffusion of ideas in different geographic areas and under limitation only by factors of total ecology and of time. Obviously, however, there is a human limitation in the degree of complexity of the human sense-organs and central nervous system as well as bodily strength. Is this human limitation the same at all times and places, or are there additional positive human factors in culture growth?

I suggest that there are and that their importance relative to the self-additive effect of culture is not fixed. I will try to identify some of these, using as example Greece over the past five millennia. I remind you now that no such historical experiment can be "controlled" in the scientific sense so that associations may have no causal meaning and that regularities which recur will not necessarily work in another context.

Anthropologically interesting variables include (1) size of population in an ecological area, and age, sex, race, and class composition of that population; (2) health, longevity, bodily strength, size and growth pattern as related to diet and environment; (3) evolutionary change and critical gene frequencies; and (4) selective attitudes toward new ideas, things, or people, as related to density and arrangement of settlement, interaction conditioned by these and pressures from neighboring groups. Physical anthropology in the past has confined itself to only a few of these. As it now develops work on physiological and psychological processes crucial in human biological evolution, physical anthropology must inevitably take up these variables singly but in cooperation with other disciplines.

Through the help of many others and with support from Harvard University, Guggenheim and Wenner-Gren Foundations, and the American Philosophical Society, I have been able to repair and study over 600 skeletons and 1200 skulls dating from Neolithic times (pre 3000 B.C.) down to the 19th century A.D. Size of population cannot be derived from skeletons with the exactness of the model work of Professor János Nemeskéri reported at this Congress: although my material comes almost entirely from the fairly unified culture area extending northeast and southwest across the isthmus of Corinth, the diversity of sites at different periods as well as excavation selection normal in sites dug before the 1930's prevent the ideal of a generation by generation study. As seen in the accompanying table changes in level of culture as judged by usual subjective criteria (inventiveness, creative activity, foreign contacts, economic development, etc.) do run parallel to changes in size of population. From scattered village groups in the third millennium B.C. the population grew steadily during pre-Greek times and again after the Greek conquest up to a dense occupation of available farmland under the late Bronze Mycenaean aristocracy. After the Early Iron Age lapse and emigrations, density increased at least up to 35 per square kilometer, according to Gomme's estimate for Attica (about the same as in 1880). After this there is a decline, irregularly, especially in the early Medieval phase, to a low of perhaps 10 per square kilometer according to Klon Stephanos' quotation of Turkish occupation census data, followed by recent increase up to 55 per square kilometer with 7 million people. Only 20–25% of the Greek land surface of 120,000 square kilometers (50,000 square miles) is cultivable at all, less for wheat than for barley plus vines, fruits, and the tenderly raised olive. The bareness of mountain areas, especially in the rain-poor east, is partly balanced by the long coastline. It is by sea contacts and imports that relative prosperity and larger populations of Mycenaean, Classic-Roman, and modern periods have been possible.

As in the early Near East the use of manpower was critical. From the biological standpoint, therefore, it is natural to find a steady association between level of culture and average age at death of adults. This shows a change of about 10 years with rise and fall of culture. Perhaps more critical than this change in the average adult life span, with its increase of time for creative leisure, are the high infant and child death rates. At Middle Helladic Lerna we observe that at least 55% of 230 Bronze Age deaths are those of infants and children and 15% over 45; by Classical times 49% of skeletons are those of children and infants and almost 25% are over 45, compared with the U.S.A. in 1900 where 35% of deaths were of infants and children and 42% over 45. A corollary is an excess of female over male deaths in the child-bearing ages, so that women apparently die younger than men in complete contrast to the new modern urban situation. Accurate estimates of pre-adult death rates would probably show more than a 10-year shift, but still the gain in longevity by Classical times was far below the total gain here over the past 50 years, where the rise in adult age of death (*not* life expectancy!) between 1900 and 1950 has been from 54 up to 64. The Greeks never solved the problem of human waste in infancy.

A puzzling lesion which shows a parallel course is cranial osteoporosis. Whether this reflects vitamin-deficiency or the thalassemia heterozygote its decrease may well parallel a general improvement in health with rise in civilization, which in turn made more human energy available, and so on in circular relationship. After this deduction we may expect that improvement in living conditions with culture growth will also produce increase in general body size, just as has happened here. There is no proof that the observed increase is not partly from genetic causes, even though the quite striking excess in body size and robusticity of the Mycenaean "kings" might most easily be explained by a better and more meat-filled diet. Though head size declines by the Turkish period, stature *may* not; our stature data here are largely limited to a single tomb under the re-used temple of Hephaistos in the Athenian Agora. Some of the chronological changes in proportions are presumably allometric. This may be only part of the explanation for the intriguing increase in depth of bony pelvic inlet, which should facilitate childbirth: a direct effect of childhood nutrition is likely here as suggested by the observations of Nicholson on British women growing up during World War I and later.

Dental lesions do not fit this pattern, showing only a major recent increase. Arthritis is puzzling: the males show a decrease with rise in culture, but the females fluctuate. I shall not speculate on this without more data. Except for fractures other disease frequencies cannot be estimated. In particular malaria and dysentery may have been of fluctuating importance. We happen to have gallstones from one of the well-fed Mycenaean kings, but no data on arteriosclerosis, little on cancer.

Turning now to psychological aspects of this chronological development one of the perennial questions has been the impact of foreigners on this crossroads part of Greece. We may note first the exotic origin of single elements in Greek culture before their local development, and second the continual influxes: first Neolithic farmers and a scattering of hunters, then Carians, then Greek-speakers in several regional shifts plus Minoan artisans, then people from all the ports of the east Mediterranean and Black Seas, Persians, Romans, Goths, Huns, Avars, Albanians, Jews, Franks, Spaniards, Venetians, Turks and others. Yet skulls show throughout a remarkable continuity: the micro-evolutionary direction of change, though somewhat spiral and fluctuating, is never so abrupt as to require explanation by replacement by a large population segment at least after 2000 B.C. The Greeks have remained essentially the same people perhaps to a greater degree than other European ethnic groups, perhaps less than Mesopotamians or Egyptians. Change did occur, here schematized by proportions of types (ranged from the most lateral to the more linear, with factors of ruggedness and of face downgrowth added). Change slows as population increases, the relation demanded by evolutionary theory. But the vital point here illustrated is the heterogeneity of population all through. The Sigma ratio of head size and form, face profile, nose shape, and jaw depth shows decrease in variability or some fusion of diverse elements as population increases and cultural achievement presents itself. Is this increased social and biological contact between people of diverse type (and origin?) connected with the intellectual and emotional energy needed for cultural advance?

After looking at a concrete example of three skulls from the Middle Helladic town of Lerna with their complete contrast in form, we note that six skulls from two Mycenaean tombs in the Athenian Agora show a surprising family resemblance with still adequate inter-family contrast. Since we should expect little stimulating effect from hybrid vigor or from genetic recombination *per se*, we are led to ask if it may be the psychological jar of contact between diverse people which raises the level of social energy.

Is this a reasonable hypothesis? Particularly after such an experience as this international meeting we have all noted that variety of experience is stimulating and usually pleasant, while repetitive monotony is boring, fatiguing, or even painful. Two neural processes are vital here. The first is fatigue and recovery, first of a single nerve circuit, where repeated stimulus demands longer rest, and second of the whole central nervous system, where sensitizing of neurons around a fatigued area means that shift to these is felt more keenly, as if pleasant or refreshing. The second observation is the change of threshold, or adaptation, when an irritating sensory stimulus is bypassed at cortical level. Both are important in the Primate and human learning process, since release of tensions of any sort, whether from hunger, action, or even sleep, is felt as pleasure, necessarily transient, but ordinarily the goal of the organism.

Hebb, Bard and Mountford, Cobb and others show that man *in utero* and in infancy learns how to learn; that is, learns perceptions, and emotional and physiological patterns. This involves an incredible number of neuron circuits both in and between different functional areas of the cerebral and cerebellar

Cultural periods *Dates*	*Culture "level" estimated subjectively*	*Population estimate per square kilometer*	*Age at death of adults*		*Osteoporosis Skulls 2–x years*	*Stature in cm. (Trotter formula)*		*Pelvic brim depth Both sexes*
			Male N	*Female* N		*Male* N	*Female* N	N
Modern 1920	High (commercial)	55+	56 (1929 census)		?	167+ (118)	(157) (guess)	?
Romantic 1800	Rising (rebuilding)	25+	40 (205)	37 (20)	30−% (190)	168− (38)	156 (32)	85? (6)
Turkish 1400	Drop: isolated (peasant)	10?	34 (29)	28 (24)	33 % (54)	171 (29)	158− (22)	(84) (2)
Medieval 600	Fluctuating (disturbed)	20−?	36 (55)	31 (23)	11+% (86)	(173) (5)	(154) (2)	(86) (2)
Roman 120 A.D.	Confused: rigid (dependent)	28−?	39 (62)	33 (26)	18 % (89)	168? (9)	(159) (5)	90? (8)
Hellenistic B.C. 300	Plateau (cosmopolitan)	30−?	43 (89)	37 (37)	11 % (123)	171 (17)	158 (13)	83? (7)
Classic 680	High (commercial)	35	45 (82)	35 (48)	2−% (115)	169 (21)	156 (14)	85 (13)
Early Iron Age (1250–1000)	Confused: rise (feudal)	(22+)			6 % (101)	165− (25)	154 (25)	79 (12)
(Cephallenia) Submycenaean 1150	Insular Adriatic ("Homeric")	(30)	38 (83)	31 (60)	0? (36)	166− (10)	159? (8)	(86) (2)
Mycenaean L.H. III 1450	High (proto-urban)	(28)	39 (164)	32 (101)	5 % (250)	166− (39)	154 (37)	79 (13)
(Royalty of Mycenae)	Class distinction	—			7 % (27)	171+ (14)	(159) (4)	(83) (2)
Middle Bronze 1900	Mixed: rise (Greeks enter)	(15+)	37 (105)	31 (78)	12 % (174)	166− (54)	153 (43)	79 (20)
Early Bronze & Neolithic	Peasant (pre-Greek)	(5–15)	33 (40)	28 (22)	25 % (60)	161? (7)	149? (6)	—

Data gathered in 12 weeks of museum and field work in 1957 (117 adult skulls and skeletons in at least fair condition plus 275 fragmentary skeletons, children, and infants) are here included with earlier material (940 adults plus over 500 immature and fragmentary skeletons). Age at death of adults (15−x age groups) is based on useable fragments as well as complete skeletons and includes subadults in order to be comparable with the rather scanty statistics for modern America, where life expectancy tables (based on birth and death *rates* relative to the number alive rather than on proportions of deaths at different ages) are the norm.

The lack of postcranial skeletons after Hellenistic times is obvious and unfortunate: Turkish and 19th century ossuary material has limited usefulness.

Key to skull types: A, Basic White or robust Mediterranean, combines a linear, big, and angular vault with an intermediate face having coarse nose and long jaw; B, Mediterranean, is smaller, also linear, more delicate, with compressed face and skull base and pointed mouth region; C, Alpine, is short-headed, broad throughout, with rather short and flat face and strong chin; D, Nordic-Iranian, is large and linear, with smooth and deep braincase, and long rectangular and notably beaky face; E, Mixed Alpine, paedomorphically combines a relatively small face with a well-filled long mesocrane skull vault having a strikingly

% frequency of arthritis				*% frequency of skull types among male and female adults*							*Mean change*	*Variability "Sigma Ratio" Males* 100= *norm*	*Head form l.-br. index*	*Head size Horiz. circumf. in mm.*
Vertebrae (disk injury) Male N	*Female* N	*Limbs, hands, and feet Male* N	*Female* N	A	B	D	F	E	C	N				
19 (U.S.A.) (4,654)		(10) (guess)		15	13	14−	19+	14−	25	(118)		102 (118)	80+	(518)
											4%			
(75) (4)	(50) (4)	25 (98)	16 (44)	15	18	13	25	10	19	(199)		102 (162)	78−	509
											7%			
(67) (3)	—	5 (40)	5 (22)	10	35	17	23	6	9	(52)		109 (24)	76−	510
											5%			
(50) (2)	(0) (2)	(25) (4)	(0) (2)	17	25	20	18	5	15	(75)		117 (39)	77	516
											7%			
14? (7)	(20) (5)	11? (9)	0? (6)	16	11	16	26	15	17	(82)		111 (49)	78	517
											4%			
43 (14)	33 (12)	20 (15)	20 (15)	20	11	22	16	15	16	(122)		103 (67)	77	520
											5%			
54 (24)	47 (17)	17 (30)	13 (23)	22	19	27	10	10	12	(105)		100 (55)	75+	521
											8%			
60 (15)	23 (22)	36 (25)	7 (29)	7	15	22	18	15	23	(60)		109 (26)	77−	521
(33) (3)	(0) (2)	11? (9)	(0) (5)	38	27	10	0	15	10	(40)		116 (18)	74−	525
											10%			
69 (35)	51 (35)	33 (42)	12 (41)	24	18	13	7	10	18	(158)		103 (66)	76	522
											8%			
46 (13)	(67) (3)	20 (15)	(25) (4)	14	0	38	24	19	5	(21)		130 (12+)	74	538
75 (48)	59 (41)	43 (65)	15 (47)	21	9	29	10	13	18	(110)		134 (57)	75+	520
											9%			
(0) (1)	(50) (2)	30? (10)	14? (7)	28	29	11	7	9	16	(56)		104 (17)	76	513

broad and full forehead; F, Dinaric-Mediterranean, describes the opposite intermediate combination of short mesocrane vault with pinched forehead and a big-nosed long hexagonal face. Male variability is averaged from "Sigma Ratios" of horizontal circumference, vault height (auricular-vertex), chin height, cranial length-breadth index or proportion, fronto-parietal breadth index, nasal height-breadth index, and facial profile angle in order to cover separate growth factors of gross size, height, degree of linearity, mouth massiveness and projection, and midfacial proportion without overlap through intercorrelations between descriptive measurements. The Sigma Ratio of a measurement or index is the relation of its standard deviation to W. W. Howells' "Mean Sigma" for that particular character (derived from a good number of male skull series accepted as normal). Mycenaean Royalty from the two sets of Shaft Graves belonging to the last half of the Middle Bronze Age (Middle Helladic with Late Helladic I + II) and the Ionian islanders from Submycenaean Cephallenia (culturally and morphologically separate from the mainland Iron Age groups) must be excluded from chronological comparisons: they are special cases of social and geographical variation respectively and must be compared with their contemporaries only.

The small sample of living Greek mainland males was drawn from a much wider area than the circum-Isthmian region from which almost all the skeletal material comes.

cortices. In contrast between a rat (or an early Primate) and man the penetration of the sensory–association–motor cortical areas which will result in integrated action involves cubing the number of pathways traversible for each linear doubling of the cortex. Hence the long human infancy and need for maternal and social security. When we add the thalamus and lower motor centers and all their reverberating and feedback mechanisms we begin to appreciate the complexity of emotional as well as physical learning and the immense potential flexibility of the human CNS in vital relation to the complexity of culture.

This complexity, plus the need for release from fatigue or monotony, means that in his search for pleasure the infant and young child must experiment and try out all novelties in the environment, but always with an equally driving return to mother, or to sleep, or to food. Thus an alteration early in life between complete security and the free exploration of challenges must produce the maximum use of the Central Nervous System and the maximum capacity for enjoyment. As a corollary the opposite may be true of people made too rigid by frustration, overprotection, or unpredictable hostility. In Classical and Hellenistic Greece specifically, and in many other situations, especially in contact between an advanced and a primitive culture, this alternation was not maintained in the educational system. This may have set a limit to the further use of new ideas and people and helped to stop cultural growth. Certainly we can contrast the rigidity of the Spartan proto-communist state, resting entirely on a slave base, with the greater flexibility of Athens, with its partial democracy.

Thus simple improvement in longevity and health following better living conditions and the free mixture of new ideas and people in an atmosphere of curiosity and relative freedom both tended to act in a circular relationship with the advance of material and philosophic aspects of culture. Both of these are seen to be sensitive processes, depending on the depth of the culture which they help to create. Further analysis of this must wait.

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THE PATTERN OF DEVELOPMENT OF AFRICAN CHILDREN

R. F. A. Dean

During the last six years, in which we have been working in Uganda, we have come more and more to believe that the growth and development of the African child follows a pattern that differs from the pattern recognised in communities in the United States and in Europe, and the time has come when it may be of some value to summarise briefly our reasons for that belief.

We have confined our observations almost entirely to the Ganda, the indigenous tribe of that part of Uganda surrounding Kampala, and it is important to remember that no attempt is being made to generalize for all Africa from our knowledge of that tribe: even in Uganda, there are peoples—for instance, the Nilotes of the north-western part, or the Hamites found in the track of the migration that led from the north-east to the south-west of the country, and so into the Ruanda-Urundi—whose physical characteristics are remarkably different from those of the Ganda, and whose pattern of development may also be remarkably different. The Ganda may have some admixture of Hamitic blood, but they belong to the Bantu-speaking groups, and their racial affinities are therefore chiefly with the people to the south, rather than to the north.

The techniques at our disposal include the photographic method of somatotyping as developed by Dr. J. M. Tanner (1951–52) at St. Thomas's Hospital, in London, on the lines evolved by Stuart and Shuttleworth in the U.S., but most of the results to be discussed were obtained by the classical anthropometric methods, supplemented by various radiological examinations of the skeleton. Great difficulties have arisen from the lack of an effective system of registration of births, and the need felt by older children to give their ages falsely because the upper limit for admission to secondary education has been fixed at 16 years. We have, therefore, very little accurate data about young people between the ages of 16 and 21 years, but we believe that our data for other ages is as valid as the present circumstances allow. On the whole, it improves in quality the younger the age group, and for two reasons: that our work on malnutrition has been very largely concentrated on the African child under the age of 3 years, and that the younger the child, the greater the certainty about the date of birth. Most mothers of children under 3 years of age know the date perfectly.

It may be best to present the evidence in a chronological order, starting with the young adult and ending with the child at birth.

We began our anthropological work in Uganda with a cross-sectional survey of schoolboys, and noticed at once that the boys who admitted that they were over 20 were nearly all taller than those who claimed to be only 17 or 18. It seemed that growth in stature continued up to 20 years, and perhaps even later, and this possibility is, we understand, being confirmed in the Student Health Service of Makerere College, the University College of East Africa. At the College, accurate measurements have only been taken for just over a year, but it is hoped that in the course of the next few years sufficient data will be collected to establish this point firmly: it is certainly believed by the officers in charge of groups of young men in various parts of Uganda, in training schools and the like, that the height of the men continues to increase long after it would have stopped increasing in Europeans: that is, long after 17½ to 18 years.

The uncertainty about ages becomes less important when we consider the relationship between height and weight. A curve expressing the relationship shows that at the lower end of the scale, African boys are usually lighter for their height than boys in the U.S. (the data for American boys we have used for comparison are those of the Brush Foundation Study, Simmons, 1944) and at the upper end of the scale, are usually heavier for their height. Curves drawn through the African and U.S. data cross at a point that represents, we believe, 14 years. Up to then the Africans are slim, and afterwards they become more and more stocky.

We are fairly certain about this age because we have confidence in the results of our examinations of children who are from 6 to 16 years old. The upper part of this range is of great importance, because it should show the effects of puberty, and especially the pubertal, or pre-pubertal, spurts in weight and height. The spurts are best indicated by plotting increments of weight and height at regular intervals, and in American or European communities, the spurts are sudden and spectacular. In our African children, there is a spurt of weight that begins at the age of 10½ years and is quite definite although it does not reach the altitude of the U.S. peak, but there is no corresponding spurt in height. This is a remarkable and disturbing finding, but we believe it to be a true one. For various reasons, we have had to rely on data that is not entirely longitudinal and is to that extent unsatisfactory: the number of children we have been able to follow regularly over the critical period of 12 to 16 years is less than 100, and for the rest we have children who were already 12 years old, or more, before we first saw them. But there are two details that are confirmatory. Inspection of the height curves of the individuals for whom we have complete records shows that a spurt does occur in some, but is very rare—so rare that we cannot be justified in guessing the age at which it usually appears, or even in assuming that it occurs at such different ages, in different children, that it disappears in the collective data. Secondly, the same children have supplied the data for weight and height. If, therefore, false ages were affecting the results, they must affect those for both measurements: but the weights show a spurt, whereas the heights do not.

The endocrine changes of the African at puberty would certainly repay close study. One of our first observations was that the distribution of body fat in boys, and the shape of their hands, were of types we associate with the female habitus in Europe, rather than the male. We are studying fat by the skin-fold thickness, measured with constant-load callipers, but our data is not yet complete, and has no mathematical justification. Our studies of the hands are a little further advanced, and are quantitative as well as qualitative. The second metacarpal is one of the most useful indicators. In girls in the U.S. and in Europe, it is a slim bone, with a finely-shaped base that has a well-defined prolongation on the side away from the thumb: in boys it tends to be short and thick, and the base is square and sturdy. In the African boys, the shape resembles that we see in European girls, not boys, and there is often an obvious "feminine" prolongation. Furthermore, the calculated ratio of width to length is "feminine" rather than masculine. The slimness of the African boys' hands as a whole can be related to ratios of the lengths of the metacarpals to the bones of the fingers: the ratios would be thought "feminine" in Europe.

We have been able to examine only about half as many African girls as boys, and our data on some of the sexual differences is scanty, but it is sufficient for us to be sure that in the Africans, the bones of the hand, at least up to the age of about 16 years, do not show all the indicators of sex that are normally found in the U.S. and in Europe, although there may, we believe, be a more definite distinction when the final form of the bones is attained. To discover the age when growth and shaping of the bones ceases would clearly be of special interest, especially in relation to the final stature.

The lack of easy and obvious differentiation is also to be found, according to our anatomical and obstetrical colleagues, in the pelvis. Although we are familiar with a series of details that should mark the male pelvis of the European distinctively from that of the female, it is a matter of the greatest difficulty to decide the sex by simple inspection of the pelvic skeleton in our African population (Washburn, 1949).

The same is true of the skull, which is usually the portion of the skeleton most readily differentiated sexually. In the anthropological collection at Makerere College a large number of known male skulls show such typically feminine features as deficient supra-orbital ridges, small mastoids, finely moulded facial skeleton and poor muscular markings (Allbrook).

The facilities and energies of our Group are largely devoted to trying to define the effects of malnutrition in young children. As a corollary of that work we are collecting as many data as possible on the growth and development of young children who, so far as we can tell, have never been malnourished. The radiological examinations have shown, contrary to findings elsewhere, that in the hands of healthy children up to 3 years old there is precocity of skeletal development in comparison with the U.S. standards in the Atlas of Greulich and Pyle (1950), and that the precocity is greatest in the youngest children. We have, by the way, arrived at this conclusion by the study of individual bones, and not merely by an overall inspection of the whole hand: we find, in fact, that the excellent Atlas cannot be used satisfactorily for an African population except on a meticulous basis, because the deviations from the standard are not uniform, but are greater in some bones than in others.

The same tendency to precocity, with the same tendency for the precocity to be greatest in the youngest children, is shown in the results of a very different measurement—that of psycho-motor development by the technique of Arnold Gesell. For the last three years we have had a visiting worker, Dr. Marcelle Geber, who is practiced in the Gesell test as applied by the Parisian group of psychologists led by Dr. Roudinesco-Aubry (cf. Roudinesco and Guiton, 1950). An account of her findings was published in the *Courrier* of the International Children's Centre, of Paris, in January of this year (Geber, 1956), but no account in English has yet appeared, and as the work may not be well known in the U.S. a summary may be of interest. There seemed to be no doubt that the "milestones" of development recognised in Western communities—lifting of the head, sitting and standing—were passed very early by the African children, and the Gesell test confirmed that in the first six months the children were especially remarkable for their ability to control the movements of the head and trunk. Some children could support the greater part of their weight, when held standing, at 6 to 8 weeks, and could stand momentarily alone at 16 weeks:

many began to walk without support at 9 months, and by 10 months were walking well. At two years, they could usually walk down a staircase using one foot on each step, and jump from the last step. Manual dexterity was also very precocious. At a very early age, the hand was put forward rapidly and directly towards the object to be grasped, with the minimum of those wide sweeping movements that are usual in our children, and from about 10 months, the index finger and thumb were used delicately and very precisely for such refined purposes as the picking up of a small pastille. The intellectual development was almost as remarkable, particularly in children under a year of age. Of 40 children, only 3 had a quotient for "adaptivity" under 100, 100 being the "normal" score for an American or European child on the Gesell schedule. "Adaptivity," as measured by the test, indicates the ability of the child to make use of the test material in an imaginative way, and therefore represents a combination of qualities, sensory, motor and intellectual. The "personal-social relations" of these young children were also extremely good—only 4 of the 40 had a quotient under 100—and "language," including all the means of comprehension and expression available to the child, was almost as advanced.

There were altogether 131 children in the series. In the 40 children already mentioned who were under 1 year old, there were 37 (93%) whose overall development quotients (the average of scores for the various groups of tests) were over 100, and in 35 children 1 to 2 years old there were 28 (80%) in whom it was over 100. After two years, however, the precocity was usually lost, and it seemed likely that most children, as they approached the age when they should begin their primary school education, would have a level of development well below that of European children.

This year, we have pursued these findings of precocity to their logical beginning, by studying the African child at birth, using the methods worked out mostly in France, and mostly by André Thomas (1952), Saint Anne-Dargassies (1954), and Koupernik (1954). At the time of writing, Dr. Geber has tested only about 100 newborn children, and her results have not been analysed. She has, however, made a tentative list of findings of importance, and we are including it, despite its provisional quality, because it seems to fit well with, and even in some ways to confirm, some of our other findings.

In 33 children examined *on the day of birth*, the sudden, almost convulsive, extension of the arms known as the Moro reflex was found in only 15. In European children it is found almost invariably. The grasp reflex (another constant finding in European children) was present in only 14 of the 33, and all except one of those were aged 8 hours or less.

On *the day after birth* (and even in one child 14 hours old), the attitude of close flexion of the limbs tended to be replaced by some degree of extension: this was especially noticeable in the legs, but several children held an arm extended and placed the hand on the crown of the head. The hands were not usually held shut, but were more often open. Many of the children, when placed on their backs on a firm table, held their heads in the median position. The head appeared to be much better under control than in European children of the same age. Half of the children, when placed on their fronts, raised the head so that the chin was clear of the surface of the table, and some could turn the head to the side from that position, or turn it from one side to the other. The automatic walking movement, that is usually established in European children in the first week, was much less constantly found in the Africans, and it was also noticed that the feet were usually held rotated internally and not externally. The African children gave the general impression that their movements were, on the whole, much better organised.

These results were not altogether unexpected, because we had also, with the help of members of the staff of the Institute for Personnel Research, in Johannesburg, begun a study of the electrical activity of the brain of the newborn child. The number of records so far obtained is very small—only 12 being considered sufficiently complete for detailed analysis, but so far as we can tell at the moment, there is a tendency towards the establishment of rapid activity, and of continuous rhythmic activity, that is very unusual in the electroencephalographic records of newborn European children. The results must be regarded as provisional, and we would not have mentioned them at this stage except for the obvious reason that they are in line with the other findings, of advanced reflex and motor activity, that have just been described.

DISCUSSION

We regard the work so far done as a small series of preliminary explorations, but we think the series is sufficiently interesting to warrant a great deal of further investigation, and we would welcome any help that might be offered for more intensive studies. There is no doubt that Kampala is an excellent centre, and no doubt that Uganda provides almost infinite material for anthropological work. We have already collected almost more data than we can handle in the next few years. The results of the analysis of our somatotype material should be extremely interesting: 250 school children are being photographed every 6 months, and in a comparatively short time it should be possible to have a fair idea of the changes in the bodily proportions of the Ganda school child during the period from 6 to 16 years of age—a period of obvious importance to the physical anthropologist. Many of these children, and of the preschool children we have also measured, seem to travel below and parallel to American or British curves of growth. Although they appear to be well-nourished, they show no inclination to reach the height or weight indicated by the curves. Have such children had, in their younger days, some debilitating or retarding experience from which they have never recovered, or are they developing "normally" for their race? We cannot yet say. We know that in the children we have been able to follow after discharge from our own hospital wards, in which they have been treated for the very serious deficiency disease we call *kwashiorkor*—a disease we think is due largely to existence on a diet poor in protein—there is hardly ever any acceleration in growth in the convalescent period to make up for the retardation caused by the disease (Jones and Dean, 1956). We know that it is only the extreme forms of the disease that are sufficiently spectacular, or sufficiently recognisable, to bring the child to hospital, and that many children pass through a period of ill-defined subnormal health, probably related to poor nutrition, in which a partial failure of growth may be an important feature. We do not, however, really understand the effects of good or poor nutrition on growth, and to what extent the differences from the European pattern that we have observed can be attributed to nutrition. It seems fairly certain that all the differences cannot be accounted for by the diet and that no amount of excellent feeding could completely eliminate them. The African child is different at birth from the child of European descent, and develops differently afterwards: we suspect also, from a few observations we have made on premature infants, that even his intra-uterine pattern of development is markedly different. His progress may be modified by his diet, but is probably determined fundamentally by his heredity. We think it may be more profitable to study the child than the adult: it may well be that some of the differences we have found are lost when development is complete. We are accustomed to believe that final stature, and final bodily form, are very largely under genetic control: it does not seem too violently unreasonable to suggest that the route followed to the final stature and form is also under genetic control. Besides the details we have mentioned, there are many others that can hardly be determined by peculiarities in the African way of life: for instance, a failure of the superior part of the helix to fuse all along the usual route—a failure that leaves a small hole in about 10% of the children we have seen under 3 years of age: the precocity of first and second dentitions, and the frequency of supernumary teeth; the frequency of supernumary digits, of which Dr. Geber saw examples in 5 of the 100 newborn children that she has examined; and the tendency of older children, when asked to draw a man, to begin with the ears, or to make the ears abnormally large—presumably because the sense of hearing is especially important.

It has become obvious that we have to expect, whenever we mention the precocity of the African child, that someone will at once be reminded of those primates—usually chimpanzees—which have been closely observed in their early life, and found to exhibit an equally remarkable precocity. It is, we think, neither a polite nor an exact parallel. The informed use of the hand—the "adaptivity" measured in the Gesell test—is of course the most immediate distinction, and the capacity for continuous learning the final one. We do not complain about the fantastic precocity of Mozart: we only marvel that the precocity could be maintained and continually developed into exceptional genius. We would be wise not to scoff at the precocity of the African, and would do better to consider how his precocity could be maintained: it is hard to think of any more important contribution that could be made towards his advancement. We have worked for the last three years in a nursery school for the children of men at a local theological college preparing for ordination, and we have found that many of the children retain their early precocity, at least in part, long after their less fortunate contemporaries have lost it. We cannot at the moment decide which of many features—more intelligent parents, better living conditions, better nutrition, better opportunities for play and for a communal life, to name only a few—are helping to set these children apart. The child in the ordinary African home, although he receives in the first year an upbringing that could almost have been designed to ensure his security, usually has to adapt himself to a violent change in his mother's attitude at some time in his second year, and we do not know to what extent his development is effected: we suspect that the results can be very severe. At best, he certainly lacks the intellectual opportunities we provide all the time for our own children from their first days. The effects of more sympathetic handling at this delicate age, and of adding those opportunities sympathetically to the African environment should, we think, be investigated with the greatest care, and should produce results that could hardly fail to be of the greatest interest.

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A STUDY OF THE ROTATION OF THE OCCIPITAL REGION IN THE NEANDERTAL AND SAPIENS SKULLS

Miguel Fusté

We attempt here to apply to the study of fossil human skulls the method for the orientation of the skull by means of the horizontal vestibular plane. This plane, first used by Girard (1923), contains both *canalis semicircularis lateralis* and is after him the most suitable one for the study of the comparative morphology of the skull. By means of it Delattre (1951) was able to measure the intensity of the backward and downward rotation of the occipital region of the skull around the vestibular axis (a line joining the middle points of the *canalis semicircularis lateralis*) involved in the attainment of the erect gait.

As the method requires either the dissection of the *canalis semicircularis* by exocranial procedure, or the obtainment of teleradiographs in skulls sectioned along the sagittal plane, it cannot be applied to fossil skulls.

After some proofs, and starting from previous assertions of some authors (Pérez, 1922; Girard, 1923; Delattre, 1951), evidence was obtained about the possibility of substitution of the vestibular plane for that passing through the nasion and the two porions (Fusté, 1956).

Using this last plane, an angle similar to that employed by Delattre for the evaluation of the above-mentioned movement of rotation was measured in all the Neandertalian skulls from which we could get suitable craniograms of the *norma lateralis*. This angle is that formed by the porion-inion-porion plane with the perpendicular one to the porion-nasion-porion plane, passing through the two porions. The following values were obtained:

La Quina H^5	57°	Saccopastore I	71°
Spy I	57.5°	Gibraltar	72°
Le Moustier	62°	Steinheim	73°
La Chapelle-aux-Saints	63.5°		
Monte Circeo	65°	Solo X	63.5°
La Ferrassie	68.5°	Solo V	66°
Skhūl V	68.5°	Solo I	74.5°?
Rhodesia	69°	Solo VI	74.5°

For comparative purposes, the same angle was measured in a series of 80 recent human skulls, as well as of the fossil men of Afalou with the following results:

	n	$V_1 - V_n$	$M \pm m$	$\sigma \pm M_\sigma$	$v \pm m_v$
H. sapiens recens	80	69° – 86°	76.85° ± 0.44	3.95 ± 0.31	5.14 ± 0.41
Afalou	30	69.5° – 86°	78.37° ± 0.72	3.94 ± 0.51	5.03 ± 0.65

According to the list above, "classic" Neandertalians (i.e., La Quina H[5], Spy I, Le Moustier, La Chapelle-aux-Saints, Monte Circeo, La Ferrassie) show the smallest rotations of the rear region of the skull, whereas the more intensive ones are those of the pre-Neandertalians (Steinheim and Saccopastore I). Neandertalians from Palestine (Skhūl V and very probably also Kafzeh) range themselves near the last ones. Gibraltar woman falls between the two pre-Neandertalians, and Rhodesia man coincides with the lower limit of the variability of the *sapiens* series. All that is in agreement with some remarks of Sergi (1934) concerning the Gibraltar specimen and with those of Morant (1928) and von Bonin (1928) about the Broken Hill remains. Solo men seem to range themselves in an intermediate position between the Neandertalian group and *H. sapiens*.

It must be pointed out that the seriation above agrees fairly well with that of the angles measuring the flexion of the cranial basis. This is an important conclusion because, as is well known, the angulation and correlative shortening of the basis of the skull are of the greatest importance for the determination of its morphology (Weidenreich, 1924; Howell, 1951). Owing to the relation on one side between the rotation of the occipital region and the degree of flexion of the cranial basis, and on the other side that of the last character with almost all the differential traits between Neandertal and *sapiens* ("chignon", degree of development and position of the external occipital protuberance, shape and orientation of the auditory foramen and of the tympanum bone, different angulation of the frontal and occipital regions) it can be concluded that the weakest rotation of the occipital region of the Neandertalian skull seems to afford a key for the explanation of its total morphological pattern.

A considerable difference results in the comparison between the Neandertalian group and the *sapiens* series. This difference, as well as the overlapping of the pre-Neandertalians with the latter, agrees with the systematic, chronological and phylogenetic position of these groups. Pre-Würmian Neanderthalians, older and for this reason closer to the stem from which the Würmian Neandertalians and *Homo sapiens* branched off, show a stronger rotation than "classic" Neandertalians. The lesser rotations (and smaller angulations) of the latter are not therefore to be considered as the persistence of a primitive condition, but secondarily acquired traits, probably related to the strong specialization of the group.

As concerns the mechanism determining this specialization, the genetic variations related to the isolation undergone by West-European Neandertalians during the Würm I period should have been, after Howell (1951) the determining factors, whilst Blanc (1955) explains the differentiation of the Neandertalian group by means of his theory of cosmolisis, according also to their geographical localization. Meanwhile, outside that region, and probably starting from less specialized forms closely related to the pre-Neandertalians, the evolutionary processes leading to *H. sapiens* were going on. Among them, the progressive rotation of the cranial elements around the vestibular axis must be considered as of the highest importance.

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EL POBLAMIENTO PALEOLITICO DE ESPAÑA

Luis Pericot Garcia

Diversos autores han intentado en los últimos años interpretar los escasos datos antropológicos y arqueológicos que se refieren a los tiempos más primitivos de España para reconstruir el proceso de sus primitivas raices. Ello no es sino seguir las huellas de lo que el profesor. Bosch Gimpera realizó con tanto éxito para su tiempo. Así Martinez Santa Olalla en su "Esquema Paletnológicó", así M. Almagro en su importante trabajo que está a punto de aparecer y así nosotros mismos en "Las raices de España".

En la presente communicación nos proponemos insistir sobre algunos aspectos que en los últimos años han ido cobrando interés, concretamente para los tiempos paleolíticos. Cierto es que nada se ha descubierto todavía como novedad para las etapas premusterienses, pero el hallazgo del *Atlanthropus* no puede por menos de despertar esperanzas de hallazgos semejantes en suelo hispano, en especial en el Manzanares, cuya industria no deja de ofrecer paralelos con la del yacimiento de Palikao.

Respecto del hombre de Neandertal, el reconocimiento por Fusté del parietal de *Cova Negra* y el más reciente hallazgo por Spahni en la cueva de Piñar (Granada) de fragmentos de craneo de Neandertal indican cuanto cabe esperar todavía de las futuras excavaciones en el inexplorado suelo español. Como ha señalado Alcobé, tenemos ahora en España representantes de las dos ramas que se han señalado dentro de la raza de Neandertal. Como arqueólogos el problema que nos preocupa es el de la evidente perduración de las técnicas levalloiso-musterienses durante buena parte por lo menos del auriñaciense. Es imposible dejar de pensar que sólo una cierta continuidad de población puede explicar esa continuidad en las tecnicas del utillaje.

Los datos antropológicos no nos aclaran la cuestión pues son en número excesivamente reducido todavía. Pero siempre que aparecen, tales restos ofrecen un caracter cromañoide. Sin embargo, los hombres del *Barranc Blanc* eran, por lo menos en parte, de la variedad africana o mediterránea de Cro-Magnon, del tipo de *Mechta el Arbi*, perpetuado entre los guanches canarios. He aquí un punto de extraordinaria importancia. Digamos ahora que tanto las excavaciones del *Barranc Blanc* como las de la *cova Negra* de Játiva han sido posibles en los últimos cuatro años gracias a la generosidad de la Wenner Gren Foundation de Nueva York, que ha mostrado así su alto espíritu de cooperación intelectual.

Dada la perduración que hemos indicado de las técnicas de lascas, nos preguntamos con frecuencia cuando se inicia realmente el Paleolítico superior, es decir, cuando se impone la industria de hojas. Ello debe hallarse en relación con la presencia de la industria gravetiense, que parece deber sustentarse en algún grupo humano definido. Mantenemos el nombre de gravetiense de preferencia al de perigordiense que los colegas franceses y algunos españoles prefieren. Creemos que mientras perigordiense se limita a una variante cultural de una zona francesa, gravetiense significa una técnica de hojas y puntas con dorso rebajado que domina en zonas muy amplias de la tierra y puede abarcar complejos culturales diversos.

Pues bien, estudiando los materiales de las cuevas del Paleolítico superior español, he llegado al convencimiento de que la técnica de hojas no predomina hasta un momento avanzado, sustituyendo a las industrias básicamente de lascas. El solutrense, al igual que el ateriense, dependería al principio tambien de una industria de lascas.

Hoy resulta evidente que toda la base cultural de la Península durante la fase avanzada del Paleolítico superior, esto es, durante la última fase de avance glaciar por lo menos, nos lo proporcionan gentes que usaron la técnica gravetiense. Sobre esta base se insertan las culturas solutrense y magdaleniense y esta base subsiste hasta el Mesolítico y da caracter a una parte del territorio español incluso durante el Neolítico.

Por otra parte, esta base gravetiense es común a todo el Occidente del Mediterráneo y también al norte de Africa. Para las fases posteriores al desarrollo del Gravetiense clásico, empleamos el término de Epigravetiense, siguiendo el ejemplo de H. Obermaier.

No nos interesan ahora las conexiones postpaleolíticas con lo africano, que los hallazgos de arte rupestre en Sicilia han iluminado con nuevos puntos de vista. Todos las aceptan y su discusión no es tan urgente.

Lo que nos preocupa es cómo se realiza la inserción de lo solutrense y lo magdaleniense en esa base que hemos señalado y si en ambas culturas tenemos acaso los dos puntos opuestos de nuestro poblamiento: Africa frente a Europa.

El caso del solutrense nos aparece dada día más como un problema universal, pues los últimos eslabones de las técnicas solutrenses hay que buscarlos en esas puntas de los llanos americanos que fueron el arma de los primeros cazadores que poblaron el Nuevo Mundo.

Hoy, tras la revisión de las fases protosolutrenses de Europa central y los descubrimientos de los colegas rusos en Kostienki y en otros lugares, que esperamos conocer mejor, no nos atreveríamos a postular sin más, como hicimos hace unos años, el origen africano del solutrense. Las cosas pueden haberse producido de manera mucho más compleja de lo que suponíamos y en todo caso el mundo europeo occidental cuenta también en el problema. Pero insistimos, aún sabiendo que grandes maestros nuestros y queridos colegas disienten de nuestro punto de vista, en que a través del estrecho de Gibraltar hubo un contacto entre solutrenses españoles y aterienses norteafricanos. Y que la punta de aletas y pedúnculo (que hoy gracias a H. Kelley sabemos que también se encontró en Francia) algo significa en ese contacto. Por otra parte el fragmento de craneo que encontré en la cueva del *Barranc Blanc* y que Alcobé estudió y clasificó como del tipo de *Mechta el Arbi*, confirma plenamente nuestras hipótesis.

Para los magdalenienses el problema es más claro, pues su caracter nórdico, su penetración esporádica por el levante y acaso el Mediodia de España, no ofrece duda.

Lo que nos preocupa es: ¿Podemos todavía mantener la hipótesis de la unidad étnica de todas las gentes que durante el Paleolítico superior poblaron España? ¿Todas pertenecían a la rama cromañoide, aunque hubiera entre ellos algunas variantes? ¿Cabe una unidad étnica con tales contrastes culturales y diversidad de procedencias? Y aun hemos dejado de lado un tema que nos es grato, el de los africanismos pseudo-capsienses, si se admite la expresión, que desde el solutrense señalamos en el Parpalló por ejemplo.

Estaríamos tentado a considerar pués que hubo durante el Paleolítico superior español pueblos del todo distintos, dejando a los antropólogos el cuidado de descubrir sus caracteres, si no nos detuvieran dos consideraciones. Una la de la continuidad en la técnica del silex una vez se ha aprendido la talla de hojas. Otra, más fundamenta, la continuidad en las formas artísticas. Cierto que a lo largo de la ocupación del Parpalló varían los gustos, desaparecen o aparecen ciertos motivos, desaparece casi la pintura. Pero no cabe duda de que entre sus productos más extremos hay un espíritu común, que no permite pensar en tipos humanos radialmente distintos. La solución estaría pues en aceptar una cierta diversidad étnica dentro de un tipo humano común. Esta conclusión no es nueva, pero volvemos a ella tras muchos titubeos y cuando estamos en posesión de muchos más datos. Y en esto radica su interés.

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SMALL ISOLATED HUMAN BREEDING POPULATIONS AND THEIR SIGNIFICANCE FOR THE PROCESS OF RACIAL DIFFERENTIATION

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Isolation is one necessary condition for the occurrence of racial diversity in man. The population enclaves which still remain isolated are subject to encroachment, however, and the conditions that permitted the origin of present racial differences are not likely to recur. The breakdown of isolation has increased not only in extent but also in speed. Throughout the world there is manifest increasing racial amalgamation. Thus, although major racial stocks could hardly have arisen from remnant groups, we must largely depend on a selected few contemporary tribes and communities for analogies to past conditions.

For the present purpose we may recognize racial differences as variations in the frequency with which any hereditary characteristics occur in different human groups. To study the process, we need not be concerned with the insuperable difficulties of a static classification of all men into a fixed number of races; any division will suit our needs if the genetic basis of the conditions is known and the group is distinguished by a generally endogamous selection of mates, at least in ancestral generations. Raciation, or race formation, then, can be equated with a change in gene frequency within a breeding population.

This definition would fit the word "evolution" equally well, but it seems probable that raciation, evolution at a subspecific level, depends on a different equilibrium of causative factors from evolution of a species or higher category. The events which influence evolution can be classed as systematic and accidental. Only systematic factors are subject to scientific analysis, although accidental events may be important in the history of some organisms. The systematic factors which may influence gene frequencies have been classed under four rubrics: 1, pressure of repeated mutation (not a single saltatory mutation, which must be classed as historical accident); 2, adaptive natural selection; 3, a tendency of random selection to produce systematic results; and 4, within the species, assortative mating, the systematic effects of isolation and its opposite, intermixture (see, for instance, Wright, 1932).

Since the appearance of Darwin's *Origin of Species* nearly 100 years ago, evolutionists have placed primary importance on natural selection. Indeed, for the human species, as for others, it is clear that man's distinctive traits, above all bipedal locomotion, manipulative capacity of the hands, and mental capacity for speech and learning, were established through natural selection. But we cannot use these traits to measure race differences. So far no attempt to identify different degrees of these and other capacities in various races of man has been free from serious criticism.

In recent years, however, the school of thought which would explain racial differences with "survival of the fittest" believe themselves supported by a new line of evidence. It has been shown that the ABO blood groups are not entirely neutral but may influence the sex-ratio of offspring (Glass and Cohen, 1956) and tendencies to certain diseases (Buckwalter, *et al.*, 1956). Furthermore, the distribution of the sickle-cell trait and sickle-cell anemia in Africa (Mednick and Orans, 1956) and the thalassemia trait and anemia in Sardinia (Ceppellini, 1955) has been explained on the basis of Allison's (1954) finding that heterozygotes for the former, at least, seem to show an immunity to malaria.[1]

This type of evidence has led some anthropologists again to attempt to find in existing racial differences the evidences of past climatic and other adaptations. Newman (1953), for instance, has shown that in continental areas where the population interbreeds and is not subdivided by natural or social barriers, the average stature is less in the peoples of the more tropical parts; thus, the southern Chinese are shorter than those of the north. He sees in this a correspondence with the general zoological principle that the ratio of surface area to volume (hence of heat loss to heat production) is less in cold regions. Coon, Garn and Birdsell (1950) have tried to apply similar adaptive rules to a variety of racial characteristics. Thus, for instance, they explain the compact body build of the Eskimo as a heat-conserving device of nature.

But the adaptive conditions are much more complex than these *post hoc* arguments imply. For instance the Eskimo, although compact, are not large; and it is a general human condition, not specifically racial, that taller people tend to be less compact in shape and have relatively longer arms and legs. Furthermore, the volume of a man is not homogeneous in its capacity to produce heat, and his surface area is not homogeneous in capacity to give off heat. Complex physiological mechanisms, the racial differences of which are unknown, would be involved in any climatic selection. Furthermore, one would have to take into account energy input as well as heat output: one would have to know what kind of man could get food best. I have previously shown, for instance, that southern Chinese grow more like northern Chinese when they live in more similar environments (Lasker, 1941).

It seems to me that the mechanism of human raciation remains an open question. In this connection I think it worthwhile to explore further the role especially of isolation and migration as systematic influences.

Mutation pressure and natural selection would operate in comparable ways on populations of various size; but another systematic factor, the random variation in gene frequencies from generation to generation, would be greatly influenced by the size of the population and the variability in family size. Birdsell (1951), Kluckhohn and Griffith (1951) and others have pointed out that contemporary human tribes and communities are small enough so that genetic variability could decrease rapidly. In a population mating at random, heterozygosis in respect to a pair of alleles would decrease at the rate of $1/(2N)$ per generation (see Kimura, 1955). In this formulation N is effective size of the breeding population, i.e., the number of parents reduced by a factor which takes account of variability in family size.

In the past I have tried to estimate the variability in number of surviving offspring for one human population and have applied that estimate to others (Lasker, 1952 and 1954). In an unpublished paper James Crow has shown that the data I used for estimating variability were drawn from an expanding population and included young individuals, whereas I applied my estimates as if the population had been stationary. Crow and Morton (1955), using a more satisfactory set of assumptions, have shown that in several human populations the effective population size is slightly less than the total number of adult parents. On this basis effective population size is roughly one third of the total population in a variety of human groups, and for the present purposes I have arbitrarily adopted that figure. A simple estimate of the rate of decrease in heterozygosis as a result of random genetic drift does not take into account the contrary effect of any admixture. Roberts (1956), however, has applied Sewall Wright's criteria to the estimation of this interaction of random genetic drift and admixture: when the product of the effective population size by the admixture rate is less than 5, changes in gene frequency due to random genetic drift are likely to be marked, and when the product is between 5 and 50 the changes will still be appreciable. Such a rule of thumb depends on the fact that the product of effective population size by the admixture or immigration rate is a measure of the degree of reproductive isolation. I shall refer to it as the "index of isolation." In fact, Glass *et al.* (1952) have demonstrated that a contemporary community isolated by distinct religious beliefs—the Pennsylvania Dunkers—shows gene frequencies significantly different from both the general population of the region and from the German state whence their ancestors have come. This random genetic drift occurred within a few generations in a group now consisting of some 300 to 350 people—and in the face of an admixture rate of some 10 to 22%. Their isolation index is thus between 10 and 25.

Such conditions are not exceptional even today; anthropologists hardly consider groups with so much admixture as isolated and endogamous (Table 1). For instance, Oberg (1953) describes the Camayurá of Brazil, a group of 110 with 13 to 15 immigrants, as a tribe which intermarries with friendly tribes and incorporates captive men and women of enemy tribes by marriage. The index of isolation is 5. Of the dozen examples which I have previously listed (Lasker, 1954), in seven the index of isolation is below 50. These groups (Australian aborigines, the Ramah Navaho, the Havasupai, the Camayurá, the Amerindian community of Panajachel, Guatemala, Tzintzuntzan and the ranchos of Quiroga, Mexico) reasonably may be presumed to have been at least as small and isolated as now for many generations past. The five other Mexican towns included in the original study now have isolation indices above 50, but such towns also may, not very long ago, have met this rather arbitrary definition of "isolate."

I can now add eight new instances of isolated breeding populations from other parts of the world.

Some of the smallest and most isolated communities are those of islands. Thus of the populations for which I have data, that of the island of Buzios has the lowest index of isolation, less than 3. In this case, however, the population although now only 126 was previously 358 in 1902 (Willems, 1952). Ulithi, like many other islands of the Pacific, was also formerly more populous (Lessa, 1955). On the other hand, the 150 year old colony on Tristan da Cunha was founded by 7 white men and their Mullatto wives (Kuczynski, 1953). In 1912 all the inhabitants but one were descended from these founders; in 1923 there were 127 inhabitants of whom 10 were born elsewhere; the present population is still small: 274 with 32 born elsewhere.

The populations of other islands and archipelagos also apparently present isolated populations. Kuczynski (1953) lists five British dependencies with populations of 2,000 to 10,000 and with immigration rates of 1 to 10% (Falk-

TABLE 1

INDEX OF ISOLATION

(Selected communities and tribes)

Place	Total Population	Immigration or admixture rate, per cent	Index of isolation. Immigration rate times effective breeding population (taken as 1/3 total population)
Buzios Island, Brazil Willems (1952)	126	7	2.9
Ranchos of Quiroga, Mexico Brand (1951)	133	10	4.4
Camayurá, Brazil Oberg (1953)	110	12–14	4.4–5.1
Fox Indian Reservation, U.S.A. Jones (1939)	342	7	8.0
Coniagui villages, Africa Lestrange (1950)	162	16	8.6
Havasupai, U.S.A. Spier (1928)	177	8–16	4.7–9.4
Bassari villages, Africa Lestrange (1950)	134	21	9.4
Tristan da Cunha Island, South Atlantic Kuczynski (1953)	274	12	11.0
Hopewell, N.C., parish, U.S.A. Dudley and Allan (1942)	250–330	12	10.0–12.0
Ramah Navaho, U.S.A. Spuhler and Kluckhohn (1953)	614–634	6	12.3–12.7
Dunkers, Pennsylvania, U.S.A. Glass *et al.* (1952)	298–350	10–22	9.9–25.7
Cruz das Almas, Brazil Pierson (1951)	331	25	27.6
Ulithi Atoll, Pacific Lessa (1955)	421	15–20	21.0–28.1
Panajachel, Guatemala Tax (1953)	688–780	5–16	11.5–41.6
Tzintzuntzan, Mexico Foster (1948)	1231	12	49.2
Australian aborigines Birdsell (1951)	100–1500	4–10	1.3–50.0

lands, Santa Helena, Turks and Caicos, Cayman and Virgin Islands). In general, the size of the land area of islands itself sets rigid upper limits to the size of their population and restricts the choice of mates.

Furthermore, on large islands the population may be divided into several more or less isolated breeding isolates. Tindale (1953) presents such a case for

Australian aborigines. John Buettner-Janusch, in a personal communication, refers to a parish in Iceland (Hofs i Vopnifiordi) where in 1850 the admixture rate was only 22.1%, and for the deanery of five small parishes, it was only 3.1%. I have previously noted that similar conditions may hold for continental areas, for instance among certain American Indian tribes. Such data may reflect changed tendencies in historic times: decreases in population size and increases in admixture. In African villages, Lestrange (1951) reports increases in admixture of from 14% to 21% and from 11% to 16% respectively for Coniagui and Bassari villages during the last generation. Conditions in some Mesoamerican villages are relatively stable.

In South America, also, isolation tends to continue to this day: among the Cubeo, a tropical forest manioc-growing tribe of some 2,000, 75% of 40 marriages are with other Cubeo (Goldman, 1948). Tschopik (1946) reports for Aymara that in the town of Chucuito, Perú, 89% of 183 recorded marriages are endogamous within the moiety; that in the nearby ayllu of Qota 85% of 26 marriages are endogamous; and that in the ayllu of Oxerana which pertains to the town of Ichu, Perú, 90% of 20 marriages are endogamous. Pierson (1951) found only 25% of the mestizo inhabitants of the town of Cruz das Almas, Brazil, to have been born elsewhere.

Before we can declare random genetic drift in an isolated subpopulation to be a significant factor in the formation of a major subdivision of mankind, we must know not only that the group has been small and self-contained over a number of generations: it is also necessary that such a group has been capable of a relatively large expansion in numbers without increased admixture. Such expansion, especially during a period of less dense occupation of the world, is, of course, plausible. The Hutterites, a religious group with colonies in South Dakota, Montana, Manitoba and Alberta, has increased nineteen fold—from 443 to 8,542 persons—between 1880 and 1950 without appreciable admixture (Eaton and Mayer, 1953). Hulse (1955) notes that, although in 1600 there were perhaps only three million persons of British stock, there are now at least 150 million—an increase of fifty fold during a period when the world population increased six fold. Hulse calls attention to the dependence of this and similar population "explosions" on industrialization. We are less able to identify such causes that may have operated in prehistoric population increases, but the early history of expansions into virgin territories, for example, may have been even more dramatic.

If we look for the racially most distinctive groups today, we find extremes in the frequency of certain genetic or presumably genetic traits among Caucasoids, Negroes, and Mongoloids, but also among Australian and Tasmanian aborigines, African Bushmen and Hottentots, Eskimos, Lapps, Basques, Blackfoot Indians, Ainus and Negritos. There is some reason to believe that those which occupy small islands had their origin on larger land masses—the Tasmanians in Australia and beyond, the Ainu on the main island of Japan and perhaps in China, the Andaman Island Negritos perhaps in Malaysia or elsewhere on the mainland.

Race formation seems to be a continental process. Although there is no reason to doubt that it has operated on man, natural selection has not been satisfactorily demonstrated as a significant factor in racial differentiation. It is more plausible that small groups would come to differ racially by the purely random process of primarily endogamous mate selection. Subsequent rapid increases in population size based on cultural advantages or historical opportunities could be responsible in the main for the kind of racial pattern manifest today.

The conditions required by such a theory are 1, small size of the breeding groups; 2, relatively low admixture rates; and 3, occasional marked natural increases in population size. These conditions are all at work in some groups even today. Degrees of isolation adequate to fix racial types have been recorded in hunting and in farming communities, on islands and in continental areas with sparse resources and geographical barriers—in short, in a wide variety of places and climates. These conditions may be presumed to have been more pronounced in past times. Explosive population increases are rarer today but still occasionally result from technological and other cultural factors. There is therefore strong reason to consider random genetic drift with subsequent expansion an important factor in the origin of human races. Human racial differentiation does not necessarily depend on the natural selection of adaptive characters; the low isolation index of some continental as well as island peoples suggests the probable importance of an alternative mechanism.

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Note

1. Note that the communities with greater frequency of these conditions are, in both cases, low-lying malarious areas, and that another explanation might be applied to the lower frequency in mountainous regions, i.e., the well-known adverse effect of rarified oxygen on all forms of anemia. It is known that persons with sickle-cell trait may show in vivo sickling at elevations of 15,000 feet, and it is possible that longevity or fecundity of these and thalassemia heterozygotes may be reduced by permanent residence in hills and mountains at much lower elevations.

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LA PALÉODÉMOGRAPHIE, BASE NOUVELLE DE L'ANALYSE ANTHROPOLOGIQUE

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La nouvelle tendance des recherches paléodémographique est en rapport étroit avec l'anthropologie historique. C'est Pearson[1] qui a été le premier—au début de ce siècle—à tenter de calculer des indices démographiques, se basant sur une série de momies égyptiennes. Depuis, un grand nombre d'anthropologistes et de démographes se sont occupés à étudier les problèmes de la mortalité des temps anciens.[2] Ces recherches, fondées sur des séries anthropologiques, plutôt que sur des documents écrits, ont fourni un grand nombre de données de haut intérêt, bienque elles ne se sont bornées qu'à la définition de quelques caractéristiques (p.e. à la répartition des morts suivant les groupes d'âges à l'espérance de vie, etc.). A notre avis, l'utilisation et l'emploi de cette partie de la paléodémographie ne doit pas s'en arrêter sur ce point là puisqu'elle ne se prête pas seulement à une reconstruction de données historiques concernant les conditions de mortalité, mais nous permet de reconstruire la composition et la situation démographique générale des populations d'autrefois, ce qui ouvre de nouvelles perspectives à l'analyse anthropologique.

Les examens paléodémographiques répondant aux exigences de l'analyse anthropologique ne peuvent être effectués que dans le cas si certaines conditions méthodologiques y soient remplies. Ces conditions sont les suivantes:

(1) La série anthopologique examinée doit être complète, ce qui veut dire que le cimetière, d'où provient la population en question, soit intégralement exploré. Si les circonstances ne permettent pas une exploration intégrale, au moins l'étendue du cimetière et les données significatives—aptes à une analyse méthodique—doivent être connues.

(2) La connaissance des données archéologiques est d'une importance de premier ordre, surtout en ce qui concerne la période pendant laquelle on s'est servi du cimetière, ainsi que la connaissance approximative de l'ordre chronologique des inhumations.

(3) Exigence fondamentale: l'analyse absolument exacte de la série anthropologique en question, selon le sex et l'âge.

(4) L'emploi des méthodes démographiques répondant aux exigences.

(5) Élaboration archéologique, démographique, et anthropologique compétentes sur le sujet, tout en faisant entrer en considération une conception homogène des travaux.

La justification de notre théorie a été tentée en deux cas classiques: notamment, au cours des dernières années, nous avons réussi d'explorer dans son integralité deux cimetières de grande étendue en Hongrie. L'une de ces fouilles a eu lieu à Polgár-Basatanya (comitat Hajdu-Bihar); ce cimetière date de l'époque énéolithique; l'autre fouille a découvert un cimetière du X et XI^{e} siècles—de plus près des temps de la dynastie des Árpád—à Fiad-Kérpuszta (comitat Somogy). Le premier, le cimetière énéolithique contenait 220 ou 230 tombes environ, on s'en est servi à peu près en 2500–2000 avant notre ère. Le cimetière arpadien de Fiad-Kérpuszta comptait 395 tombes (le nombre des squelettes découverts y a été 405—il y en avait plusieurs à double sépultures). On s'est servi de ce lieu d'inhumation pendant 120 ou 130 ans environ (de 970 à 1100). Jusqu'à l'heure actuelle ce n'est que l'élaboration démographique de ce cimetière du moyen âge qui a été publiée.[3] Dans cette étude nous avons donné une esquisse sur la répartition d'âge des individus excavés. Elle montre une mortalité infantile rélativement modérée de 160 sur mille environ—par contre une mortalité tres forte parmi les jeunes, concernant surtout les jeunes femmes. En général, la mortalité des adultes parait avoir été élevée, l'espérance de la vie, à la naissance, basse. Nous avons mis au point certaines lignes des tables de mortalité de la population du X^e et XI^e siècles, (par exemple espérance de vie, et vie probable survivants). Partant des conditions de mortalité, nous avons estimé le nombre de la population (80 a 90 têtes environ) et le pourcentage des sexes, qui indiquent une majorité du sexe masculin.

L'études paléodémographiques faites sur les deux séries mentionnées, notamment la répartition des morts, les indices bas des vies normales et probables, nous ont amenés à reconnaître l'importance significative de la détermination de l'âge et du sexe. Cette question constitue, comme il était à voir plus haut, la troisième condition de l'enquête. Afin d'élucider ce problème-ci, nous avons procédé à des examens de contrôle dans l'Institut de Médecine Légale de la Faculté de Médecine de Budapest. Pour la détermination de l'âge une méthode complexe a été élaborée correspondant aux visées des recherches paléodémographiques. Lors de nos examens y rélatifs, nous n'avons pris en considération que dans une mesure très modérée la correlation lâche existant entre la synostose des sutures du crâne et l'âge; la détermination de l'âge[4] a été premièrement basée sur l'observation des phénomènes suivants: développement ou régression de l'humérus, l'évasion du canal médullaire s'étendant du côté du diaphysis vers l'epiphysis proximale, construction radiale ou colonnale de l'ossature. A part de ces facteurs, nous avons encore utilisé les conclusions tirées des transformations de la surface du symphysis et de l'abrasion des dents. Reconnaissant la valeur décisive de l'état des dents dans la détermination de l'âge, surtout en ce qui concerne les enfants, nous avons modifié le schéma de Martin en y constituant 18 degrés (d'après les degrés d'usure de la surface masticatoire). Lors de la détermination de l'âge et l'utilisation des données nous n'avons pas perdu de vue les différences existant entre la conception chronologique et biologique. En ce qui concerne la détermination du sexe[5] 20 caractéristiques—en partie morphologiques, en partie métriques—ont été prises en considération.

Les résultats des examens de contrôle ont prouvé la nécessité de modifier nos déterminations, surtout pour l'âge des adultes. Dans le cas de la série de Fiad-Kérpuszta les déterminations principales n'ont pas dû subir des changements notables, bien que les valeurs de l'âge normale et de l'espérance de vie ont dû être sensiblement modifiées; notamment en appliquant les méthodes classiques, l'âge normal se montrait de 30 à 40 ans environ. Ayant fait les examens de contrôle ces chiffres se sont remis à 50 et 75 ans. Cette modification survenue dans la répartition des morts entraîna un changement dans l'espérance de vie moyenne lors la naissance. Celle-ci est montée, en effet, de 24 ans à 34 ans environ.

L'étude approfondie des deux cimetières a fourni un matériel abondant pour la constatation des conditions démographiques. Les résultats n'ont pourtant pas suffi à former la base d'une nouvelle méthode d'analyse anthropologique. Nous ne nous sommes donc pas contentés de la révision des méthodes classiques dela détermination de l'âge et du sexe, nous avons fait de même en ce qui concerne les méthodes démographiques employées. Il nous a réussi de mettre au point un système démographique correspondant aux possibilités offertes par le matériel qui nous a été à disposition.

La méthode démographique élaborée en vue de l'enquête, nous permet de déterminer—par rapprochement stationnaire—le nombre de la population examinée, sa composition concernant le sexe et l'âge. On obtient ces données par calculs faits sur la base de la table de mortalité. De la même manière, les processus démographiques de la population en question, la mortalité, et la fécondité peuvent être élucidées, ainsi que d'autres caractéristiques du même ordre (comme espérance de vie, etc.). Cette méthode des recherches paléodémographiques fournit encore deux données, essentielles à l'analyse génétique; le nombre des générations consécutives et celui des femmes à l'âge de procréation.

L'exploration intégrale d'un cimetière très étendu daté du X^e et XII^e siècles a été exécuté à Halimba-Cseres (Com. Veszprém) déjà compte tenu des cinq conditions mentionnées. Ce cimetière a renfermé 932 tombes, et cette grande série anthropologique a été analysé selon les méthodes modifiées. Nous donnons ici un résumé de cette analyse à laquelle nous avons procédé.

On s'est servi du cimetière pendant 220 à 230 ans environ (de 900 à 1150). La répartition des tombes divise ce cimetière en cinq groupes chronologiquement bien limitables, dont chacun représente une période de 50 ans environ. C'est sur cette base que nous avons pu obtenir des données indiquant le développement de la population et les changements survenus dans les conditions démographiques.

La table de mortalité de la population du X^e et XII^e siècle a été établie selon le principe de la répartition d'âge des individus exhumés (x) égale le nombre des décès à l'âge x de la table de mortalité (dx).

Quant à la répartition des sujets de la table de mortalité concernant la population de Halimba-Cseres, la mortalité infantile, la mortalité des personnes d'âge bas et celle des personnes d'âge normale sont à remarquer. En connaissance de la ligne dx de la table de mortalité on a pu calculer les autres lignes, c'est-à-dire les survivants (lx); le taux de survie (px); et le quotient de mortalité (qx) selon les formules connues dans la bibliographie démographique. Les calculs une fois effectués ont permi d'en conclure une mortalité infantile oscillante entre 40 et 120 pour mille selon les époques, d'une moyenne de 70 pour mille environ. La mortalité des jeunes personnes âgées de 18 à 20 ans approchait les 30 pour mille. Pour l'âge normal de 60 ans, la mortalité dépassait les 100 pour mille.

La mortalité infantile relativement basse a été suivie d'une mortalité extrêmement élevée—de 40 à 50 pour mille environ—parmi les enfants âgés de 2 à 6 ans. Ce qui témoigne un niveau bien bas des conditions de vie en général et surtout d'une hygiène très défavorable. La mortalité rélativement petite des nourrissons s'explique en partie par la répartition selon l'âge des enfants au dessous d'un an, car la mortalité périnatale et celle des nouveaux-nés y sont peu élevées. Entre l'ordre de survie des hommes et des femmes une divergence est à constater laquelle correspond aux différences de conditions de mortalité ayant existé entre les deux sexes. Il est à noter que cette divergence montre une tendance exactement contraire à celle que l'on constate sur les populations de nos jours.

Se fondant sur les tables de mortalité nous sommes arrivés à pouvoir aussi déterminer la durée moyenne de la vie et le pourcentage de la mortalité. L'espérance de vie à la naissance s'est présenté: 30,6 années; le taux de mortalité: 32,7 pour mille (oscillation entre 30 et 40 pour mille). Les valeurs démographiques significatives, obtenues du cimetière de Halimba et celles du cimetière de Fiad-Kérpuszta—datant tous les deux de la même époque—se présentent à peu près identiques.

Après avoir pris connaissance de la situation démographique et ayant déterminé les tendances—qui, d'ailleurs, prouvent qu'au cours de la période en question les conditions de vie ont plutôt empiré—nous avons procédé à la reconstruction du nombre de la population ainsi que de sa composition. Pour déterminer le nombre, nous avons trouvé une formule très facile à calculer:

$$P = k + \frac{D\,e_0}{t}$$

où P signifie le nombre de la population, k est un facteur constant représentant 10 pour cent environ de la valeur de la fraction, D est le nombre des morts, e_0 signifie l'espérance de vie à la naissance, enfin t, la durée de la période pendant laquelle on s'est servi du cimetière. Le nombre de la population a été exprimé en moyenne: 140 têtes. (Ainsi les 80 individus vivant ici au début du X^e siècle n'a atteint que le nombre de 240 au début du XII^e siècle.)

En ce qui concerne la composition de la population c'est le sexe et l'âge qui sont les deux facteurs ayant des rapports étroits et très importants avec la biologie, étant donné qu'ils exercent une influence directe sur la reproduction. Le pourcentage des sexes montre dans la population au dessus de 15 ans une forte masculinité. (C'est seulement la dernière période fait exception à cette règle, laissant supposer soit des événements de guerre, soit des mouvements migratoires.) En moyenne, il y a eu 770 femmes sur 1000 hommes.

En ce qui concerne l'âge la composition stationnaire de la population peut être reconstruite, ce qui présente le meilleur moyen d'approcher la composition réelle. Le résultat de l'examen montre une population de type jeune, avec une tendance de croissance. Le socle de la pyramide d'âges est bien large, mais se rétrécie bien vite devenant enfin asymétrique vu la masculinité et la courte durée de vie des femmes, 41.7 pour cent de la population montre l'âge au dessous de 20 ans.

La reproduction de la population, de plus près les limites des variants génétiques se déterminent en premier lieu par le nombre de la population d'âge de procréation, c'est-à-dire par le nombre des familles procréatives. En nous basant sur le nombre des femmes âgées de 15 à 49 ans, nous estimons le nombre de ces familles à 23 au début et à 64 à la fin de cette époque, ce qui donne une moyenne de 30. Le nombre des familles et des fractions de famille a pu éventuellement être un peu plus élevé. Le nombre des générations consécutives—l'autre donnée de grande importance pour l'analyse génétique—est à estimer, sur la base de l'espérance de vie, à 7, pendant la période de 220 ans environ.

Les recherches paléodémographiques ont encore fourni bien d'autres données contribuant à la connaissance de la population historique examinée. Il est impossible de nous étendre ici sur tous les détails, mais nous croyons avoir prouvé par ce que nous venons d'exposer que ces recherches se prêtent à une reconstruction assez large d'une population; de ses conditions de vie et de son développement.

L'anthropologie historique, qui poursuivant le même but sur le domaine biologique, ne peut évidemment pas renoncer à l'emploi de ces résultats, ainsi que les analyses paléodémographiques sont réduit à utiliser l'appui fourni par l'anthropologie physique et par l'archéologie. En fin de compte, ces sciences apparentées poursuivent les même buts, par conséquent leurs voies sont convergeantes.

Il est pourtant à remarquer qu'à part quelques exceptions la reconstruction mentionné tout à l'heure n'a pu être approchée que dans une mesure fort restreinte par les recherches de l'anthropologie historique. Les anthropologistes ont donné la primauté à la classification, sans avoir eu une connaissance assez ample des caractéristiques qui doivent pourtant y servir de fondement. Cette lacune ne serait guère comblée par l'emploi des méthodes métriques et biométriques. Cela se manifeste surtout dans le fait qu'au cours des dernières années plusieurs des anthropologistes ont tenté d'y trouver une voie nouvelle et les études actuelles sont toujours caractérisées par la recherche de nouveaux principes directeurs.

Les visées immédiates de la plupart des recherches de l'anthropologie historique, effectuées jusqu'à nos jours ne représentait qu'un progrès très réduit vers la compréhension de l'évolution du genre humain, bien que cette question est le problème central de l'anthropologie. Ces recherches se proposaient de trouver, à l'aide de la classification, des réponses à certaines questions obscures remontant loin dans le passé de l'homme et des populations. Par contre les nouvelles tendances anthropologiques s'efforcent surtout à élucider les processus d'ordre biologique.

Nous voilà donc revenus à la question qui nous occupait à la préface. Dans quelle mesure les recherches paléodémographiques peuvent-elles augmenter et compléter les possibilités de l'analyse anthropologiques? Les résultats des recherches paléodémographiques peuvent, à eux seuls, fournir des renseignements fort précieux au sujet des populations de certains âges. Pourtant, leur importance anthropologique réside ailleurs. C'est l'archéologie qui est appelée à définir la structure chronologique des matériaux à examiner. Considérant la question du point de vue anthropologique, la démographie historique représente la connaissance des structures et des tendances biologiques et ethniques des anciennes populations. S'appuyant sur cette base, l'anthropologie historique aura des possibilités bien plus vastes, car elle pourra connaître grâce à une reconstruction qui se fait sur des fondements concrets, les unités biologiques fondamentales des populations d'autrefois ainsi que la succession de ses unités. Par là, elle se trouvera en mesure de définir la structure génétique d'une population donnée.

De quelle manière la structure génétique peut-elle être approchée? Ayant défini la structure chronologique et démographique d'une population, étant ainsi en connaissance de son nombre, de ses générations successives, et de son ordre de reproduction. A l'intérieur de ce domaine se produisent les variations des phénomènes biologiques. Ces variations ont une importance spéciale surtout dans les cas de populations premières. Car c'est sur cette base que nous pouvons les suivre par la suite des générations. En fin de compte, nous pourrons arriver par des moyens démographiques à connaître les relations biologiques entre les individus, ainsi que le caractère homogène ou hétérogène d'une population. La série anthropologique du cimetière de Halimba-Cseres fournit un excellent matériel à cet effet. A part d'autres caractéristiques anthropologiques, à peu près 160 variations anatomiques s'offrent à l'examen de l'investigateur. La structure génétique de la population doit être définie à l'aide des méthodes métriques adaptées à ces caractéristiques et aux variations, ainsi que par l'étude statistique des phénoménes observés. C'est après avoir terminé l'analyse génétique que nous pouvons nous mettre à élucider les problèmes typologiques et à esquisser la structure typologique.

Dans cet ordre d'idées, la paléodémographie présente une nouvelle possibilité de l'anthropologie historique, en lui assurant des fondements plus solides pour ses recherches et, par là, des résultats beaucoup mieux fondés.

Budapest, Hungary.

Notes

1. M. Pearson: On the Change in the Expectation of Life . . . *Biometrika,* 1 : 261–264, 1901–1902, London.
2. Vallois, L. Henry, Nougier, Angel, Giot, Senyurek, G. Kurth, Franz et Winkler, Macdonnell, Willcox, Gheorghiu, Bartucz, Schaffer, Skerly, Gomme, Donnel, Hooton, Goldstein, Forrest, et autres.
3. J. Nemeskéri–G. Acsádi: Történeti demográfiai vizsgálatok, *Archeologiai Értesitő,* 79 : 134–146. Budapest, 1952.
4. Cf. les recherches y relatives de Schrantz, Bernát, Graves, Brooks, et de Hansen.
5. A la base des méthodes élaborées par R. Martin, Hooton, et Hrdlička.

SIGNIFICANCE OF RECENT PRIMATOLOGY FOR PHYSICAL ANTHROPOLOGY

Adolph H. Schultz

One of the chief and most basic values of primatology for anthropology consists in the gradual recognition of and distinction between that long list of characters which man has in common with apes and monkeys and that comparatively short list of those qualities which have become different and specialized in man alone. Our best hope for general agreement in the reconstruction and explanation of man's evolution lies in more primatological research. The fact that so far we have not yet progressed along this line any farther is largely due to the one-sided limitations of most of the comparative work undertaken in the past. The best-known example of this one-sidedness is in the striking preference for primatological studies of skulls and teeth, due not only to their importance in connection with fossil finds, but also on account of their availability in our collections. In trying to edit a systematic handbook of primate anatomy I have become fully aware of the enormous differences in the amount of available data for the various organs. While the chapters on skeletal parts are difficult to condense into 100 pages each, those for the endocrine glands, e.g., are still pitifully small, and limited to observations on a few scattered species. It must suffice here to report an encouraging new trend to fill these long-neglected gaps.

Among other limitations in the older work on primates which are being overcome there are two essential ones which I would like to discuss briefly to emphasize their significance for anthropology and to illustrate the inhibiting influence of entrenched assumptions. All too many anthropologists for far too long a time have taken it for granted that the non-human primates must be far less variable than recent man and that, hence, one or a few of a kind suffice to reveal typical conditions. In a similar manner and with few exceptions we used to be satisfied with comparisons between "adults" or "newborns," assuming that we are dealing with precisely comparable stages of life in whatever primates we have selected. The newer literature on primates has convincingly demonstrated that both these old beliefs are untenable.

Today it has become hazardous to speak of "the" chimpanzee, since in many respects they have been found to vary so extensively that it seems far safer to refer merely to the particular, limited series one was able to examine, realizing that some other series might very likely contain very different variations. For instance, in all the several hundred chimpanzee skulls I had seen in former years the M3 were well developed, and only among the last fourteen that I have just obtained from Africa is there one with a mere vestige of an M3, and two with congenital lack of these teeth. Among 350 skulls of *Pan troglodytes* there was a fronto-temporal contact in 99% of the cases, but among the recently discovered *Pan. t. paniscus* from the left bank of the Congo I have not yet seen this condition, but instead, only the parieto-sphenoidal contact, typical for man.[1] The number of palatine ridges has become reduced to an average of only 4 in man with variations reaching in very large series from 2 to 7. On the basis of only three observations it had been claimed repeatedly that the chimpanzee stands nearest to man in this respect, but among 44 of these apes I found these ridges to vary from only 5 to as many as 15, 10 being most frequent. This is another clear proof that real help from studies on non-human primates can come only through a full consideration of their often surprisingly high variability.

The number of tail segments or coccygeal vertebrae has become greatly reduced in all higher primates. In man these vertebrae number anywhere from 3 to 6, at least after embryonic life, with an average number of more than 4. In 308 gibbon skeletons the coccyx consisted of anywhere between 0 and 6 segments and the average amounts to only 2.6. This is again a character more variable in an ape than in man and, incidentally, one of many which teach us that a condition in an ape is not necessarily more primitive or less specialized than the corresponding character in man, as has been taken for granted far too often.

Nissen recently dwelled on the high variability of behavior in chimpanzees reared in a uniform environment. With this sort of new data it has become clear that much of the older primatological information was merely tentative because it was based on too little material. For a growing list of features it can be shown today that their ranges of variations in large series overlap in man and one or another of the non-human catarrhine primates, and that, hence, their value as differentiating features is strictly limited in contrast to such other characters of great phylogenetic significance whose variations are really specific in all cases.

The importance of the factor of *age* is becoming appreciated more slowly than that of the factor of variability. Ever since Haeckel formulated his biogenetic law, according to which ontogeny is supposed to be an abbreviated repetition of phylogeny, we have tried to use evidence from ontogenetic changes for theories on human evolution. Zoologists have come to realize that ontogeny does not really recapitulate phylogeny, but that changes in ontogeny create phylogeny, a vastly different and much more fruitful viewpoint. Thus Bolk had come to formulate his fetalization theory as an attempt to explain man's specializations as results of retarded development and even of retention of fetal characters. Today, however, we regard this as an insufficient theory, though as one on the right track, inasmuch as Bolk did show the evolutionary effect of *some* ontogenetic alterations. The latter, we have since discovered, consist of just as many accelerations as of retardations, and both such ontogenetic changes have affected not only man but also other primates, and this in widely different degrees.

To mention a few examples as briefly as possible: a central bone in the wrist, an old carpal element among mammals, is normally lacking in adult man, gorilla and chimpanzee, while present in all other apes and monkeys. Even in the former three it is still laid down regularly in embryonic life, only to fuse with the navicular bone at or soon after birth in the African apes and early in pre-natal life in man. The testes leave the abdominal cavity to descend permanently into the scrotum at widely different ages among primates, in most species not before some time during infantile or even juvenile life, but in man already in the 8th month of the fetal period, though in a few races this process has been reported to be markedly delayed in a very considerable percentage of cases. The pigmentation of the skin appears soon after the middle of fetal life in monkeys, later—but also well before birth—in apes, and not until shortly before term in the colored races of man. The sharp break in the curvature of the spinal column at the lumbo-sacral border, the promontory, can no longer be regarded as a unique possession of man, since it develops also in the apes, though much later in life and hence remaining much less marked than in man.

One could mention a great many more such newly found examples of accelerations and retardations in man's ontogenetic processes which bring us nearer to an understanding of man's evolution than the old mere descriptions of differences between adults of man and apes. Some of these ontogenetic modifications leave no effect on the conditions found in adults, while others can result in significant differentiations reached only at the end of growth. Thus, the sequence of eruption of the permanent teeth differs widely in apes on one side and most human races on the other, but the final dental formula is the same in all. Here we are in the fortunate position to show by means of fossils and even prehistoric remains that this particular ontogenetic change of man has been a very recent one, probably connected with the extreme prolongation of our postnatal period of growth. As an example of an ontogenetic innovation in man, leading to a steadily widening specialization during growth, I may mention the relative position of the atlanto-occipital joint. This important joint has come to lie far forward in fetuses of simian primates, in contrast to all other mammals. In monkeys and apes, this joint shifts back after birth in varying degrees, while in man it retains its fetal position practically unchanged, thereby keeping the near-equilibrium of the head, so advantageous for the erect posture.

Naturally such comparative ontogenetic findings offer as yet no real explanation for man's specializations, but they bring us nearer to ultimate explanations than mere statements of adult man's uniqueness in this and that respect. Experimental embryology and endocrinology are making rapid progress in their analysis of growth processes, especially regarding retardations and accelerations of development, so that we are justified in hoping for explanations after we have discovered all the facts concerning those age changes of man which have become modified. We can describe the age changes of man in great detail for at least a few races, but a lot of work remains to be done before we can decide which of these changes do not also occur in non-human primates in the same sequence, at corresponding physiological ages, or with the same intensity, etc. The relatively new primatological research on the age changes of monkeys and apes is still hampered by the lack of suitable preserved material and the scarcity of opportunities for repeated examination of living animals. We can no longer limit such ontogenetic studies to the pre- and post-natal periods of life in which the developmental changes are most evident and rapid, but must consider fully also the continuing slower changes after maturity. It is during adulthood that various changes occur in apes which in man take place much earlier in life, thereby resulting in merely temporary differences which are often mistaken for permanent ones. I may merely mention here the example of the mastoid processes, which develop in man very early to reach their full size as soon as the dentition is complete, whereas in the African apes they are barely indicated at the latter stage of development but later can grow to very large size in old age. Similarly the part of the breastbone, known as corpus sterni and consisting of multiple pieces in the young of all primates, becomes solidified into a single bone in practically all human beings aged 18 to 20 years, whereas in the apes not until their teeth have become extensively worn with age.

Primatology of today with its emphasis on variability and on age changes contributes a growing mass of reliable data which are of greatest help in evaluating the human conditions we are able to describe in detail. For instance, we have known for some time that in body weight women attain roughly 84 per cent that of men. That this represents a very conservative sex differentiation can be recognized from recent findings on large series showing that the corresponding average percentage fluctuates among monkeys and apes between less than 50 and more than 100, with surprisingly large differences in some closely allied species. This fact should warn students of fossil hominids to interpret differences in size with great care, and as sex differences only if it can be supported by other evidence. Differences in body size, whether due to sex, variability, or species, can change the proportionate size of bodily parts according to laws of allometric growth which we have just begun to analyze. This has long ago been demonstrated for the relative size of the brain and of the neurocranium, and more recently for that of the eye and orbit. It now appears that many other proportionate sizes in the body are also influenced in varying but significant degrees by the general size of the individual.

In trying to visualize the life of early man and for any reasonable speculations on the origin of human society, we should first collect more hard facts concerning the social behavior, the composition of the group according to age and sex, the causes of death, etc., in wild non-human primates. In this promising field of primatological interest progress is disappointingly slow and still limited to only few representatives of the many different types of primates. We have already learned, however, that some basic conditions can vary extensively from species to species, especially such items as the percentage relations between young, mature, and senile individuals within a group, between adult males and females, and between pregnant and non-pregnant females of fertile age. In some monkeys and apes the sexes exist in about equal numbers at all ages while in others there are, at least among adults, far more females than males. Among the few species for which we have gained fairly reliable data, the ratio between young and old individuals seems to correspond closely to the condition typical for man in some instances, but in others the proportion of immature specimens is very small, as also is that of pregnant females. In attempting to find the causes of such discrepancies, it appears that diseases play the main role. There exists good evidence that diseases, accidents, and even malformations are unexpectedly prevalent among wild monkeys and apes, and that they are certainly much more potent influences in the growth or extinction of populations than are the large predators. Healed fractures, arthritic changes, and dental diseases are particularly common in the man-like apes in which they become rapidly more frequent with advance in age. We are justified in assuming that early man was not exceptional among the higher primates, but also had to pay the price for phylogenetic increase in longevity which is not accompanied by an improvement in the durability of the organs.

Work on fossil primates has made very valuable gains in recent years through the discovery of the new and extremely interesting representatives of higher primates: *Limnopithecus*, *Proconsul* and the Australopithecides of Africa and *Oreopithecus* in Europe. For the full appreciation of these fortunate finds we still need many more comparative data on the effect of age, sex, size, and variability in recent primates. Only with such data can every available detail of the fossil fragments become utilized for the final determination of the exact position of the fossils on the primate pedigree. It is in this connection that the closest possible cooperation is needed between palaeontologists and primatologists trained with the viewpoints and methods of physical anthropology.

In conclusion I would like to point out that there exist today a great many opportunities for new primatological investigations of immediate and great interest in anthropology. We have finally learned to keep monkeys and apes in captivity in really good health, as proved by the success of breeding and raising them in modern zoos, in which we could and should collect more information regarding all developmental changes in morphology, physiology, and behavior. It is in captivity that many instances of hybridization have already occurred, and that more can be deliberately planned for genetic studies of a sort which could never be provided by crosses between human races. In various places and for different reasons monkeys are living today in environments, new to them, so that it has become possible to investigate the effect of a new habitat and to find out whether there occurred similar rapid changes as have been found repeatedly in the offspring of human emigrants. Finally, with the new means of transportation it has become far less time-consuming to reach the homeland of wild monkeys and apes for the much needed continuation of such work on social behavior, as had been started so brilliantly by Carpenter. By air it has become possible for the first time to import many live primates, such as the tarsier, which formerly had never survived long sea voyages. New opportunities have thereby appeared for a multitude of investigations of direct and great interest for the study of man.

Zurich, Switzerland.

Note

1. Two days ago Dr. Vandebroek, based on a study of many more paniscus, has reported some cases of fronto-temporal contact.

NEW RESEARCH IN GERMAN FORENSIC ANTHROPOLOGY

Ilse Schwidetzky

The anthropological determination of paternity in forensic cases of uncertain descent, particularly those of illegitimate children, is a method of applied anthropology which has been developed and most frequently practiced in Germany and Austria. Every year several thousands of expert opinions are presented to the courts. I need hardly state that German physical anthropology is not by any means restricted to this practical application but is concerned in fact with all fields of human biology, particular attention being paid to problems of genetics, population biology and biopsychology. Since anthropology in Germany, compared with other countries, is characterized by a considerable amount of work being done in the field of paternity diagnosis, I prefer to report on this topic before this international gathering, all the more so since Germany will not be represented by other papers from the field of human biology.

The principal basis of the diagnosis of paternity is the same as that of twin diagnosis: the analysis of similarity in as many hereditary traits as possible. Blood groups and other serological traits are usually determined in advance. They frequently permit the exclusion of the possibility that a suspected man is really the progenitor of the child in question, but they cannot make positive assignment. However, this is possible by the similarity method, which compares child, mother and suspected progenitor (or suspected progenitors) in a large number of polymeric traits. The greater the number and the degree of similarities between the child and the man in question, the more probable it is that he really is the child's father.

Inasmuch as at the present time about 10% of the paternity cases remain undecided and others cannot be decided with complete confidence, further research is needed. There are several avenues for development of the anthropological diagnosis of paternity.

1. New hereditary traits suitable for the analysis of similarity are being sought. During recent years, for example, Loeffler published his impressive family studies on hair streams of the neck; Tillner studied palm lines which are, in addition to palm ridges, suitable hereditary traits; Matsunaga examined two traits frequently studied in Japan in relation to paternity diagnosis: hair growth on the middle phalange of the third finger and ear wax. It is true that the possibilities of discovering new good traits are limited, but as yet not all possibilities have been exhausted.

2. Attempts have been made to determine more precisely the variability of hereditary traits already known. Particularly metric characters have been studied. Standard deviations of a number of measurements and indices, classified according to age and sex, have been published on the basis of data from Munich (Wünsche) as well as from Cologne and Hamburg (Schade). Thus, the metric traits which change greatly during growth can be compared among children, mothers and suspected progenitors.

3. Statistical methods have been examined for the more precise summary of the detailed findings. Two methods have been discussed and tested:

(a) The Essen-Möller method which calculates the probability of determining the "right" father on the basis of the critical values Y/X of numerous single traits, where X is the frequency of conformity between the children and the "right" father, and Y the frequency of conformity between the children and the "wrong" father.

$$P_r = \frac{1}{1 + \frac{Y_1}{X_1} \cdots \frac{Y_n}{X_n}}$$

Keiter has altered this method by his "logarithm of paternity" which does not multiply the critical values but rather adds their logarithms together.

(b) Discriminatory analysis in the form suggested by Penrose has been applied for the first time to the determination of paternity by Baitsch and Bauer (as yet unpublished).

4. Families with unquestioned fathers were studied to define more exactly the variability of "total resemblance." In fact, only family research can really prove the validity of methods for the determination of paternity. Inasmuch as this is my own (partially unpublished) contribution to forensic anthropology, I should like to report on it in somewhat more detail.

100 families with 373 children were studied with the same methods as are used in forensic cases. Each child was compared with the mother, the unquestioned father and an unquestioned non-father who was always the father of the next family in this study. About 150 morphological traits were considered. The detailed findings have been summarized by several methods:

(a) The "visual similarity method" (Schauverfahren) summarizes similarities in the single traits into a "total resemblance" with 7 degrees, varying from −3, which signifies greatest dissimilarity, to +3, which signifies greatest similarity. This is the method used by most experts in forensic cases. It is true that this kind of summary can only be an approximation, as there are no clearly defined limits between the several degrees of total similarity or of similarity at all for that matter, and the personal error is an important factor in condensing a great many observations into one discreet judgment. But this method also permits the consideration of very rare traits, extremely fine details, and of special complex structures which cannot be proved statistically but which may be nevertheless very important for the diagnosis. There were not only cases in this family material in which one could not discriminate between "right" and "wrong" fathers by the use of the anthropological method but also cases where a false diagnosis would be made. Among the 373 children there was one child who was more similar to the unquestioned "wrong" father than to the unquestioned "right" father. But such false diagnosis may be expected only in the low degrees of total similarity. It has been calculated on the basis of the distribution of degrees of total similarity (as shown in Figure 1) that a similarity of +2 between a child and a "wrong" father may be expected in only 0.03% of the cases. Only once in two million cases is there a chance that a "right" father whose similarity to the child is −2 will coincide with a "wrong" father whose similarity is +2. In other words: one would make once in two million cases a false diagnosis in assuming that the man who appears more similar to the child in question is in reality the father.

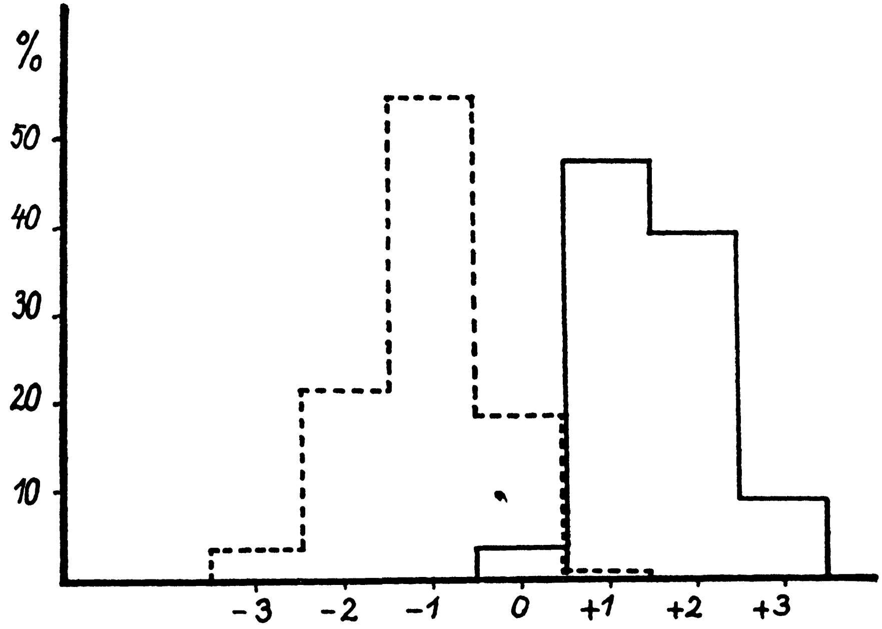

Fig. 1. Distribution of the Degrees of "Total Similarity" between Children and Unquestionable Fathers (full line) and Children and Unquestionable Non-Fathers (dotted line)

(b) For the first time the Essen-Möller formula was applied to the family material. Up until now it had been applied only to forensic cases. The greatest number of traits, 31 in all, was summarized and the greatest number of cases (120) were tested by Wichmann. So-called "right" and "wrong" fathers were very well separated, as can be seen in Figure 2a. There was no overlapping of the distribution of probabilities of the "right" fathers with that of the "wrong" fathers. This may demonstrate the reliability of the tested paternity diagnosis. But it does not represent the distribution of Essen-Möller probabilities in an unselected series of fathers and non-fathers, for only those cases could be tested statistically in which the visual similarity method was successful in determining paternity or non-paternity; the undecided cases could not be considered.

Indeed, the family material shows quite a different distribution. There is strong overlapping of fathers and non-fathers, as is shown in Figure 2b. The probabilities of unquestioned fathers drop to 0.3% while those of unquestioned non-fathers rise to 98.7%. In other words, there were many (14%) improbable fathers with probabilities less than 50% and many (also 14%) of probable non-fathers with probabilities higher than 50%. Fathers and non-fathers are well separated only beyond 99% and 1%. Therefore, the Essen-Möller formula was abandoned for further analysis, and Keiter's "logarithms of paternity" were used in its stead. They have a normative distribution and are hence a kind of discriminant function, where the "supertraits" are represented by the sum of the logarithms of critical values (which are multiplied by the Essen-Möller formula). It could be doubted, as Bauer has done, whether this is the

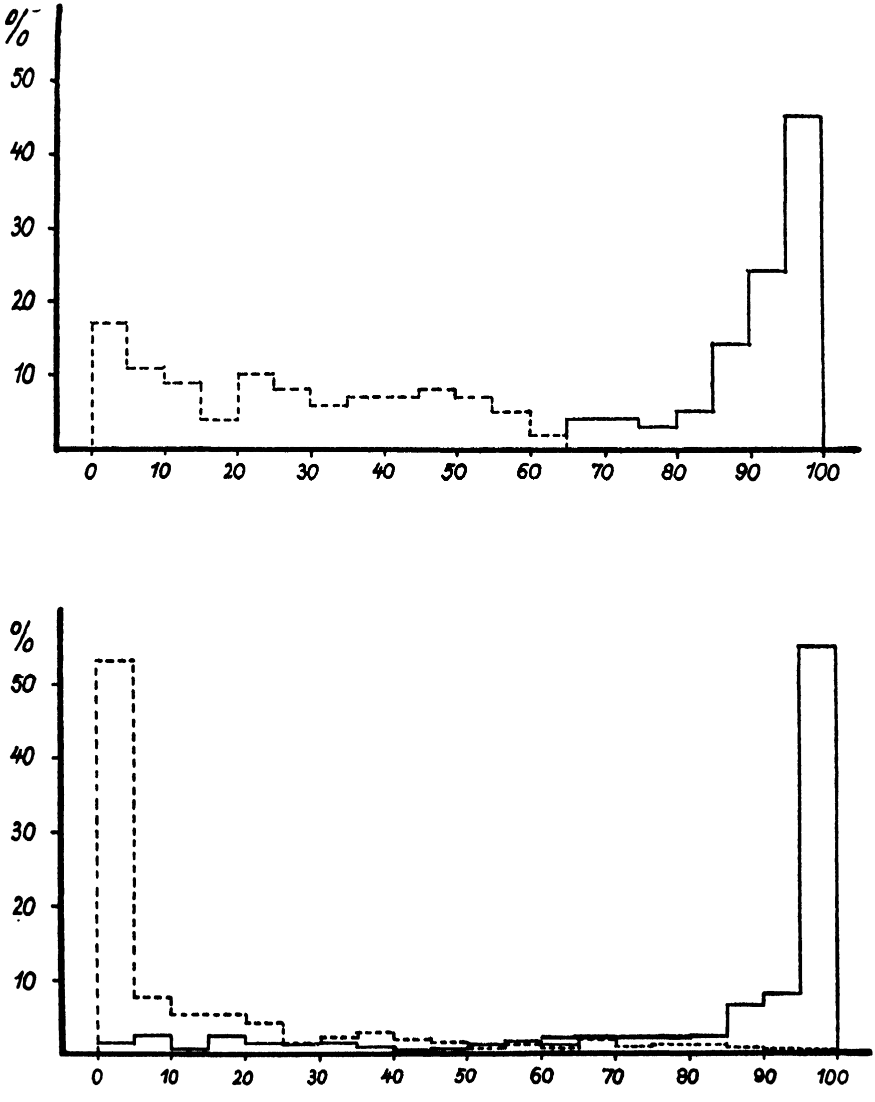

Fig. 2. Distribution of the Degrees of Probability According to the Essen-Möller Formula
(*a*) In Forensic Cases (After Wichmann)
(*b*) In Families
Full line: Fathers
Dotted line: Non-Fathers

best method of calculating "supertraits," but up to the present no other kinds of discriminant functions have been applied to family material.

Figure 3 shows five sets of paternity logarithms differing in the number of traits. The more traits that are included, the better fathers (solid line) and

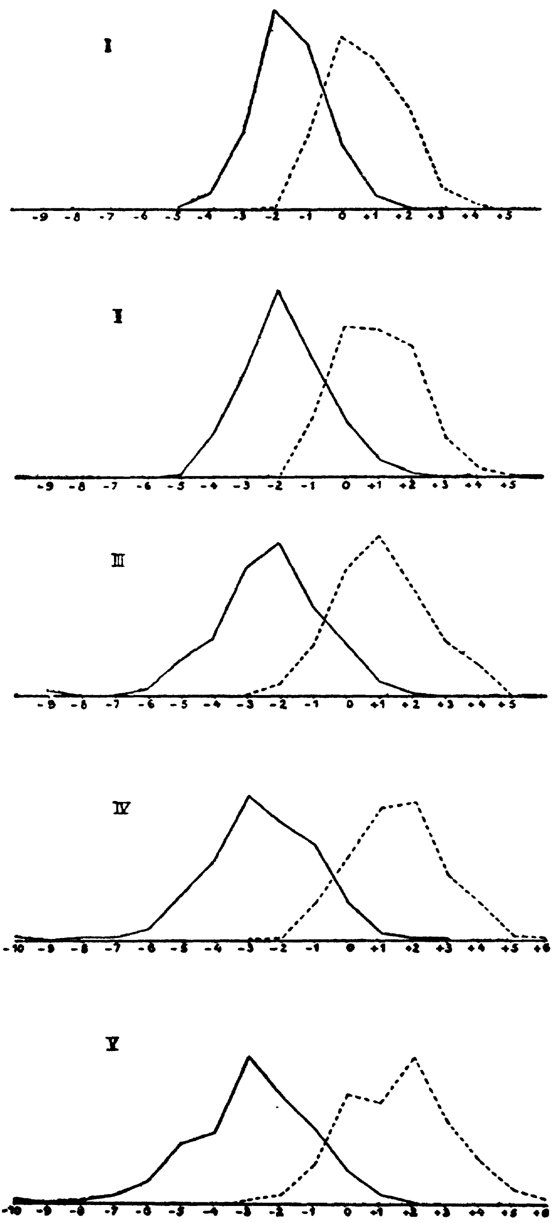

Fig. 3. The Distribution of "Logarithms of Paternity"
I 25 traits
V 70 traits
Full line: Fathers
Dotted line: Non-Fathers

non-fathers (dotted line) can be separated. Although the first set summarizes the best traits, that is, those which are the least influenced by sex, age, and environment, the separation is not so good as in the case of larger sets where more "bad" traits were included. The number of false diagnoses drops from 4.6% in the first set of 25 traits to 1.1% in the fifth set of 70 traits.

The correlation between paternity diagnosis by the visual similarity method —with 7 degrees of "total similarity"—and by the statistical method is rather low: 0.32 for fathers and only 0.05 for non-fathers. This means that each of the two methods has its own advantages and disadvantages; each of them points out similarity relations which are not revealed by the other method. The visual similarity method permits the consideration of very specific structures and evident similarities in rare traits which cannot be proved statistically. On the other hand, the statistical method sums up more precisely small conformities and differences which are not evident in the visual method. Therefore, the simultaneous use of both methods will give the best results, that is to say, the smallest number of false diagnoses and the highest number of clearly decided cases.

No methods employed thus far have succeeded in discriminating perfectly between "right" and "wrong" fathers. 100% accuracy can probably never be attained. The limits of false diagnoses can at least, however, be established, and thus with careful analysis false diagnoses can be avoided with sufficient accuracy for all practical purposes.

It is possible that the methods developed in forensic anthropology may prove useful for other purposes as well. It is a genetical method diametrically opposed to the Mendelian analysis of single traits. It attempts to determine the degree of genetic relationships, by which we mean the portion of genes held in common by two persons or two populations. It is obvious that such a method may be useful also in the fields of racial analysis, social biology, and so on. It may be an avenue not only to practical analysis but also to a genetic theory of similarity.

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PARTIAL VOLUMES AND SURFACE AREAS OF THE HUMAN BODY

Božo Škerlj

INTRODUCTION

To evaluate the whole body volume (V) in man, the safest technique seems to be to submerge the subject in a medium like water or air in a container, and to measure the volume of the displaced medium. To estimate a person's body surface area (SA) one may use the formulas based on stature and weight developed by various authors, or DuBois' nomogram, which supply rather crude yet practical information. But there are problems, such as volume distribution in, and heat radiation from, various body parts, which can be mastered only by evaluating partial volumes and surface areas. Though something is known about sex and age differences regarding the whole body volume and surface area, very little, if anything, is known about partial volumes and partial surface areas. This paper is mainly concerned with partial volumes and surface areas, as well as with their relations at various age levels.

TECHNIQUE AND MATERIAL

To get the basic formulas, the human body is conceived as the sum of ten geometrical bodies which stand for: head with neck, trunk, both upper arms, both forearms with hands, both thighs, and both legs with feet. All these parts, except thighs, are considered as cylinders, whereas the thighs as truncated cones. To obtain the volumes and surface areas of them, one has to know the circumferences, radii, and heights (lengths) of the pertinent body parts, and to apply the known formulas for the volumes and surface areas for the abovementioned geometrical bodies (Škerlj, 1954a, Škerlj and Kulčar, 1956).

As a pilot study, four series were investigated: two of college age comprising nine women and eight men, and two series of 45 women and 90 men of various ages from school children to adults, from the island of Susak (Škerlj, 1955, 1956).

RESULTS

Except for the lower limbs, the absolute values of the partial volumes and surface areas (Table 1) are lower in women than in men, whose stature is 161.7 and 170.6 cm. respectively. The average SA/V relation for the whole body amounts to 2.93 sq. dm./cubic dm. in men, and 2.90 in women.

As expected, the SA/V relation is much lower in the trunk—the bulkiest body part—than in other parts. Second to it are, in men, the head with neck, in women the thighs. In agreement with the fact that, in women particularly, thighs are rounder, the biggest sex differences appear in the lower extremities.

Considering (Tables 2 and 3) first the sums, *i.e.*, the values for the body as a whole, it appears that the whole body surface areas and volumes gain intensively, in both sexes, from the first to the third age group. In conformity with

TABLE 1. Surface Area, Volume and SA/V Relation in Women and Men of College Age

	Head with neck		Trunk		Upper arms		Forearms with hands		Thighs		Legs with feet		Total body	
	m	w	m	w	m	w	m	w	m	w	m	w	m	w
Surface area/sq. dm./	16.8	14.8	43.6	41.5	18.8	16.5	23.5	20.4	36.6	36.5	32.7	33.2	172.0	162.9
Volume/cubic dm./	5.20	4.36	28.36	25.97	3.50	3.03	4.62	3.57	9.41	11.61	7.85	8.35	58.94	56.89
SA/V relation	3.14	3.41	1.54	1.61	5.40	5.62	5.16	5.75	3.96	3.30	4.18	4.02	2.93	2.88

TABLE 2. Volume/cubic dm./, Surface area/sq. dm./ and SA/V Relation in Males of Various Ages

Age in years	6–10			11–15			16–20			21–55		
	SA	V	SA/V	SA	V	SA/V	SA	V	SA/V	SA	V	SA/V
Head with neck	11.56	3.121	3.70	13.25	3.579	3.70	16.50	5.133	3.21	16.90	5.387	3.14
Trunk	24.83	11.531	2.15	32.05	17.891	1.79	44.93	29.348	1.53	48.98	34.230	1.43
Upper arms	8.51	1.042	8.17	12.27	1.859	6.60	18.76	3.625	5.17	20.75	4.137	5.01
Forearms + hands	12.39	1.572	7.88	16.55	2.588	6.39	25.57	5.086	5.03	27.05	5.433	4.98
Thighs	18.60	2.677	6.95	26.20	4.628	5.66	36.58	8.300	4.41	37.18	8.651	4.30
Legs + feet	17.06	3.141	5.43	25.72	5.510	4.67	35.43	9.038	3.92	35.70	9.090	3.93
Sum	92.96	23.084	4.03	126.04	36.056	3.49	177.77	60.531	2.94	186.56	66.928	2.79

TABLE 3. Volume/cubic dm./, Surface area/sq. dm./ and SA/V Relation in Females of Various Ages

Age in years	6–10			11–15		
	SA	V	SA/V	SA	V	SA/V
Head and neck	11.51	3.032	3.70	13.59	3.888	3.49
Trunk	24.46	11.386	2.15	34.02	18.932	1.80
Upper arms	8.41	1.050	8.01	12.60	1.933	6.52
Forearms + hands	12.48	1.635	7.63	18.23	2.802	6.51
Thighs	18.85	2.972	6.34	29.52	6.079	4.86
Legs with feet	19.23	3.525	5.45	27.68	6.256	4.42
Sum	94.94	23.600	4.02	135.64	39.890	3.40

Age in years	16–20			21–50		
	SA	V	SA/V	SA	V	SA/V
Head and neck	14.52	4.333	3.35	15.18	4.547	3.34
Trunk	41.23	25.741	1.60	42.32	27.205	1.55
Upper arms	15.71	2.869	5.47	16.61	3.071	5.41
Forearms + hands	20.70	3.552	5.83	20.20	3.400	5.94
Thighs	35.35	10.060	3.51	34.29	9.373	3.66
Legs with feet	32.12	7.754	4.14	32.18	7.998	4.02
Sum	159.63	54.309	2.94	160.78	55.594	2.89

the changing body volumes and surface areas, the SA/V relations decrease, *i.e.*, improve from the point of view of heat loss. In the female age-group of 21–50 years, the SA/V relation is in agreement with that stated in women of college age, whereas in men the group of 16–20 years of age agrees with the college men. The over-all trend through all ages is the same as stated in a former study (not published as yet) and as expected; though his trunk is relatively bulkier, a child has a relatively larger surface area than an adult, because the child is smaller.

As stated earlier (Škerlj, 1955), the relative volume of the head with neck decreases, in both sexes, from about 6–20 years of age; in men, later on it gains somewhat again. The relative trunk volume, which is about 50 per cent of the whole body volume in children of 6–10 years of age, later decreases until about 20 years; thereafter it gains, particularly in men. The relative volume of the upper arm increases intensively up to about the age of 20 years. The relative volume of the forearm with hand increases in men, at least until about 20 years of age, whereas in women it gains only until puberty and later decreases. The relative volume of the thighs increases, particularly in females, up to about 20 years of age; later on it decreases in both sexes. The relative volume of the leg with foot stops gaining even earlier (about 15 years of age), and decreases thereafter.

Females have a slightly lesser relative volume of the head with neck than males till about 20 years of age only. They do have a less voluminous trunk through all ages under consideration. The relative volume of the upper arm is a little smaller in females than in males, whereas the females' forearms with hands become relatively less voluminous than in males, particularly in the adult age group. The relative volumes of the female thighs and legs with feet participate, in all age groups, considerably more than that of males in the whole body volumes.

TABLE 4. SA and V Distribution in Males in Percentages of the Whole Body SA and V Respectively

Age in years	6–10		11–15		16–20		21–55	
	SA	V	SA	V	SA	V	SA	V
Head and neck	12.5	13.6	10.4	10.3	9.3	8.5	9.1	8.1
Trunk	26.8	50.0	25.5	49.4	25.3	48.7	26.3	51.1
Upper arms	9.0	4.5	9.6	5.0	10.6	6.0	11.0	6.2
Forearms with hands	13.3	6.8	13.3	7.3	14.3	8.1	14.5	8.1
Thighs	20.0	11.5	20.7	12.6	20.5	13.7	19.9	12.8
Legs with feet	18.2	13.6	20.4	15.3	19.9	14.9	19.1	13.7

TABLE 5. SA and V Distribution in Female Age Groups in Percentages of the Whole Body SA and V Respectively

Age in years	6–10		11–15		16–20		21–50	
	SA	V	SA	V	SA	V	SA	V
Head and neck	12.2	13.0	10.1	10.0	9.1	8.0	9.5	8.3
Trunk	25.8	48.2	25.1	47.6	25.9	47.4	26.3	48.9
Upper arms	8.7	4.4	9.3	4.7	9.8	5.3	10.3	5.5
Forearms with hands	13.1	6.9	13.4	7.0	13.0	6.5	12.6	6.1
Thighs	19.8	12.6	21.7	14.9	22.1	18.5	21.3	16.8
Legs with feet	20.2	14.9	20.5	15.7	20.1	14.3	20.0	14.3

As to partial surface areas, the trend during aging is approximately the same, *i.e.*, the surface areas roughly follow the volumes. However, the relative partial volumes of the limbs are always considerably below the pertinent relative surface areas (Tables 4 and 5). In agreement with physical laws, in the thighs (and to a lesser degree also in the legs) the rapid increase of the volume, in the third age-group, is accompanied by a very slow absolute increase of the surface areas, whereas the SA/V relations become much more favorable.

On the other hand, whereas the volume of the trunk is about a half of the whole body volume, its surface area is only about a quarter. It is clear, then, that the trunk with its relatively small surface area may be compared to an oven, while all other body parts serve as heat radiators.

The SA/V relation is most favorable, in all age groups, in the trunk, whereas it seems to be worst, in prepubertal children, in the upper arms, later on in the forearms with hands. Tables 2 and 3 show the changes in detail. Therefore it appears that, in both sexes, the most favorable relation is reached in the head with neck, trunk, and upper arms by the oldest age-group. In the forearm with hand the most favorable SA/V relation is achieved, in women, by the third, in men by the fourth age-group. The same holds also for the thigh which in women —as shown by the measurements of O'Brien and Shelton, 1941, Škerlj, Brožek and Hunt, 1953, Škerlj, 1954, 1955—loses in circumference and subcutaneous fat thickness with age. Finally, in men at least, the leg with foot reaches the best SA/V relation with the third age-group (16–20 years). In comparing the SA/V relations of the head with neck and the trunk with those of the whole upper and lower limbs, it appears that the latter are considerably worse off, through all ages (particularly prior to puberty), than the former. Only in the youngest age-group are the SA/V relations of the head with neck and of the trunk below the average SA/V relation for that age. Later on, it is only the trunk which is below the average SA/V relation for the whole body in the pertinent age groups.

DISCUSSION

The technique employed cannot give more than a very approximate picture of the partial volumes and surface areas of the human body which, naturally, is not a sum of simple geometrical but of very complicated, rather irregular, bodies. Thus the distal parts of the limbs, conceived as cylinders, do not show the expected SA/V relation which should be higher because of the larger surface area conditioned particularly by the fingers, less so by the toes. This is, apparently, due to a deficiency of the applied method; nevertheless, the proposed technique seems to furnish some valuable insights into known and—up to the present—less known or not satisfactorily explained processes and features of the human body.

The SA/V relation, one must not forget, gives an average picture only of the geometrical bodies to which the real human body parts were reduced, but nevertheless it shows considerable and plausible sex differences and age changes, though we know that they may not be true for each person measured. As to sex differences, in all body parts except perhaps the lower limbs, the SA/V relation suggests that heat radiation to the volume unit (one cubic decimeter, *i.e.*, roughly 1 kg.), seems to be at a higher level in adult females than in males. This is partly due to the shorter stature and over-all gracility of the female body. In the age group of 11–15 years, however, girls—who are during this period somewhat taller and bulkier—show a lower SA/V relation than boys. On the other hand, the thicker subcutaneous fat layer in women changes the situation substantially; actually, this insulating layer seems to prevent heat loss, in women, far more effectively than in men—otherwise women would not be such successful swimmers; and successful male swimmers would not have a thicker subcutaneous fat layer than average men, as have long-range swimmers and water-polo players.

If, as some authors (Rensch, cf. Coon, 1954, Coon, Garn and Birdsell, 1950, Coon, 1955) suggest, the desert form of man is particularly lean and slender with long limbs, that of women with large breasts and steatopygia, the same cooling effect is achieved by just these "appendages" which may considerably enlarge the surface area of the trunk without covering the whole body with a thicker subcutaneous fat layer. However, as anthropologists know, among Hottentot females there are also very fat individuals with the above-mentioned "appendages" well developed.

The sex differences and age changes stated in this study are in agreement —*mutatis mutandis*—also with Bergmann's and Allen's rules. They seem to encourage the investigation of various populations and races by these (or similar) techniques. It would be interesting to see how the SA/V relations, whole and partial, work out, for instance, with Eskimos and Nilotics, or Bushmen and Pygmies. Besides, they could be easily applied also to various "constitutional" types and body-form vectors (Škerlj, Brožek, Hunt, 1953).

Practical applications imply the study of heat loss and heat regulation, as well as of major injuries of the skin, such as burns and chilblains. Wallace (1951), one of the foremost specialists in treating burns, as well as Lund *et al.* (1944), gave approximate estimates of surface areas of human body parts. Wallace's "rule of nines" agrees, however, with our evaluations only regarding the head with neck, whereas his estimate for the trunk surface area is far too high, for the upper limbs much too low, and for the lower limbs slightly too low, particularly regarding females (Table 6). Though Lund's estimates are in better agreement with the results achieved in this study, they are still too high for the trunk, and too low for the upper limbs. However, a proper evaluation of partial surface areas, as well as of the injured areas in relation to the surface area of the pertinent body part, may have a considerable effect on the treatment (Škerlj and Kulčar, 1956).

TABLE 6. Comparison of Surface Areas in Percentages of the Whole SA

	Wallace	Lund *et al.*	Škerlj Men of college age	Škerlj Women of college age	Škerlj Men 21–55	Škerlj Women 21–50
Head and neck	9	9	9.7	9.1	9.1	9.5
Trunk	36 (+1 for the sexual region)	32	25.4	25.5	26.3	26.3
Upper limbs	18	19	24.8	22.6	22.5	23.0
Lower limbs	36	40	40.2	43.2	39.0	41.2
Sum	100	100	100.1	100.4	99.9	100.0

CONCLUSIONS

In reducing the human body to a sum of simple geometrical bodies it seems possible, more or less correctly, to evaluate its partial volumes and surface areas. The SA/V relations of the whole body as well as of its parts show considerable sex differences and age changes. Whereas the trunk volume, in both sexes and through all ages between 6 and 55 years, is about a half of the whole body volume, its surface area is only about a quarter of the whole body surface area. Being more slender than other body parts, upper limbs have relatively large surface areas. Age changes appear to be similar in both sexes. Children have a less favorable SA/V relation as a whole, and particularly of the limbs, whereas there is only a relatively small difference between the head with neck and trunk in the youngest age group of 6–10 years. Up to the twenties the whole body and all its parts gain in volume much more than in surface area, and therefore the SA/V relations decrease.

It would pay to extend these investigations from birth to great ages. Extrapolating the trends stated in this study, and considering the measurements of women by O'Brien and Shelton, by Škerlj, Brožek and Hunt, as well as by D. A. W. Edwards, one may expect:

1. A higher SA/V relation in younger children (from birth to five years of age), also in the body parts; a still lesser difference in the SA/V relation between head with neck and trunk.
2. A slow increase of the SA/V relation in older people, particularly in their extremities which lose subcutaneous fat and therefore must gain in relative surface areas.

These changing relations between volumes and surface areas explain, partly at least, why children and older people radiate more heat and have to wear warmer clothes than well-developed young adults.

Though the technique employed has certain disadvantages, it seems to afford some valuable results. Therefore it should be applied also in studies of various races and body types, as well as in treating skin injuries.

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CHANGES IN THE SKULL FEATURES OF THE JAPANESE PEOPLE FROM ANCIENT TO MODERN TIMES

Hisashi Suzuki

In Japan, excavations of human remains of prehistoric and protohistoric ages have long been carried out, and a fairly large number of human skeletons have been unearthed and studied. Until recently, however, human skeletons of medieval and modern times have not been excavated in as great abundance as have those of former periods. Fortunately, we have now a considerable number of them excavated in and around Tokyo City. We are now able to study a continuous series of human skeletons of all periods from the neolithic age down to the present day.

The skeletons of the neolithic ages are so different from those of the protohistoric period that there is some dispute as to whether or not they are directly related. The skeletons of the protohistoric age, however, are unanimously considered by Japanese anthropologists to be the direct ancestors of contemporary Japanese, although various skeletal characteristics show considerable differences. The difference between them is considered to have been brought about gradually with the change of time.

I want first to point out some of the major changes in various skull features of the Japanese people from the protohistoric age (4th to 8th C.) to the present day, and then to present a brief account of my opinions as to their cause.

The materials used in this study are male skulls of common people excavated in the Tokyo-Yokohama area. These are classified into five groups by periods.

Skulls of (1) The protohistoric Tomb Age (4th–8th C.).
(2) The Middle Medieval Age (14th C.).
(3) The Late Medieval Age (15th–16th C.).
(4) The Tokugawa Feudal or Yedo period (17th–19th C.).
(5) The Contemporary Period (20th C.).

CRANIUM

I want to mention first something about changes in cranial characteristics.

In the three main dimensions of the cranium, there has been a tendency toward decrease in the maximum head length, and an increase in maximum head breadth, but not much change in the basi-bregmatic height through these periods. The only exception is that the protohistoric skulls are not longer and narrower than those of the middle medieval period. The early medieval Japanese are the longest and the narrowest headed among the Japanese of all these periods. (Table 1.)

In indices, therefore, the protohistoric Japanese are mesocephalic (the index being near that of dolichocephaly), the middle medieval people are dolichocephalic, and the people in later periods have continuously become rounder

TABLE 1

		Protohist. times 4.–8.C.	Medieval times		Modern times	
			14.C. Kamakura Era	15.–16.C. Muromachi Era	17.–19.C. Tokugawa Era	20.C.
1 Maximum length	n Mean S.D.	48 182.9 6.0	170 184.2 7.7	61 183.6 5.2	165 181.9 6.5	143 178.9 6.6
8 Maximum breadth	n Mean S.D.	50 140.1 5.5	164 136.5 5.6	60 138.9 4.7	161 149.8 5.1	143 140.3 5.2
17 Basi-bregmatic height	n Mean S.D.	24 136.6 4.1	96 137.2 6.0	48 136.4 5.4	140 137.5 4.9	143 138.1 4.7
8:1 Length-breadth index	n Mean S.D.	46 76.5 3.9	164 74.2 4.1	60 75.9 3.3	160 76.9 3.9	143 78.5 3.9
17:1 Length-height index	n Mean S.D.	23 74.7 2.9	94 75.0 3.2	44 74.7 3.0	140 75.6 3.6	143 77.3 3.3
17:8 Breadth-height index	n Mean S.D.	20 97.7 4.0	93 99.8 5.3	44 97.8 4.1	138 98.6 3.9	143 98.6 4.2

headed. The contemporary Japanese are mesocephalic (rounder than the protohistoric Japanese). (Table 1.) According to the writer's study, some contemporary Japanese populations, for example the students of Tokyo University who came from all over Japan, are, on the average, round headed.

The length-height indices of both the protohistoric and the medieval skulls are about the same (orthocephalic), whereas modern Japanese are hypsicephalic. (Table 1.)

The breadth-height index of the skulls in all of the five periods is between metriocephalic and tapeinocephalic, except that in the middle medieval skulls acrocephalic forms prevailed. (Table 1.)

As I have pointed out, there are considerable differences in the shape of the skull, but there have been no essential changes in such measurements as the horizontal circumference of the cranium, the median sagittal arc, and the cranial module. One exception is the auriculo-bregmatic arc which in contemporary Japanese is much greater than in the preceding periods, especially the medieval, the reason being that the skulls of the earliest periods are narrower and somewhat lower than those of contemporary Japanese. (Table 2.)

The minimum frontal breadth does not show any marked difference in these five periods. (Table 3.)

TABLE 2

		Protohist. times 4.–8.C.	Medieval times		Modern times	
			14.C. Kamakura Era	15.–16.C. Muromachi Era	17.–19.C. Tokugawa Era	20.C.
23 Max. horiz. circumf.	n	42	151	45	156	142
	Mean	518.8	516.9	520.5	520.2	513.7
	S.D.	14.1	14.7	12.4	14.2	15.0
24 Auriculo-bregmat. arc	n	36	127	47	142	143
	Mean	312.9	309.4	308.9	313.1	324.4
	S.D.	10.2	11.0	9.3	10.3	10.7
25 Median sagittal arc	n	24	111	53	152	143
	Mean	373.1	373.7	371.5	373.6	371.7
	S.D.	9.4	14.8	13.4	11.9	11.6
1 + 8 + 17/3 Cranial module	n	24	88	47	137	143
	Mean	152.9	152.4	152.9	153.1	152.8
	S.D.	3.6	4.6	3.9	3.6	4.0

FACIAL SKULL

In the measurements of facial breadth—that is, in upper facial breadth, biorbital breadth, bizygomatic breadth and maxillary breadth—the protohistoric skulls have the greatest values and there is a tendency toward decrease with the change of time. One exception is that the bizygomatic breadth of the middle medieval skulls is narrower than that of the late medieval skulls, although it is broader than that of contemporary Japanese. (Table 3.)

In superior facial height, on the contrary, there is a tendency toward increase, the lowest being that of middle medieval skulls (Kamakuru era). (Table 3.)

Kollmann's superior facial index is the lowest in protohistoric Japanese, but increased in the later periods, and contemporary Japanese are medium faced in index. The story is the same in Virchow's superior facial index. (Table 3.)

ORBITA

In the orbita, the breadth does not show any significant differences, but in height there has been a change, an increase being registered with the change of time. (Table 4.)

Accordingly, the orbital index has also shown an increase, although this is by no means great, because the skulls in all of the five periods in question were of the "middle orbita" category. (Table 4.)

INTERORBITAL REGION

The protohistoric Japanese skulls have the greater anterior interorbital breadth. The breadth has gradually increased in later periods, and contemporary Japanese are the narrowest in measurement. The anterior interorbital arc, on the contrary, does not show any significant differences, except

TABLE 3

		Protohist. times 4.–8.C.	Medieval times		Modern times	
			14.C. Kamakura Era	15.–16.C. Muromachi Era	17.–19.C. Tokugawa Era	20.C.
9 Min. frontal breadth	n	42	150	63	161	143
	Mean	94.6	93.5	94.7	94.5	93.2
	S.D.	4.0	4.3	4.8	4.5	4.7
43 Sup. facial breadth	n	36	118	58	121	144
	Mean	106.3	105.5	104.9	104.8	103.8
	S.D.	4.8	3.7	4.1	3.6	4.1
44 Biorbital breadth	n	32	101	46	108	144
	Mean	100.5	100.0	99.4	98.8	97.2
	S.D.	4.2	3.8	3.9	3.4	3.8
45 Bizygomatic breadth	n	23	96	39	109	144
	Mean	138.3	134.8	136.0	135.4	132.9
	S.D.	5.2	6.5	4.6	4.9	.5.8
46 Maxillary breadth	n	25	107	45	107	143
	Mean	102.8	101.8	101.4	99.6	98.6
	S.D.	5.1	5.1	4.4	4.8	5.2
48 Sup. facial height	n	29	119	43	102	144
	Mean	68.3	64.7	68.5	69.3	70.7
	S.D.	3.6	4.4	4.2	3.7	4.2
48:45 Sup. facial index (Kollman)	n	24	87	33	95	144
	Mean	47.8	49.6	50.7	51.1	53.3
	S.D.	3.2	3.7	2.9	3.1	3.3
48:46 Sup. facial index (Virchow)	n	25	97	40	92	143
	Mean	64.8	65.6	67.6	69.7	71.8
	S.D.	4.8	5.4	4.2	5.1	4.6

that in the protohistoric, skulls differ significantly from the other four periods. (Table 4.)

The index between the two, therefore, shows a great tendency toward decrease, the difference in the index between contemporary and ancient Japanese being statistically significant. (Table 4.) This means that the nasal root of the ancient Japanese was broader and showed less protrusion than that of contemporary Japanese. The nose of the ancient Japanese must have had a markedly concave profile. This was already mentioned by me in 1952.

NASAL BONES

In the measurements of the nasal bones, the minimum breadth shows a tendency toward decrease, whereas maximum breadth does not show much difference in all of the five periods. (Table 4.)

Table 4

		Protohist. times 4.-8.C.	Medieval times		Modern times	
			14.C. Kamakura Era	15.-16.C. Muromachi Era	17.-19.C. Tokugawa Era	20.C.
51 Orbital width	n	26	111	46	117	142
	Mean	42.5	43.1	43.2	43.2	42.7
	S.D.	1.7	2.2	1.7	1.9	1.7
52 Orbital height	n	24	99	45	117	144
	Mean	33.8	33.7	34.2	34.4	34.3
	S.D.	1.8	2.3	2.1	1.9	1.8
52:52 Orbital index	n	17	96	45	117	142
	Mean	78.8	78.2	79.3	79.5	80.4
	S.D.	3.7	4.7	4.4	4.7	4.6
50 Ant. interorb. breadth	n	41	108	50	120	63*
	Mean	19.5	19.1	19.1	18.5	17.2
	S.D.	2.2	2.3	2.4	2.0	1.9
F Ant. interorbital arc	n	35	99	48	108	63*
	Mean	22.5	21.4	21.1	21.2	20.5
	S.D.	2.3	2.7	2.6	2.2	2.1
50:F Ant. interorbital Curvature	n	35	96	47	108	63*
	Mean	87.1	89.0	89.7	87.4	84.3
	S.D.	5.1	4.7	4.8	4.7	4.9
57 Least breadth of nasal bones	n	38	108	51	128	144
	Mean	8.4	8.0	7.8	7.6	7.0
	S.D.	2.0	2.3	2.1	1.7	2.0
57(1) Greatest breadth of nasal bones	n	18	87	44	105	142
	Mean	17.5	17.9	18.6	18.4	17.9
	S.D.	2.1	1.9	1.7	1.7	1.7
54 Nasal width	n	33	122	50	120	144
	Mean	26.9	26.6	26.1	26.2	25.0
	S.D.	2.2	2.2	2.0	2.0	1.9
55 Nasal height	n	31	121	47	120	143
	Mean	51.0	51.1	51.6	52.5	52.0
	S.D.	2.8	3.3	3.1	2.9	3.2
54:55 Nasal index	n	30	113	47	119	143
	Mean	52.9	52.1	51.3	49.9	48.4
	S.D.	4.5	4.8	4.1	4.3	4.3

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* These data were collected by me, whereas the rest in this column were collected by Professor S. Morita in 1950. My data were obtained from skulls of Japanese who have died since 1925.

THE SHAPE OF THE NOSE

The nasal breadth is the greatest in the protohistoric Japanese and has become narrower in later periods. The difference in this measurement even between the Tokugawa feudal or Yedo and the contemporary Japanese is statistically significant. (Table 4.)

The story is quite opposite in nasal height. In this measurement there has been an increase with the change of time, although not so marked an increase as the decrease in the former measurement. (Table 4.)

Therefore, considered from the standpoint of the nasal index, the nose was the broadest among the protohistoric Japanese; it became narrower in later periods, and is classified as a medium nose in contemporary Japanese. The difference in this index between contemporary Japanese and the preceding populations is also statistically significant. (Table 4.)

UPPER JAW

In the characteristics of the upper jaw, the protohistoric Japanese are not very different from the contemporary ones. The middle medieval Japanese had the greatest and longest upper jaws.

The shape of the upper jaws is brachuranic in all of the five periods, that of medieval Japanese being most dolichuranic, while in later periods it became more round. (Table 5.)

The greatest change in the upper jaw characteristics occurs in palatal height. The height is the lowest in the protohistoric Japanese and there has been a great increase in later periods, the difference even between Tokugawa or Yedo population and contemporary Japanese being statistically significant. (Table 5.)

ALVEOLAR PROGNATHISM

Another trait which distinguishes ancient Japanese skulls from contemporary ones is the strong alveolar prognathism of the former. Among the ancient people, prognathism of the middle medieval Japanese is the most pronounced (hyperprognathous). Contemporary Japanese are only slightly prognathous. (Table 5.)

SUMMARY AND OBSERVATIONS

Summarizing the above facts, the Japanese in ancient times had long heads, broad faces, a wide and flat nasal root and were strongly prognathous. The Japanese in later periods have become rounder headed, medium faced, narrow and high in the nasal root, and less prognathous. It seems that there is a correlation in the changes in these traits. Correlations of this kind were mentioned by Dr. G. M. Morant in his study of predynastic Badari skulls compared with the skulls in later periods found in Egypt, which conclusion was supported by Dr. F. Weidenreich.

As to the cause of the changes in the skull features of the Japanese population, most people would consider first, a possibility of race mixture.

As we all know, the Ainu who are dolichocephalic live near the Japanese main island of Honshu. This is the race which has quite frequently been referred to in connection with the origin of the Japanese people.

TABLE 5

		Protohist. times 4.–8.C.	Medieval times		Modern times	
			14.C. Kamakura Era	15.–16.C. Muromachi Era	17.–19.C. Tokugawa Era	20.C.
60 Maxillo-alveolar length	n	24	98	40	93	144
	Mean	52.8	55.7	53.3	53.5	52.4
	S.D.	2.8	3.4	3.0	3.0	3.6
61 Maxillo-alveolar breadth	n	25	100	38	89	144
	Mean	64.5	65.2	66.2	66.5	65.8
	S.D.	3.9	3.0	3.7	3.6	4.0
61:60 Maxillo-alveolar index	n	20	86	36	89	144
	Mean	123.4	121.8	123.7	125.0	126.0
	S.D.	6.1	7.4	6.7	7.7	8.2
64 Palatal height	n	21	86	40	71	109
	Mean	9.8	10.7	11.0	11.2	12.2
	S.D.	2.3	2.5	2.2	2.1	2.6
72 Facial profil angle to FH.	n	16	73	44	101	143
	Mean	83.3	81.7	82.3	83.2	85.1
	S.D.	2.7	4.3	3.3	3.7	3.4
73 Nasal profil angle	n	18	79	43	115	144
	Mean	90.2	88.6	89.4	89.7	88.5
	S.D.	3.2	4.2	3.7	3.3	3.7
74 Alveolar profil angle	n	15	68	44	98	143
	Mean	64.4	60.3	62.6	63.3	76.4
	S.D.	8.0	7.4	4.4	5.6	5.8

Our study has clearly shown, however, that there could have been no noticeable influence from the Ainu, because the two races are significantly different in many of the skull features. The Ainu have larger heads both longer and broader, and the interorbital region is narrower and higher than in the Japanese people. The Ainu also show more orthodontia than the psalidodontia of the Japanese.

The possibility of mixtures with other races is also very slight. Besides the fact that Japan is a country surrounded by the sea, and that recent findings of so-called palaeolithic implements from all over Japan have revealed the presence of a native population in Japan from the diluvial periods, there could not have been so many immigrants, at least after the 7th C., as to have caused a noticeable difference in the traits of the Japanese people (the population of which was already 6 million at the time), although there might have been some drifters or immigrants in the 7th C. from Korea. Such would have been even more the case after the 17th C. when Japan was isolated from other countries by her policy of seclusion during the Tokugawa era. Considering all of the possibilities and historical records, the skulls mentioned in this study are most likely to have been those of native Japanese in the Tokyo-Yokohama area.

The changes I have mentioned, then, in the features of the skull must be considered as a transformation brought about with the change of time.

The modification of human traits has already been suggested by Dr. E. Fischer in his study of brachycephaly of southern german people and also by Dr. F. Weidenreich in his article on brachycephalization. Both authors attributed the changes to change in culture or modes of living.

The fact that human physical traits, especially those of the head, could be considerably changed by environment has also been stated by Drs. F. Boas, W. Dornfeldt, H. Shapiro, R. Hauschild and others. Also Drs. H. Newmann, O. v. Verschuer and others, by studying differences between couples of twins, have shown that, although the horizontal circumference of the head is not influenced easily by environment, the shape of the head is subjected to considerable differences by changes in environment. Our study of more than 400 Japanese twins also confirmed this fact.

Human traits, therefore, are influenced very much by environmental changes as well as by heredity.

The change in the skull features of the Japanese people, if we are able to exclude the possibility of race mixture, is most likely caused by progress in the mode of living of the Japanese people from ancient times to the present day, especially after the end of the 19th C. when the living standards of the Japanese people were greatly raised with the influence of European civilization.

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PRIMATE EVOLUTION AND HUMAN BEHAVIOR

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The central interest of physical anthropology is the study of human evolution. Many physical anthropologists have tended to concentrate upon the biological phases of this problem, avoiding the implications of human evolutionary studies for understanding human behavior. Or, worse yet, some of them have offered theories of human behavior with a spurious biological bias, ignoring the findings of psychologists, sociologists and other anthropologists. This article attempts to close some of the gap between physical and cultural anthropology, giving an interpretation of the evolution of the primates and of man, and applying this information to some problems of human behavior.

Living primates give valuable information about the evolution of the order. Prosimians show the major primate adaptation for grasping, the tendency for eyesight to be very important, and the tree-living forest habitat so frequently observed. It is likely that the importance of vision is related to living in trees, since this three-dimensional world is not as well suited to use of the sense of smell as is the ground.

Both New World and Old World monkeys show a major adaptive advance with stereoscopic color vision. All but one group are diurnal, undoubtedly related to increased visual efficiency (Willmer, 1954). The evolution of larger and more highly developed brains is also probably related to the greater importance of vision (Elliot Smith, 1927). There is little doubt that Old World monkeys are more closely related to man than their New World counterparts, although the degree of their relationship to each other is still obscure (Simpson, 1945).

The anthropoid apes have the basic grasping adaptation, but the mode of progression is substantially altered toward an upright position. The adoption of the upright posture was probably associated with the habit of brachiation (Washburn, 1951). Major changes in the shoulder girdle give the apes a much greater freedom of movement for the arms than monkeys, which primarily move the front limbs in the two-dimensional plane of other quadrupeds and show similar form and orientation of the scapula, clavicle and humerus (Inman, Saunders and Abbott, 1944). In the head, however, ape organization is much like that of monkeys, with the same importance of vision. Also, the relative intelligence of the most thoroughly tested ape and monkey, the chimpanzee and rhesus monkey, is disputed enough by psychologists to indicate that the differences tend to balance each other and may not be very great.

Keith (1923) and Elliot Smith (1927) developed the theory of human upright bipedal adaptation based upon the major advances in grasping, vision and brachiation shown in living primates. There is some dispute as to whether brachiation was a precursor to human bipedal locomotion (Straus, 1949; Le Gros Clark and Leakey, 1951), even though the evidence tends to favor the anthropoid apes as closer relatives of man than the monkeys (Schultz, 1936; Zuckerman, 1933; Mourant, 1954).

It is the belief of the author that the extent to which living primates represent evolutionary stages ancestral to man can be at least partly evaluated, and that this information can be of great value to behavioral sciences. To make this evaluation, a few basic ideas from evolutionary theory require attention, along with some knowledge of the fossil record of the order.

Evolution may be defined as a change in gene frequency in a population. Many factors may influence stability or change within populations, but natural selection is by far the most important. The key to natural selection is adaptation. Simpson's (1944) concept of "adaptive zone" is very useful for understanding the actual evolutionary history of animal groups in terms of their fundamental ecology. Since many different factors enter into this adjustment to the environment, a single species may be described as being in several adaptive zones. To illustrate this, an interpretation of the adaptive zones of a small prosimian primate, the tarsier, is given.

This animal has the basic primate grasping adaptation, which has been particularly valuable in the trees. Enormously enlarged eyes are an adaptation for night vision. The teeth are adapted for killing and consuming smaller animals, a predatory adaptive zone. The greatly elongated tarsal region gives the species its name. The leverage from this allows the animal to make prodigious hops from a stationary position. It is useful in progressing through their arboreal adaptive zone and for capturing prey, their predatory adaptation.

Adaptation is the crucial element in natural selection; it is constantly operating on different groups of animals in relationship to each other, and between animals of the same species. Within a single adaptive zone groups of animals may remain much the same for tremendous periods of time or may change in different ways, as Simpson (1944) has shown from the paleontological record. He proposes three models of evolutionary change that are very useful in bringing order out of the chaotic picture paleontology frequently presents.

Simpson's evolutionary modes are:

(1) speciation—descendants of an original group spread out into various sub-zones, using either different territories or different ecological aspects of the same territory;
(2) phyletic evolution—an ongoing change within an adaptive zone, so that descendant populations have progressively differed from their ancestors as they improved their adaptation;
(3) quantum evolution—a change from one adaptive zone to another by an evolving population.

This article is primarily concerned with the latter two modes. Phyletic evolution is a non-teleological approach to the apparently directed changes within vertebrate groups frequently observed in the paleontological record, the delight of orthogenically inclined investigators. Quantum evolution may be brought about through changes in the environment, by expansion of a population beyond an environment transitional to its old habitat, or by genetic changes which then allow subsequent exploitation of new adaptive zones. In all evolutionary modes genetic modifications within the population are extensive (Wright, 1949) until stabilization within a new adaptive zone is attained.

The fossil record of primates is spotty but allows some reconstruction of their history in terms of these evolutionary modes. Primates are among the oldest of surviving orders of mammals, being relatively abundant in the Paleocene and Eocene. They were all small animals confined to forested or swampy country. Barth (1950) has called them the rats and mice of their day. Some of them, indeed, had gnawing incisor teeth similar to present-day rodents, but others showed unmistakable ties to modern prosimians (Simpson, 1940). They may not have been much different in adaptive zone, though smaller in brain and perhaps not primarily nocturnal.

These early primates disappear from the fossil record by the end of the Eocene or shortly afterward. It is likely that many were replaced in the struggle for survival by the earliest rodents (Barth, 1950) and by their own descendants, the first of the higher primates (Patterson, 1954). The fossil record on early monkeys and apes is very scanty. The story of their evolution and the rise of human ancestors from among them is the most significant existing gap in the paleontology of the primates.

The fabulous discoveries of fossil man-apes in the last few years give fascinating information about what the immediate ancestors of primitive men must have been like. These animals stood fully upright, did not use their teeth as weapons—implying that the arms and hands unencumbered by locomotor functions characteristically used implements—and probably were hunters and gatherers (Bartholomew and Birdsell, 1953). There is no good evidence that they manufactured tools or used fire.

The evidence of the fossil man-apes gave the strongest verification of Weidenreich's (1949) thesis that the erect, bipedal posture was attained before the great expansion of the brain in human evolution (Washburn and Patterson, 1951). They also provide the most important addition to the sequence of primate forms that fits in with the major evolutionary advances: prosimian to monkey to ape to man-ape to man. These all conform to major evolutionary quanta; the fossil record supports these sequences wherever it is complete enough to give a clear picture. Except for the man-apes, representatives of each of these stages remain today, valuable sources for study. This does not imply that these groups have not continued to change genetically through time, but that the changes have probably not been nearly as extensive as those resulting from major shifts in adaptive zone.

Washburn (1951) has developed the anatomical theory of these evolutionary quanta as far as the appearance of the man-apes. Our concern here is to account for the evolution of man himself in terms of adaptation and evolutionary modes. Man is in the upright, tool-using adaptive zone, but more important, his basic adaptation is that of culture, in which learned behavior through the symbolic medium of language is of primary importance. The intelligence required to master this adaptation gives individuals and societies the ability to use natural forces, predict the consequences of actions, and make decisions, all to an extent unprecedented in the animal kingdom (Linton, 1936; Kroeber, 1948; Spiro, 1954). Human evolution in the Pleistocene is best explained as a progressive genetic change in adapting to this new cultural way of life. In fact, no other non-teleological explanation has been offered, even though the theory has had most of its explicit development in the past ten years (Dobzhansky and Montagu, 1947; Tappen, 1953; Etkin, 1954). The quantum evolutionary shift into this new cultural adaptive zone would undoubtedly be made easier by the tool-using adaptation, the greater cortical control which probably resulted, and by the loss of emphasis upon inherited behavior shown increasingly by higher mammals and particularly by monkeys and apes (Beach, 1947;

Nissen 1954). In spite of this pre-adaptation, the new adaptive zone was absolutely unprecedented.

Once the shift into the cultural adaptive zone was made, there must have been strong selective pressures favoring more intelligent individuals, and hominid groups better adapted to culture must have spread at the expense of the more backward societies. The fossil record of the Pleistocene indicates, by any paleontological standards, an extremely rapid increase in cranial capacity to two or more times that of the man-apes. Tool-making traditions develop during the same general period. These facts are interpreted as evidence of improving adaptation to culture; if so, it provides one of the clearest examples of a phyletic evolution, and the most rapid one known to this author. The genetic changes were probably extensive; those influencing neuro-behavioral readjustments were probably particularly great, because the new adaptation was essentially a neurological one. The crude measurement of increased brain size is very likely only a small indication of these changes.

When a new adaptive zone is opened up, the usual consequence is a rapid dispersal of descendant forms into various sub-zones (Wright, 1949), Simpson's speciation mode of evolution. Man at present constitutes but a single species by genetic criteria (Dobzhansky, 1944), and it appears probable that this was the condition throughout the Pleistocene (Weidenreich, 1949). This seems to be a peculiarity resulting from the cultural adaptive zone. The wide geographical adaptability prevented any long-term breeding isolation in large regions of the Old World, and the new cultural basis of sexual behavior itself prevented other isolating mechanisms from taking place. As a result, the continuous interbreeding that roughly defines a species was maintained.

The conception of a cultural way of life as an adaptive zone may help give a sense of unity to the tremendous diversity that characterizes human societies and human individuals. The society and the individual both depend upon symbolic learned behavior to survive and reproduce. As Spiro (1954) has pointed out, it becomes impossible to separate culture from "human nature," and the individual without his culture cannot survive by his own efforts. It is the human way of making a living, and the emphasis upon learning requires the individual to grow up within a society to obtain the techniques of survival. This requires him to be capable of learning these and many other things, and to be able to interpret complex situations in terms of what he has learned. Selection has not only tended to develop a population consisting of such individuals, but also tends to maintain this level of ability (Dobzhansky and Allen, 1956).

Human groups need more than control over natural forces to survive. The cohesiveness of a particular culture is dependent upon the maintenance of common values and beliefs, which themselves are culturally defined and must be learned by the individual. These mainsprings of the culture may be so contradictory that he cannot reconcile them, or his culturally influenced goals may be too difficult to attain and result in serious psychic impairment. Various individual pathologies, such as neuroses, psychoses, homosexuality, stuttering and psychosomatic disorders are probably a result of some interference with the symbolic learning processes; conflicting cultural stimuli and, as an increasing amount of evidence indicates (McGeer, McGeer and Boulding, 1956), organic causes. Because of their relationship to the symbolic adaptation of man, attempts to find their equivalents in sub-human primates or to produce them experimentally in these forms may be of dubious value. It is the belief of the author that the last four conditions mentioned above are peculiarly human.

Within normal ranges of human behavior, differences in personality are probably substantially related to adaptation of individuals to their culture. This is probably effected by genetic differences, in addition to the well documented environmental ones.

There remains to be discussed the future application of studies of primate evolution and of living primates to problems of human behavior. A major task of physical anthropology is a continuing analysis of the relationship of non-human primates to man and to each other in the light of new techniques which give clues to genetic similarities and differences. Evolutionary theory and a large body of specific primates studies give first approximations to this analysis. With each shift in adaptive zone, there probably results an extensive modification of interacting systems of genes in response to selection for characteristics which were previously non-existent, of less importance, or even detrimental. These changes can be expected to be much greater than those in which the descendant forms retain the same general adaptation as their ancestors. The larger the number of quantum shifts in adaptive zone, the greater the genetic difference from the ancestral group. From the analysis of the fossil record and the structure of living primates it seems probable that modern prosimians are most similar to the earliest primate ancestors, and that the anthropoid apes are genetically closest to man. Likewise, modern apes are probably quite similar in many more respects to human ancestors which were of a similar evolutionary development. These conclusions are of considerable practical importance, since comparative biology and psychology need this kind of information in applying the findings of experiments to man; frequently it is needed more than experimenters in these fields realize. As more is learned, there is a good possibility that genetic similarities and differences between primates and man can be measured with great precision.

The assessment also applies in the use of experimental animals from other orders, where the genetic differences are inevitably even greater. For example, many of the neurological processes rat psychologists are inferring are doubtless applicable to all mammals, but it is frequently very difficult to tell which ones apply to human learning. When Seward (1948) seeks to demonstrate that rats are capable of symbolizing, the genetic gap between these animals and man makes the comparability of the phenomena he describes very difficult to evaluate. On the other hand, the observations of Beach (1947) and Nissen (1954) on sexual behavior in various animals indicate that this basic function becomes increasingly dependent on learning rather than instinct in monkeys and particularly in chimpanzees. This not only is tied in with the probable degree of relationship to man; it is important evidence that the primate ancestors of man were developing learned behavior as a characteristic adaptation. This made a good foundation for the giant stride into the cultural adaptive zone made by the earliest men.

This does not mean that apes are men. The consequences of the cultural, symbolic adaptive zone are of enormous magnitude. The quantum evolution to the upright bipedal posture and to the cultural adaptive zone both intervene between the two forms, and the phyletic evolution of the Pleistocene which adjusted more sharply to the symbolic adaptation probably increased the genetic gap greatly. Many of the consequences of this adaptive zone must be either entirely missing or only dimly foretold in other animals; its peculiarities are many and varied and frequently do not offer much hope of enlightenment through animal studies. But animals have given a great deal of information about fundamental biology in man, and will give more in the future. In behavioral sciences they are giving valuable information by learning complex problems, and should eventually give many clues to the general nature of learning. Primates particularly should give key data about the beginnings of symbolic processes through the study of the anatomy and physiology of nervous control of the body. While the work of primate psychologists will certainly have limitations because of the evolutionary peculiarities of man, biological and cultural anthropologists cannot afford to ignore their findings.

Perhaps the most important use of primates may eventually be their application to problems of human genetics (Hooton, 1954). When this science is sufficiently developed, the genetic bases of human behavior and human evolution itself will at least in part be under human control.

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LES PROPORTIONS DE LA TÊTE CHEZ LES FRANÇAIS

Pierre A. Vassal

L'étude des proportions de la tête a figuré au centre des préoccupations des artistes et des morphologistes longtemps avant de susciter les travaux des anthropologues.

L'idée d'une certaine harmonie céphalique s'est trouvée ainsi à la base de la plupart des procédés d'étude de cette région.

En anthropologie, la notion d'harmonicité est capitale. Elle justifie la distinction commune entre crânes harmoniques et dysharmoniques, suivant que l'allongement ou l'élargissement du calvarium se prolonge au niveau de la face ou au contraire l'épargne. Elle se retrouve encore à l'intérieur des limites de la face. On sait que les visages minces et allongés ont généralement des orbites hautes et des nez étroits, comme c'est le cas dans la race méditerranéenne. Inversement, une face large possède souvent des orbites basses et un nez élargi (race alpine).

Des relations ont même été établies entre la forme de la tête et certains caractères somatiques. Nous citerons ici entre autres Collignon (1883), pour qui la brachycéphalie était plus développée chez les sujets de grande taille, Manouvrier (1896), Papillault (1902), Pittard (1905), Boas (1913) qui ont été d'un avis opposé. Tchestiakoff (1926) de son côté, ne trouve aucune corrélation entre la stature et l'indice céphalique. De même Collignon (1883) pensait que la leptorhinie était plus fréquente chez les brachycéphales et Pittard (1911) trouvait qu'elle s'accentuait avec la taille.

Tous ces travaux se basent le plus souvent sur des observations exactes. Beaucoup sont le fruit d'une longue expérience qui a amené les chercheurs à dégager une relation par une sorte de surimpression inconsciente de leurs observations. Ailleurs, elles sont dues à la remarque de liaisons qui étaient peut-être fortuites. Les "lois" qui en résultent, viennent alors de l'étude de sujets extrêmes ou pathologiques, en tout cas fort rares. Cette expérimentation que nous fournit la nature est précieuse. Cependant, il est permis de se demander si elle possède une portée générale.

Il paraissait intéressant de rechercher si les relations mises en évidence par différents auteurs étaient le fait de groupes plus ou moins spéciaux, ou si l'examen d'une série de sujets "moyens" les confirmait.

L'étude d'un grand nombre de mesures anthropométriques que j'ai pu effectuer sous la direction du Professeur Vallois sur un groupe important de sujets semblait pouvoir donner une réponse à cette question.

J'envisage donc ici 16 mesures de l'extrémité céphaliques:

— le périmètre de la tête
— la longueur maxima de la tête
— cinq mesures verticales: la hauteur de la face morphologique, la hauteur de la face supérieure, la hauteur du nez, la hauteur de la bouche, la longueur de l'oreille

— neuf diamètres transverses: la largeur maxima de la tête, la largeur frontale minima, la largeur bizygomatique, le diamètre bigoniaque, les diamètres bipalpébraux interne et externe, les largeurs du nez, de la bouche et de l'oreille

une mesure intéressant la tête et le cou: la hauteur cervico-céphalique, ou distance vertex-point suprasternal.

De plus, j'ai considéré des caractères anthropométriques extérieurs à la tête ou ne l'intéressant pas directement et susceptibles de traduire certains facteurs du développement.

— le poids, mesure à trois dimensions
— la stature et la longueur du membre supérieur, pouvant représenter les mesures à tendance unidimensionnelle
— le périmètre du cou

Une telle étude statistique demande certaines précautions. Manouvrier (1896) insistait déjà sur la nécessité de travailler sur des séries homogènes si l'on veut mettre en évidence des différences qui aient un sens. Il faut en effet éliminer les corrélations "parasites" et, pour ce, réduire au maximum le nombre des facteurs susceptibles d'influer sur les caractères étudiés.

Notre série comprend 372 jeunes adultes du sexe masculin âgés de 19 ans 6 mois à 22 ans 6 mois (âge moyen: 20 ans 10 mois). Il s'agit de français d'ascendance métropolitaine vérifiée (parents et grands parents) à l'exclusion des sujets d'origine étrangère ou extra-européenne. Enfin tous ces hommes étaient des soldats qui avaient été reconnus bons au service armé.

Cette série est donc très homogène quant à son origine et quant à la tranche d'âge qu'elle intéresse. La corrélation âge/poids est ici nulle. On peut donc considérer qu'il a été travaillé à âge constant.

Les sujets ont été examinés dans des conditions rigoureusement identiques, dans le même local et au même moment de la journée par le même observateur. La méthode d'examen anthropométrique était celle du Laboratoire d'Anthropologie de l'École Pratique des Hautes Études (Professeur Henri V. Vallois).

Nous allons étudier successivement les principaux résultats des intercorrélations de Bravais-Pearson entre les différents caractères ainsi qu'entre les caractères et leurs indices. Nous aurons également à envisager les données de la décomposition en facteurs suivant une méthode voisine de méthode centroïde, puis suivant la méthode de Hotelling. Nous renvoyons pour les détails aux diverses publications en cours à ce sujet. (Comptes-Rendus Association des Anatomistes, Paris 1955, Société d'Anthropologie de Paris 1956).

Les intercorrélations entre les caractères peuvent être fortes: c'est le cas de celle du poids et du périmètre du cou, r: .74; elles peuvent être de valeur moyenne, faible ou nulle: c'est ainsi que sur les 15 corrélations que donne la hauteur de la bouche avec les autres mesures céphaliques, 13 sont nulles et 2 ne sont légèrement positives qu'avec des mesures qui la recouvrent: la hauteur de la face morphologique et la hauteur de la face supérieure. Dans le même ordre d'idées, nous signalerons les corrélations élevées qui unissent les hauteurs de la face morphologique, de la face supérieure et du nez qui ont un segment commun très important. Notons enfin que les corrélations sont significatives à partir de r: .12 au seuil de P: .02.

Si l'on fait la somme des corrélations données par chaque caractère avec toutes les autres mesures de la tête étudiées ici en utilisant une méthode voisine de la méthode centroïde, on voit que les corrélations *moyennes* données par chaque caractère sont de valeur très différente. Par ordre décroissant, on trouve:

périmètre céphalique avec une moyenne de	.48
bizygomatique	.45
poids	.44
bipalpébral externe	.40
hauteur de la face morphologique	.38
périmètre du cou	.38
taille	.38
hauteur de la face supérieure	.37
largeur frontale minima	.34
longueur maxima de la tête	.34
longueur du membre supérieur	.34
hauteur cervico-céphalique	.31
hauteur du nez	.30
largeur maxima de la tête	.30
largeur bigoniaque	.26
largeur bipalpébral interne	.25
largeur de la bouche	.25
longueur de l'oreille	.24
largeur de l'oreille	.20
largeur du nez	.20
hauteur de la bouche	.05

On peut ainsi distinguer plusieurs groupes de caractères:

1) les mesures qui donnent en moyenne des liaisons élevées, supérieures à .40. Ce sont: le périmètre céphalique, la largeur bizygomatique, le poids et la largeur bipalpébrale externe, c'est à dire une mesure tridimensionnelle (le poids), un périmètre et deux diamètres transverses de la face très voisins l'un de l'autre.

2) les caractères qui donnent des corrélations de valeur moyenne comprises entre .30 et .40. Ce groupe renferme en particulier les hauteurs de la face morphologique et supérieure, la stature et la longueur du membre supérieur, la hauteur du nez, la hauteur cervico-céphalique, la longueur et la largeur maxima de la tête.

3) les caractères qui donnent en général des corrélations faibles allant de .20 à .30. On note ici la présence du bipalpébral interne, de la largeur du nez et de la largeur de la bouche ainsi que des mesures, largeur et longueur, de l'oreille. Il est à remarquer que le diamètre bipalpébral interne est ainsi tout à fait séparé du diamètre bipalpébral externe et que la largeur bigoniaque s'isole également du groupes des autres diamètres transverses.

4) Nous avons vu plus haut que la hauteur de la bouche ne donnait que des corrélations nulles sauf avec certaines mesures qui la recouvrent en totalité ou en partie.

Parmi les caractères extérieurs à l'extrémité céphalique, les corrélations les plus élevées avec les mesures de la tête sont données par ordre décroissant par: 1) le poids, 2) le périmètre du cou, 3) la stature, 4) la longueur du membre supérieur, 5) la hauteur cervico-céphalique.

Le poids donne les corrélations suivantes avec les 16 mesures céphaliques étudiées:

r poids/périmètre du cou	.74
r poids/bizygomatique	.53
r poids/périmètre de la tête	.52
r poids/bipalpébral externe	.45
r poids/bigoniaque	.41
r poids/longueur maxima de la tête	.37
r poids/frontal minimum	.36
r poids/hauteur de la face supérieure	.36
r poids/largeur maxima de la tête	.32
r poids/longueur de l'oreille	.32
r poids/hauteur de la face supérieure	.28
r poids/hauteur du nez	.24

On note que les corrélations les plus fortes sont celles des périmètres et des diamètres transverses. *Les liaisons avec les hauteurs viennent en dernier.*

La taille donne avec les mesures céphaliques les corrélations suivantes:

r taille/périmètre céphalique	.39
r taille/hauteur de la face morphologique	.35
r taille/largeur bipalpébrale externe	.34
r taille/largeur bizygomatique	.32
r taille/périmètre du cou	.32
r taille/longueur maxima de la tête	.31
r taille/hauteur de la face supérieure	.29
r taille/hauteur de l'oreille	.27
r taille/hauteur du nez	.24
r taille/largeur maxima de la tête	.23
r taille/frontal minimum	.23
r taille/largeur de l'oreille	.20
r taille/largeur bigoniaque	.19
r taille/largeur de la bouche	.17
r taille/bipalpébral interne	.16

Les corrélations avec les périmètres ici aussi figurent parmi les plus fortes mais les corrélations entre la taille et les hauteurs sont relativement importantes par opposition à ce qui se passait avec le poids.

L'opposition entre les diamètres transverses et les mesures verticales ressort encore du tableau suivant des intercorrélations de ces différents caractères:

	Largeur maxima de la tête	Bipalpébral externe	Frontal minimum	Bizygomatique	Bigoniaque	F.M.	F.S.	Hauteur du nez
Périmètre céphalique	.57	.50	.42	.52	.28	.38	.31	.21
Largeur maxima de la tête		.31	.41	.61	.32	.23	.20	.18
Bipalpébral externe			.53	.52	.13	.29	.26	.21
Frontal minimum				.53	.17	.27	.22	.25
Bizygomatique					.49	.34	.27	.23
Bigoniaque						.20	.19	.13
Hauteur de la face morphologique							.81	.58
Hauteur de la face supérieure								.78

On note les corrélations élevées données entre elles par les diamètres transverses: la largeur maxima de la tête, le frontal minimum, le bipalpébral externe et le bizygomatique. Là encore, le bigoniaque prend une place à part, en donnant avec ces mesures des corrélations notablement plus faibles.

Les hauteurs de la face et du nez donnent entre elles, nous l'avons vu, des corrélations élevées du fait que ces mesures se recouvrent. Avec les diamètres transverses, elles donnent des corrélations relativement faibles, toutes inférieures à celles du bigoniaque avec ces mêmes valeurs. Le diamètre bigoniaque se trouve ici faire la transition entre les mesures verticales et les diamètres d'épaisseur.

Les corrélations de Bravais-Pearson entre les caractères et les indices sont en général très faibles ou nulles. Mais elles deviennent fortes entre les indices et les caractères qui les constituent. La corrélation est alors négative avec le dénominateur et positive avec le numérateur.

Deux éventualités sont possibles suivant que les corrélations sont en valeur absolue très différentes ou très voisines. Dans le premier cas, l'indice est lié de façon préférentielle à l'un des ses constituants. Quand la différence est très forte, l'indice ne renseigne guère en réalité que sur le caractère avec lequel il est le plus lié.

On a:	r indice auriculaire/hauteur de l'oreille	.62
	r indice auriculaire/largeur de l'oreille	.38
	r indice buccal/hauteur de la bouche	.93
	r indice buccal/largeur de la bouche	−.29

Il est clair que dans ce cas l'indice renseigne surtout sur la grande dimension à laquelle il est lié de façon préférentielle:

Ailleurs, la différence entre les corrélations est plus faible en valeur absolue.

r indice facial morphologique/hauteur de la face morphologique	.72
r indice facial morphologique/bizygomatique	.−42
r indice facial supérieur/hauteur de la face supérieure	.77
r indice facial supérieur/bizygomatique	.−41

Dans d'autres cas enfin, l'indice est lié à chacun de ses constituants par des corrélations égales ou presque en valeur absolue et de valeur élevée.

r indice céphalique/largeur maxima de la tête	.68
r indice céphalique/longueur maxima de la tête	.−62
r indice transverso-zygomatique/bizygomatique	.42
r indice transverso-zygomatique/largeur maxima de la tête	.−47
r indice nasal/largeur du nez	.67
r indice nasal/hauteur du nez	.−69

On voit ici que les indices céphalique et nasal dont les corrélations avec leurs constituants sont fortes et de valeur voisine échappent à l'objection qui peut être formulée à l'encontre des indices auriculaire et buccal. L'"indépendance" de l'indice sera réalisée toutes les fois que les caractères qui le composent ont des variabilités très voisines et ne seront pas liés l'un à l'autre par une corrélation élevée.

Variabilité de la largeur maxima de la tête	3,73
Variabilité de la longueur maxima de la tête	3,47
r largeur maxima/longueur maxima de la tête	.15
Variabilité du bizygomatique	3,63
Variabilité de la largeur maxima de la tête	3,73
r bizygomatique/largeur maxima de la tête	.61
Variabilité de la largeur du nez	6,84
Variabilité de la hauteur du nez	6,94
r largeur/hauteur du nez	.08

Les indices *céphalique* et *nasal* ainsi qu'à un degré moindre l'indice transverso-zygomatique paraissent donc offrir toute garantie sous ce rapport.

On a au contraire:

pour l'indice auriculaire

Variabilité de la largeur de l'oreille	7,20
Variabilité de la hauteur de l'oreille	6,11
r largeur/hauteur de l'oreille	.48

pour l'indice buccal

Variabilité de la hauteur de la bouche	17,08
Variabilité de la largeur de la bouche	6,55
r hauteur/largeur de la bouche	.08

pour l'indice facial morphologique

Variabilité de la hauteur de la face morphologique	4,74
Variabilité du bizygomatique	3,63
r hauteur de la face morphologique/bizygomatique	.34

pour l'indice facial supérieur

Variabilité de la hauteur de la face supérieure	5,15
Variabilité du bizygomatique	3,63
r hauteur de la face supérieure/bizygomatique	.27

On voit de suite que l'indice auriculaire et l'indice buccal n'offrent pas ici les garanties que donnent les indices céphalique et nasal. Les indices faciaux occupent *dans notre série* une place intermédiaire.

Sous ce rapport, on note que si l'on remplaçait *ici* dans les indices faciaux la largeur bizygomatique par le frontal minimum ou par le bigoniaque, on serait plus près des conditions idéales de l'indice, car les variabilités de ces diamètres transverses sont plus proches de celles des hauteurs de la face que ne l'est celle du bizygomatique:

Variabilité de la hauteur de la face supérieure	5,15
Variabilité de la hauteur de la face morphologique	4,74
Variabilité du diamètre bigoniaque	4,84
Variabilité du diamètre frontal minimum	4,34
Variabilité du diamètre bizygomatique	3,63

De plus, les hauteurs de la face donnent avec le frontal minimum et le bigoniaque des corrélations qui sont plus faibles en caractères normaux *dans cette série* que celles avec le bizygomatique:

r hauteur de la face morphologique/bizygomatique	.34
r hauteur de la face morphologique/frontal minimum	.27
r hauteur de la face morphologique/bigoniaque	.20
r hauteur de la face supérieure/bizygomatique	.27
r hauteur de la face supérieure/frontal minimum	.22
r hauteur de la face supérieure/bigoniaque	.13

Signalons enfin que l'indice fronto-mandibulaire donne ici les garanties désirées.

Variabilité du frontal minimum	4,34
Variabilité du bigoniaque	4,84
r frontal minimum/bigoniaque	.17

Il en est de même, bien qu'à un degré moindre, en ce qui concerne l'indice naso-buccal:

Variabilité de la largeur du nez	6,84
Variabilité de la largeur de la bouche	6,55
r largeur du nez/largeur de la bouche	.42

L'analyse factorielle de Hotelling a été pratiquée pour 10 des 16 caractères anthropométriques de la tête: la longueur et la largeur maxima de la tête, les diamètres frontal minimum, bipalpébral externe, bizygomatique, bigoniaque, la hauteur de la face morphologique, la hauteur du nez, la largeur du nez et de la bouche, la longueur et la largeur de l'oreille.

La grande complexité de l'organisation de la tête apparaît nettement. En effet, le facteur général de croissance n'explique ici que 30% de la variance totale et les 5 premiers facteurs n'épuisent que 71% de cette même variance.

Le facteur général fait ressortir les diamètres bizygomatique et bipalpébral externe, dont nous avons vu plus haut la corrélation élevée (r: .52). Le facteur no. 2 extériorise la hauteur et la largeur de l'oreille (r: .48), le facteur no. 3 les largeurs du nez et de la bouche (r: .42), les facteurs no. 4 et 5 le diamètre bigoniaque.

Certains caractères apparaissent en outre comme liés de façon très intime et sortent avec des chiffres voisins dans les 5 facteurs. Tels sont: la hauteur et la largeur de l'oreille déjà envisagées, la hauteur de la face morphologique et la hauteur du nez dont on sait par ailleurs la corrélation élevée (r: .58) et enfin d'une façon un peu plus lâche le bizygomatique et la largeur maxima de la tête (r: .61). La décomposition en facteurs suivant la méthode de Hotelling nous conduit donc à des résultats assez voisins de ceux auxquels conduisaient les intercorrélations de Bravais-Pearson. Notons au passage la place à part occupée dans les deux cas par le diamètre bigoniaque. Cette mesure ne représente pas en vérité une largeur. De fait, l'écartement des branches de la mandibule n'est que la résultante de deux forces, qui tendent, l'une à croître dans le sens latéral vers l'extérieur, l'autre à progresser dans le sens sagittal en profondeur.

CONCLUSION

L'analyse des intercorrélations de Bravais-Pearson effectuée sur 16 mesures céphaliques et en prenant des caractères de référence extérieurs à la tête donne des résultats que vient confirmer l'analyse factorielle. Malgré l'extrême complexité de l'organisation céphalique, on peut séparer les mesures en hauteur des diamètres transverses et de certaines mesures comme les périmètres céphalique et du cou, le bizygomatique, le bipalpébral externe qui semblent refléter un facteur de croissance générale qui apparaît nettement dans le poids. Les mesures verticales constituent le pôle opposé en se rapprochant de la stature et des caractères unidimensionnels.

On peut concevoir en gros les différents segments de la tête sous la forme d'ellipses, dont le petit axe, diamètre transverse, est plus dépendant du facteur de croissance générale que le grand axe qui tend vers l'unidimensionnalité, et donne de ce fait des liaisons plus étroites avec des caractères comme la longueur des membres. Les grands axes donnent leurs corrélations élevées avec les caractères linéaires dès qu'ils dépassent les 150% du petit axe (exemples: le nez, l'oreille). De toute façon, le contraste entre des caractères à tendance linéaire et des mesures à tendance pluridimensionnelle éveille l'idée d'un certain anisotropisme de l'extrémité céphalique.

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A STUDY OF THE RACIAL MORPHOLOGY OF THE FRENCH POPULATION

M. Verdun, J. de Taille, R. Bourdiol, and J. Poggi

By the use of a uniform statistical methodology the authors have examined a sample of 300 boys aged 7 to 17 years and 984 adult males aged 18 to 52. The sample is representative of the French male population in geographical and sociological categories.

From the sample were selected those individuals whose morphology approximated most closely one or another of the four racial types resident in France (Nordic, Mediterranean, Alpine, and Lorraine, as elsewhere described by the authors). These individuals were selected on the basis of the following seven criteria of racial morphology: color of hair, color of iris, cephalic index, facial index, nasal index, height, and Manouvrier's index. 67 (22%) of the 300 boys and 287 (29.3%) of the 984 adult males fell within the limits of selection as representatives of the racial types. Among the adults were found 102 Mediterraneans, 84 Nordics, 61 Alpines, and 40 Lorraines. For each racially homogeneous sub-group mean values for the morphological indices were computed. Furthermore, it was found that while the mean height of Frenchmen has increased 1 7/12 inches since the beginning of the present century, to a value of 5 feet 6 5/12 inches, the increase was unequally distributed among the racial sub-groups: 1 8/12 inches for Mediterraneans, 1 2/12 inches for Alpines, 10/12 inches for Nordics, and 5/12 inches for Lorraines.

The 697 individuals who showed a mixed racial morphology were also studied in order to determine the modalities of inter-racial morphology. It was found that contrast between hair color and iris color, and checkered iris, always in children and often in adults, manifests the mixture of two heterochromic races. Hyperdolichocephaly appears as the result of the crossing of the two dolichocephalic races, and hyperbrachycephaly as the result of brachycephalic crossing. Mesocephaly generally appears as the consequence of crossing dolichocephal and brachycephal. Crossing between the two shorter races increases stature of the cross toward the average values; crossing of the two taller races lowers it toward the same average values.

Finally, certain secondary morphological peculiarities have been studied, such as curly hair, fissured chin, and adherent earlobe.

The general conclusion from these studies is that Mediterraneans and Nordics, on the one hand, and Alpines and Lorraines on the other, undergo the same variations and act generally in the same way in crossing. Mediterraneans and Alpines seem to be the two "primitive" racial types of Europe; Nordics and Lorraines would seem to be merely depigmented varieties showing increased stature.

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THE EVOLUTIONARY TAXONOMY OF THE HOMINIDAE IN THE LIGHT OF THE PILTDOWN INVESTIGATION

J. S. Weiner

The Piltdown disclosures affect the study of human evolution in a number of ways. In the first place we can point to the rapid introduction of a whole battery of physical and chemical tests for the establishment of the relative chronology of fossil assemblages. Oakley, who resolutely pioneered the fluorine method for the dating of Galley Hill and Fontéchevade (amongst other remains), and promoted many of the new tests used in the Piltdown investigations, has now used some of these to confirm the antiquity of the Swanscombe remains (Oakley, 1955). Recently (Wendorf *et al.*, 1955), estimations of nitrogen, organic carbon and water as well as fluorine have been employed to good effect on the West Central Texas skull with results lending support to the genuine antiquity of this early pre-Folsom American. As with Piltdown, analysis of soil adhering to the specimen also yielded useful information. Other examples of fluorine 'dating' are the tests made on the *Pithecanthropus* remains (Bergman and Karsten, 1952) and on the Ganovče (Czechoslovakia), Neanderthal-like skull-cap (Vlček and Pelikán, 1956). I do not want now to say more about this beneficent outcome of Piltdown, its catalytic influence on dating techniques has been well recognized.

The second beneficial outcome—and one hopes that this is also generally appreciated—is the fact that the removal of *Eoanthropus dawsoni* does nothing to weaken our evolutionary picture. On the contrary, there has accrued a decided gain in coherence and a marked narrowing of the field of controversy in assessing relationships and trends in the phyletic history of Man. We are freed from the spectacle of seeing some students gratuitously side-stepping this fossil in their interpretations, while others (a minority but no less able) continued to build their evolutionary schemes with *Eoanthropus* as the very keystone. I trust that the general strength and coherence of our present picture will be clear by the time I have considered, in rather more detail, the third useful service which the Piltdown investigation can perform. It enables us to recognize a number of weaknesses in our phylogenetic interpretation, and can therefore help towards improvement in our taxonomic and palaeontological reasoning, so that we may build up, at any time, as unassailable a theory as the evidence will allow. In this endeavour to attain analytical rigour in human palaeontology we are fortunate in having, among other works, the recent publications by Simpson (1953) and by Le Gros Clark (1955). What I have to say is in the nature of an appendage to the systematic and illuminating treatment of taxonomic problems provided in these works. Piltdown has emphasized a number of quite far-reaching theoretical issues, some obvious and others less so, and they seem to me to deserve examination in reference to the main stages of hominid phylogeny.

1. PILTDOWN MAN: SOME PHYLETIC CONSIDERATIONS

What brought Piltdown Man into serious dispute and finally ill-repute was not, scientifically speaking, the anatomical incongruity of a modern human brain-case with an ape's jaw. Smith Woodward (Dawson and Woodward, 1913) fully recognized this anatomical peculiarity but for him, of course, it was just that which made the 'missing link'. On purely anatomical grounds one could not decisively dissociate the skull from the jaw—the broken articular condyle of the mandible left it an open question whether the jaw would 'fit' the cranium and the 'human' characters of the molars and canine could not be discounted. There was also the apparently human appearance of the roots of the molar teeth shown in the early X-ray photograph.[1] All this was of course backed up by the close territorial coincidence of the remains, their apparent state of mineralization, the supposed antiquity of the gravels and by the second Piltdown find. We must clearly understand that it was strictly as an outcome of palaeontological inference that Piltdown man eventually assumed an isolated and peculiar place in the evolutionary picture. Thus, the South African prehuman hominids, *Pithecanthropus* from Java and China, and the varieties of Neanderthal

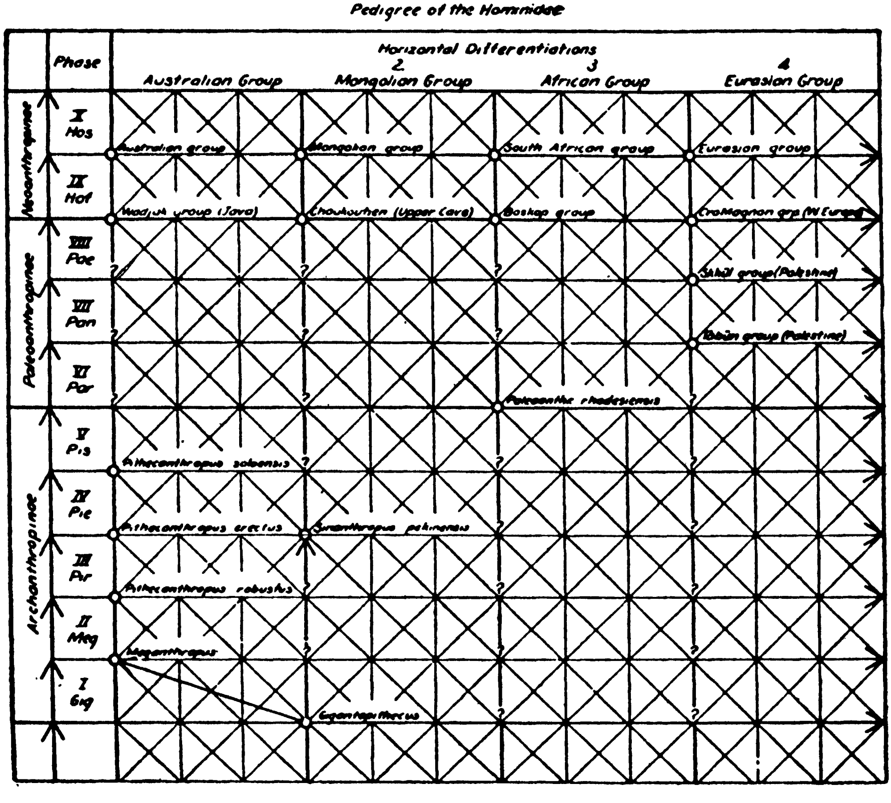
Pedigree of the Hominidae
Phase
Horizontal Differentiations
2
3
4
Australian Group
Mongolian Group
African Group
Eurasian Group
Neoanthropinae
Paleoanthropinae
Archanthropinae
X Hos
IX Hof
VIII Pae
VII Pan
VI Par
V Pis
IV Pie
III Pir
II Meg
I Gig
Australian group
Mongolian group
South African group
Eurasian group
Wadjak group (Java)
Choukoutien (Upper Cave)
Boskop group
CroMagnon grp (W Europe)
Skhūl group (Palestine)
Tabūn group (Palestine)
Paleoanthr. rhodesiensis
Pithecanthropus soloensis
Pithecanthropus erectus
Sinanthropus pekinensis
Pithecanthropus robustus
Meganthropus
Gigantopithecus
?

Fig. 1. Weidenreich's Chart illustrating his conception of the evolutionary phases of man

and fossil sapiens man were found to evince a common evolutionary trend—the braincase expanding progressively, the face and jaws undergoing reduction and showing increasingly pronounced unapelike features in the dentition and in the absence of the simian shelf so characteristic of modern apes. The combination, in Piltdown, of a large braincase with a predominantly pongid dentition and jaw with simian shelf ran flagrantly counter to this.

It was this violation by the Piltdown assemblage of the morphological continuity presented by a large and impressive array of fossil hominids which roused anxiety in Arthur Keith (1948), a suspension of belief in Le Gros Clark (1949), and downright rejection by Weidenreich (1947). The latter viewed Piltdown with the greatest suspicion, avowedly on the score of its anomaly in the morphological gradational continuum (Fig. 1) which he conceived as representing human phylogeny (Weidenreich, 1947). There is much to quarrel with in Weidenreich's simple phylogenetic scheme but he gives cogent reasons for the impossibility of admitting *Eoanthropus dawsoni* to it. (The terminology of the stages 'Archanthropinae', 'Palaeanthropinae' and 'Neoanthropinae' is taxonomically quite inadmissible.)

This very brief history of Piltdown man's fate in the light of palaeontological deduction raises at once two general issues of taxonomic and phylogenetic significance. In the first place it brings up for examination the validity of accepting the course of hominid evolution as a single, morphological progression within the relatively short period of the Pleistocene. The example of Piltdown indicates that a theory which postulates a close morphological affinity within the Hominid family carries a certain 'predictive' power, in throwing doubt on the status of specimens for which quite separate lines of development must be postulated. Secondly, the Piltdown investigation raises the question of the admissibility to any evolution theory of disputed, uncertain or fragmentary evidence. The Piltdown example suggests that in general a serious methodological error is likely to be incurred when a theory is built up embodying doubtful specimens—as is the case still in some current theories. The lesson of Piltdown points to the logical necessity of excluding uncertain specimens so as to give the internal cogency and reliability which will in fact provide the basis on which to judge the possible nature of the doubtful specimens as well as to 'predict' with a measure of confidence the existence of missing stages. In a descriptive science such as palaeontology, obscure specimens like Piltdown serve in fact as tests of theory. A well-based theory can be refuted only by evidence at least as strong as that which it embodies, so that the inclusion of doubtful specimens weakens the theory from its inception. Thus I think it scientifically of doubtful value at the moment to frame phylogenies in which Swanscombe Man or the Kanam material are used as key specimens with an *a priori* assumption of their nature.

The acceptance of Piltdown Man leads inevitably to the postulate of unconnected lines of evolutionary change within the Pleistocene. In two well-known evolutionary theories (Fig. 2 A and B) taken from Dr. Clark Howell's (1951) discussion, Piltdown Man is accorded the status of a true and distinct ancestor of *H. sapiens*. This judgement invites a line of descendants which includes an uncertain specimen like Swanscombe (whose full morphology, whether *sapiens* or 'generalized' Neanderthal, is still unclear) and a disputed one like Galley Hill now known of course to be inadmissible as an early fossil. Once accepted, this questionable scheme yields a picture of hominid evolution with little internal affinity, ignores the close morphological similarities of specimens in the two separate lines, and invokes parallelism to an alarming extent.

In Leakey's (1953) phylogenetic tree as a result of the introduction of the disputed specimens from Kanam and Kanjera, we see a complete subdivision of the non-australopithecine hominids throughout the Pliocene and the Pleistocene. Forms like Steinheim Man appear as quite distinct in their evolution from *H. sapiens* though practically all workers regard them as directly ancestral to sapiens man and indeed Le Gros Clark (1955) has after most careful taxonomic reasoning allocated these specimens to *H. sapiens* itself. The degree of parallelism postulated in the hominid radiation in the tree is inordinately great, the ancestral relationship between so-called 'Neanthropinae' and 'Palaeo-anthropinae' is left extremely vague and is thrown back to an unknown Miocene past. This scheme, designed to secure a very early Pleistocene date for the Kanam remains—and therefore requiring the wide separation of sapiens from the remaining hominids—raises more problems than it solves while setting aside well-recognized affinities. When such results follow from the introduction of a disputed specimen, it seems only to emphasize the doubtfulness (of age) investing the specimen and until further evidence comes along, the phylogenetic tree must be built without it.

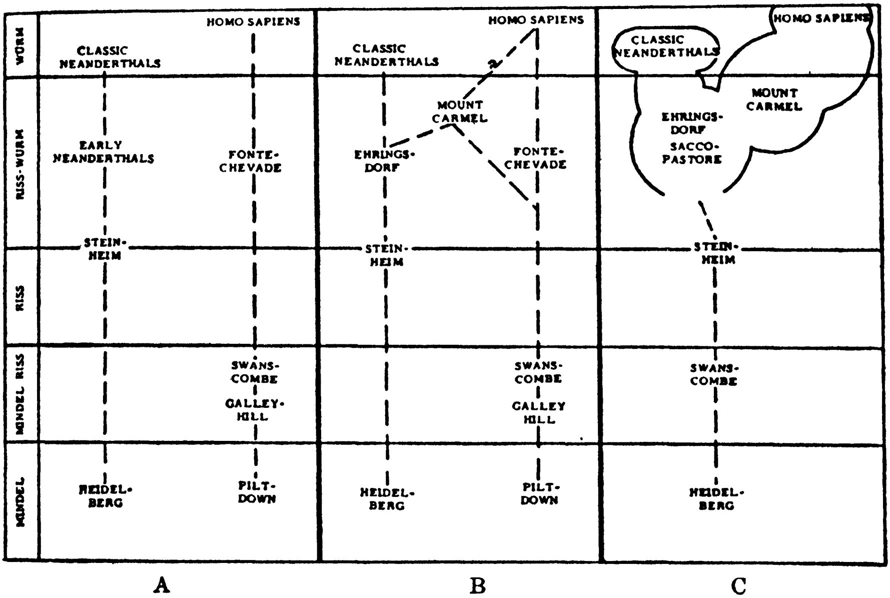

Fig. 2. Diagrams to illustrate three interpretations of hominid phylogeny (from Howell, 1951)

In itself, the recognition of the anomaly of Piltdown, or the further anomalies it admits, does not finally establish the fact of a close morphological continuity within the *Hominidae*, but it certainly provides one strong presumption in favour of such a state of affairs. And while the removal of Piltdown (and Howell's figure (Fig. 2C) gives an example) clears the way for a picture of closer affinity, it is of course necessary by actual morphological comparisons to establish the existence of a close-knit phyletic succession.

Piltdown, before we leave it, furnishes food for thought on two further matters of taxonomic analysis.

To employ debatable specimens as keystones of a phylogenetic theory is to invite trouble, as we have seen. It is also hazardous to lean too heavily on single specimens, even when well attested, for tracing the course of evolutionary change from one stage to another. Sometimes a single specimen will look convincing as a 'missing link' but we can only be on safe ground, as has been fully discussed by Simpson (1953), when we can see evolutionary transformation arising from a whole phyletic 'lineage' (or 'matrix') displaying a range of variation for selection to work upon and this is a condition which is also in

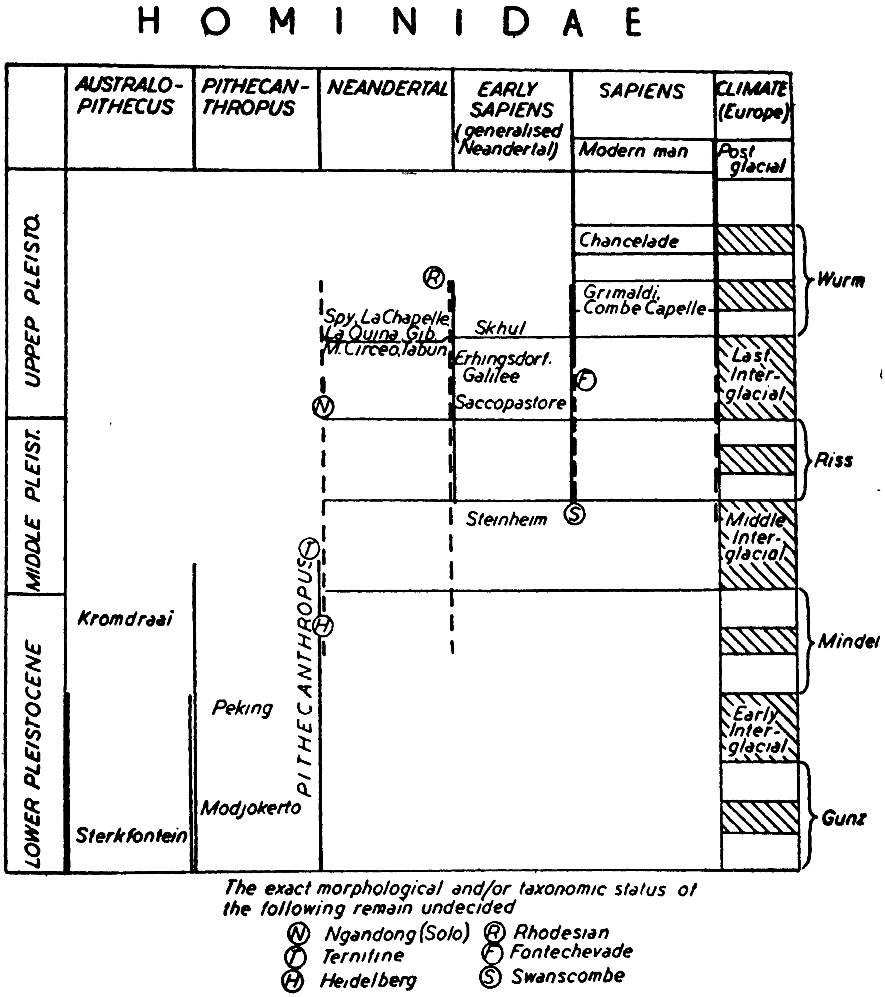
HOMINIDAE
AUSTRALOPITHECUS
PITHECANTHROPUS
NEANDERTAL
EARLY SAPIENS (generalised Neandertal)
SAPIENS
Modern man
CLIMATE (Europe)
Post glacial
UPPER PLEISTO.
MIDDLE PLEIST.
LOWER PLEISTOCENE
Chancelade
Grimaldi, Combe Capelle
Wurm
Spy, La Chapelle, La Quina, Gib., M. Circeo, Tabun
Skhul
Ehringsdorf, Galilee
Saccopastore
Last Inter-glacial
Riss
Steinheim
Middle Inter-glacial
PITHECANTHROPUS
Kromdraai
Mindel
Peking
Early Inter-glacial
Modjokerto
Sterkfontein
Gunz
The exact morphological and/or taxonomic status of the following remain undecided
Ⓝ Ngandong (Solo)
Ⓣ Ternifine
Ⓗ Heidelberg
Ⓡ Rhodesian
Ⓕ Fontechevade
Ⓢ Swanscombe

Fig. 3. Phyletic sequences of the *Hominidae*

accordance with genetic requirements.[2] The jump from Piltdown to say Swanscombe or Galley Hill (Fig. 2 A and B) is based on nothing like a 'lineage,' and the morphological transformation involved is left almost entirely to the imagination. Any morphological sequence for the *Hominidae* should display the various evolutionary stages as phyletic 'lineages'. The development along these lines may take various patterns; there may be continuous transformations along a given line, one palaeospecies giving rise gradually to another (this is suggested below for the evolution of Neanderthal from *Pithecanthropus* and late sapiens from early sapiens); there may be at the same time splitting off with divergent lines showing different rates of change (the evolution of Rhodesian and early sapiens from *Pithecanthropus* approximates to this pattern) or the original lineage may undergo relatively little morphological change over a long period (this is exemplified by the persistence of a given form after the splitting off of a new line has occurred). Morphological sequences of these kinds, discussed below, are summarized in Fig. 3.

This brings us finally to an old question: the relation of chronological to morphological succession. The Piltdown investigation as we know, has helped through new techniques towards establishing reliable relative chronology. Yet it indicates that dating, however reliable, will not necessarily settle questions of morphological succession. Thus when Piltdown was regarded as the earliest known hominid, a line of descent of the sort already indicated (in Fig. 2 A and B) was found for it. But even when Piltdown Man was for a while regarded as of late Pleistocene age, various workers were able to accept it as a specialized or aberrant terminal product.

These four issues raised by Piltdown, of morphological continuity, of the danger of disputed specimens, of the weakness of single specimens and the need for adequate series, and of chronology, will now be briefly pursued in an attempt to evaluate our present understanding of hominid evolution.

2. AUSTRALOPITHECUS

The phyletic position of this genus is, as Dart, Le Gros Clark, Broom and Robinson have demonstrated, such as one would predicate for a phase of hominid evolution preceding *Pithecanthropus* and converging backwards towards more pongid ancestors of the hominids. The indirect evidence of comparative anatomy as well as that afforded by fossil *Hominoidea* strongly favours such an approximation of the pongid and hominid lines. The South African fossils furnish morphologically the early stages of the developing line of the hominids for they still carry with them pongid traits (just as the Miocene fossil apes still bear cercopithecoid traces). The small brain and large muzzle and face, the large molars and sagittal crest are what we may expect as pongid reminders in these representatives of early hominids in whom such pongid specializations as the prominent milk and late erupting permanent canines, the associated unicuspid first pre-molars and the brachiating limb and associated pelvic structure are all absent. Le Gros Clark (1955) has illustrated in detail how the over-all morphological patterns diagnostic of the *Hominidae* place *Australopithecus* in the hominid, and not the pongid family. The genus shows wide variability (cf. *Telanthropus*) and evidence of local differentiation but its hominid characters are such as to furnish the 'matrix' for further development in the direction of *Homo*. This deduction is supported by the close resemblances Robinson (1953) has been able to trace between Sterkfontein and so-called *Meganthropus*, a *Pithecanthropus* variant, according to Le Gros Clark

(1955). The available material, it is hardly necessary to remark, escapes the strictures directed at the employment of morphologically debatable specimens or of single specimens taken to represent a series.

When we come to examine the question of chronology, *Australopithecus* at once confronts us with the dilemma of reconciling morphological and temporal succession. Oakley (1954) has given reasons for ascribing an upper Villafranchian date for the earliest known representatives so it is quite possible that these antedate the first known *Pithecanthropus* from Java. Robinson (1952) argues for a chronology earlier than that of Oakley. But even if these datings do not prove tenable, the facts of morphological succession have led most workers to accept the *Australopithecines* as the surviving representatives of 'prehuman' creatures which gave rise to the 'human' *Hominidae* at an earlier time. There can be no theoretical objection to this view. The known Australopithecine fossils seem geologically to cover a fairly long period, long enough to have undergone some morphological differentiation. The fact of survival of an ancestral stock is of course a commonplace occurrence in other mammalian evolutionary sequences (e.g., *Eohippus*).

If most, or all, of the representatives of this genus can be regarded as chronological survivors, we must be prepared to admit a similar possibility at later Hominid stages, as we shall see. Thus *Australopithecus* provides the earliest known Hominid sequence in which one line continues the original lineage and another, it is inferred, diverges to give rise to *Pithecanthropus.*

3. PITHECANTHROPUS

The morphological continuity of *Pithecanthropus* with *Australopithecus* has already been referred to. Most workers have long been convinced, from the relatively abundant material, that there are no valid morphological distinctions sufficient to make, at most, a species difference between Java and Pekin Man. The latter shows a still further development in the direction of *Homo* particularly in its brain-size. The morphological characters of *Pithecanthropus* are such as to provide a 'lineage' or 'matrix' from which the further development of the large brained genus *Homo* appears entirely feasible. As links with *Homo* we have the Ternifine mandibles with their Pithecanthropine and Heidelberg resemblances and, later, the Ngandong calottes (Weidenreich, 1951). Chronologically, the early representatives of *Homo* such as Heidelberg and Steinheim arise apparently after the appearance of the earliest *Pithecanthropus* (Zeuner, 1952) so that here the accord between the phylogenetic and the temporal succession seems satisfactory. At the same time the evidence strongly suggests a fairly late persistence of the genus probably into the Middle Pleistocene. If Solo Man is regarded as closely related (as Weidenreich argued) the Pithecanthropine genus would have survived as late as the 3rd interglacial or later, manifesting continuous development in one line along with a divergent line of transformation to *Homo.* The later *Pithecanthropus* specimens thus continue after phyletic splitting off has taken place, a pattern we have seen already with *Australopithecus.*

4. HOMO

The taxonomic status of Ternifine Man (so-called *Atlanthropus*), Solo Man and Rhodesian-Hopefield Man remain rather undetermined at present but all other specimens of *Homo* fall into the two species of sapiens or Neanderthal. One of the most important conclusions advanced by Le Gros Clark in his recent book (1955) concerns those specimens till now usually called 'generalized' or 'early' Neanderthaloids and represented by the fossils from Steinheim, Ehringsdorf, Saccopastore, Mount Carmel and Krapina. That these should be assigned to the category of *H. sapiens* despite the possession of features reminiscent of Neanderthal Man has been urged by Le Gros Clark who refers to them as 'early *H. sapiens*'. It seems also eminently reasonable, as argued by that author, by Clark Howell (1952) and Vallois (1954), that by progressive loss of Neanderthaloid traits these 'early sapiens' forms develop chronologically as well as morphologically into fully fledged sapiens forms of which the earliest certain member is probably Fontéchevade Man (Vallois, 1954). The developing sapiens 'line' therefore stretches back to Steinheim Man if this is indeed as early as the Second Interglacial (Adam, quoted by Zeuner, 1954). Notice that this sequence owes nothing to views on Swanscombe or Fontéchevade or the Quinzano occipital (though those forms also fall naturally into the 'early' sapiens category taxonomically). The theory merely 'predicts' a likelihood of the Second Interglacial Swanscombe turning out to be less like sapiens and more like Ehringsdorf or Steinheim but in the absence of definite information we are not obliged to build these specimens into alternative and more speculative theories.

The 'primitive type of *H. sapiens* came into existence by the Middle Pleistocene', writes Le Gros Clark, 'presumably from an earlier small-brained type represented by the *Pithecanthropus* stage of human evolution.' This early sapiens stage gives rise, as we have accepted, to later *H. sapiens*, but according to prevailing views (Fig. 2 C) it also diverges to yield the late classical Neanderthal (i.e. *H. neandertalensis*) of the first phase of the last glaciation, this radiating line thereafter becoming extinct. I believe that this derivation of Neanderthal is open to further examination. If *Pithecanthropus* can give rise to 'primitive Neanderthal' as the 'early *H. sapiens*' appear to many workers (and some Neanderthal features are indeed marked, e.g. in Saccopastore, Steinheim and Takin), and if this in turn seems morphologically capable of progressing to Neanderthal, *Pithecanthropus* itself should *a fortiori* be capable of developing in a Neanderthal direction. Indeed in Solo Man there is very strong evidence of this, for this form, closely allied to *Pithecanthropus* (Weidenreich, 1951), shows in the skull (but not in the limbs) a close approximation to *H. neandertalensis* in a number of important features (Le Gros Clark, 1955, p. 77). To argue that it is from *Pithecanthropus* that Neanderthal and Solo Man ultimately developed is of course to revert to Weidenreich's viewpoint, but without at all adopting his view that Neanderthal gave rise to generalized Neanderthal and sapiens.

The strength of the belief in a derivation of classical Neanderthal from early sapiens lies in the concerted appearance of the Neanderthal form later than all, or the majority, of early sapiens or generalized Neanderthal. For 4 or 5 classical Neanderthalers the last glaciation date is firmly established; for several others the dates remain unascertainable. But there is a possibility (Zeuner, 1954) that Monte Circeo, a 'classical' Neanderthal man, may be late in the last interglacial thus weakening the chronological succession and throwing back the postulated transition to Steinheim, the only 'early sapiens' which seems early enough (Zeuner, 1954). The transition of early sapiens to Neanderthal has at times been placed at the late Mt. Carmel stage but this becomes superfluous if Steinheim is as old as suggested. The later representatives of 'early sapiens' such as Krapina, Saccopastore and Skhul thus appear as 'survivors' of the Steinheim stage, after the transition to H. sapiens has been affected, as evidenced in the occurrence of Fontéchevade.

On the alternative view advanced here, the relative dating of Neanderthalers and early sapiens is not critical. The classical Neanderthalers are all late derivatives of the suggested *Pithecanthropus* 'lineage' which also gives rise to the early sapiens sequence. The early sapiens 'line' persists after splitting to give rise to late sapiens. We have here then a further instance to add to those provided by *Australopithecus* and *Pithecanthropus* of the persistence of a 'lineage' after splitting off has occurred.[3]

The most pertinent objection which can be advanced against the belief in a change from the relatively small-brained 'light' early sapiens form (e.g. Steinheim) to the large-brained 'heavy' classical Neanderthal is that it appears to demand an inordinate degree of evolutionary reversibility, whereas Weidenreich and others have demonstrated a continuity in the morphological transition from *Pithecanthropus* to 'classical' Neanderthal, a transformation for which Solo Man seems to offer objective evidence. In view of the admitted weaknesses of invoking reversibility, especially of complex morphological features (Colbert, 1949), one must register serious doubt at the re-emergence, on passing from early *H. sapiens* to Neanderthal, of morphological patterns characteristic of the earlier Pithecanthropus stage. Amongst such features we may list the following, comparing *Pithecanthropus*, early sapiens and classic Neanderthal:

	Pithecanthropus	*Early Sapiens*	*Neanderthal*
Skull height (relative)	low	high	low
Forehead	sloping	vertical	sloping
Face relative to brain-case	large	small	large
Occiput	angulated	rounded	angulated
Cranial base	less flexed	more flexed	less flexed
Supraorbital torus	continuous	ciliary and orbital parts	continuous
Chin	absent or rudimentary	present	absent or rudimentary

In reiterating Weidenreich's view of Neanderthal as an expanded and modified version of *Pithecanthropus*, there is no need to deny the local peculiarities of the classic Neanderthalers (indeed late sapiens itself develops his regional peculiarities). The heavily built limb bones showing pronounced curvature and the strongly developed spinous processes of the cervical vertebrae are features indicative of this specialization. As Clark Howell (1952) has argued, isolation and all its genetic consequences must have been at work to produce the relatively homogeneous terminal populations of the Neanderthal line.

5. THE TAXONOMY OF RHODESIAN MAN

The suggestion put forward above embodies the view of Vallois, Howell, Le Gros Clark and others of a transition from early sapiens to later sapiens—a process in the course of which Neanderthal-like traits are eliminated. Such a process it appears would begin with the small-brained Middle Pleistocene *Pithecanthropus* precursor. If this precursor can undergo modification in both the Neanderthal and early sapiens directions, there is good reason to postulate that it could also give rise to Rhodesian Man. Rhodesian Man manifests, as has long been recognized, a curious amalgam of Neanderthal and sapiens characters, a combination recognizably different from that seen in the 'early sapiens' group, and with resemblances also to Solo Man. It could also be argued that Rhodesian Man has 'early sapiens' as a possible ancestor, by those who accept 'early sapiens' as ancestral to both Neanderthal and full sapiens; the arguments against this view have been considered above. In any case, Rhodesian Man appears as an outcome of a 'matrix' in which early sapiens, late sapiens and Neanderthal are all developmentally interconnected.

What taxonomic designation then should Rhodesian Man be accorded? The analogies between this form and classical Neanderthal are manifold. Just as the latter appears as the European 'terminal' of one evolving line so too does Rhodesian-Hopefield in the South African enclave. Both stand in a roughly similar descendant relationship to *Pithecanthropus*, both have their resemblances to Solo Man and to early and late sapiens, both appear to represent late populations proceeding to extinction. As long as a species difference is held to separate Neanderthal from sapiens, it seems necessary to accord a species ranking to Rhodesian Man—as of course is often done. The alternative, a subspecific status, immediately brings up the difficulty as to which of the two species of *Homo* the attachment should be made; as already pointed out neither, on our present evidence, has strong claims to *immediate* ancestral relationship to Rhodesian Man.

The more one considers the overlapping and interwoven relationships between all the forms of *Homo* the more one feels that taxonomically, as Dobzhansky (1944) and Mayr (1950) have urged, these all might more appropriately be considered as of subspecific status within a single species *Homo sapiens*. Force is given to this by Mayr's objection to a generic difference between *Pithecanthropus* and *Homo*. As he points out, and the evidence discussed above supports his contention, no morphological gap exists between these forms such as to justify generic discrimination. It could be urged that the two postulated species, *H. erectus* and *H. sapiens* occupy a similar 'adaptive zone' as is to be expected with a single genus. This is the case biologically and even culturally if we compare the technological and fire-making activities of Pekin Man with the earliest sapiens forms. Thus Neanderthal, early and late sapiens as well as Rhodesian Man would all be of subspecific status and would be described by an appropriate trinomial—the regional variation which these subspecies show would be that of local 'races' or local populations or even demes. It could be further urged that if Neanderthal and sapiens are only of subspecific rank, there should be evidence of 'crossability' between them. The Mt. Carmel and the Krapina populations have of course been regarded by some as just such relatively late hybrid populations. Whether Solo Man would be a subspecies of *H. erectus* or *H. sapiens* is a moot point which merely emphasizes the intermediate character of this form—and either is preferable to creating a full species category for Solo Man, either of *Pithecanthropus* or *Homo*.

CONCLUSION

It seems that unless the strong resemblance and continuity of morphological pattern between the *Pithecanthropus* genus and Neanderthal can be shown to be merely superficial, there is much to urge in favour of a theory in which the evolutionary radiation to early sapiens and classical Neanderthal (as well as to Rhodesian Man) takes its course from some early more neanderthaloid form on the *Pithecanthropus* line. The existence of such a stage is not entirely a matter of inference, for the incomplete finds from Montmaurin, Heidelberg and Algeria give some grounds for hope of discovery of specimens which should serve to test the theory. The Montmaurin mandible recently described by Vallois (1956) fits the theory here described, both chronologically and morphologically. The specimen is stated to be earlier than all the classical Neanderthal and it is strongly Neanderthal in conformation while showing clear affinities with Heidelberg Man. Ternifine Man in turn seems to have Heidelberg and Pithecanthropine affinities. It should, however, be pointed out that despite the arguments presented here the alternative theory of Neanderthal derivation cannot be summarily dismissed, for it is certainly necessary to re-examine in detail the morphological relationships of *Pithecanthropus* from Java and China to early sapiens and to Neanderthal shown only in outline in the table presented above.

In summary, and to illustrate the main points of this communication, Fig. 3 shows the Hominid sequences as they appear on present evidence. (A genealogical 'tree' has been avoided as a method of display which is apt to be over-simplified to the point of error.)

(1) The *Hominidae* present a succession of overlapping 'lineages' for the four groups considered here. It is clear that *Pithecanthropus* and *Homo* give an appearance of evolutionary differentiation which is at once reticular and directional.

(2) In the progression shown there is an absence of reliance on debatable or single specimens.

(3) There is apparent the widespread feature of persistence of each 'lineage', after 'splitting' to the next 'level' has occurred.

That our present picture of human evolution is a coherent and impressive one and the loss of Piltdown Man has in some measure helped, I trust has been borne out by the foregoing discussion.

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Notes

1. Though it proved in the end a most suspicious circumstance that only *one* character appeared positively and unequivocally to connect skull and jaw, the wear on the molars (Weiner, 1955).

2. A case can thus be made for those who doubted Dart's claims in 1926 based on the single immature Taung skull and for deprecating the naming of a new genus on the basis of a single tooth as did Davidson Black. The case of the ill-fated *Hesperopithecus* remains the greatest warning.

3. The persistence of 'ancestral' forms may reasonably be regarded as showing that the transition from one stage to the next extended over a long period invoking repeated appearance of 'intermediates'. This is where a 'lineage' representation fits the facts better than any genealogical 'tree.'

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SECTION VII

APPLIED ANTHROPOLOGY

APPLIED ANTHROPOLOGY IN THE BELGIAN TERRITORIES IN AFRICA: (AN EXPERIENCE OF INTEGRATION OF THE TRIBAL INSTITUTIONS INTO THE PATTERN OF THE NEW SOCIAL ACTION IN CENTRAL AFRICA)

G. E. J. B. Brausch

1. THE AFRICANS AND ACCULTURATION

Europeans consider the matter of acculturation (the process and result of adopting the cultural traits of another group) of the African and European civilizations as a transition from an inferior status to a superior one. They are convinced that the whole Western organization is superior to the African, that the political, social, economic, familial and trade-union institutions are superior to the African institutions of tribe, clan, and corporation.

This opinion is undoubtedly shared by a few Africans, among the intellectual elite. But the great majority of the Africans and even a fraction of the intellectual elite (a fraction much larger than is generally believed) consider with complete objectivity that this problem arises not from the superior-inferior difference *per se* but from the contact, the coming together of two dissimilar civilizations: theirs and ours. In their opinion, our superiority is manifest only in the technical field. As for the political, social and family forms of our society, they hesitate and even choose not to accept our institutions while they continue to give preference to their own traditional patterns.

The problem related to the development and change of the native collectivities under the influence of the European techniques and economy must be studied from the point of view of economic underdevelopment.

The populations of the Belgian Congo wish and are trying to move beyond the present stage of economic underdevelopment. In order to do so, they request our assistance, particularly in the technical and economic fields. But these same populations desire that this technical and economic development be realized as much as possible within the frame of their traditional institutions.

It is in recognition of this desire of the African populations, i.e., "technical and economic development realized as much as possible within the framework of their traditional institutions," that the "Institut de Sociologie Solvay" was moved to organize, last January, an international conference on the native economy.

In his opening address, Professor Janne, Director of the "Institut de Sociologie Solvay," noted the various methods evident when studying the means being used to encourage and accelerate the development of the African collectivities.

First, the technocratic method, according to which technical means are all-sufficient; it is therefore adequate to send only technicians and machines to ensure the economic development. Even family welfare has been influenced by this approach. In certain African homes, the women are taught how to prepare jams and preserves with mangoes, papaws, guavas and to make cakes with corn or manioc flour, and are even taught the techniques of "haute-couture," whereas the traditional backgrounds of homemaking and dressmaking are ignored.

The "ethnocentric" method, which aims to impose, with our techniques, all the values and also all the attitudes and motivations of the west. This is a frequent mistake with certain sociologists who think that the process of economic development is a perfect reproduction of the industrial revolution of the western world.

Finally, there is the "ethnologist" method which is shared by the traditional elite of the underdeveloped countries, who are disciples of the traditional system. According to this tendency, every element of the African socio-cultural structure plays an important part in the maintenance of that structure, considered as a coherent whole in which every piece is indispensable. This method puts technical knowledge at the disposal of a traditional society, refusing to change anything in its structure or its values. The use of any new technique entails certain methods of working, a division of labour, and many changes in human relations. This, necessarily, brings about change in that society.

Prof. Janne is of the opinion that these three methods are naive as sociological and economic approaches to the fundamental problem. His conclusion is that the solution of the problem raised by the development of the native collectivities is to be found midway between the "ethnocentric" and the "ethnologist" attitudes. This means that technical and economic assistance should be given to these collectivities as much as possible within the framework of their traditional institutions. Only such a logical procedure will reduce to a minimum the unbalances and jolts occasioned by the interpenetration, within the social body, of the African and occidental forms, their conflicting values, attitudes, motivations and behaviours.

The technical and economic assistance entails therefore a modification of the traditional African institutions.

This certainly raises the most delicate of all the problems discussed here: how to capture the interest of the collectivities and to inspire their personal effort so that they will furnish the initiative and the drive to adapt and change their "underdeveloped" native institutions into a more fully developed integrated economic and technical society?

2. SPONTANEOUS ADAPTATION OF THE AFRICAN COMMUNITIES

Let us first consider how this society has been trying for three-fourths of a century to meet the new situations with its own resources.

The very first relations between Europeans and natives were based on treaties which the pioneers concluded with the established local chiefs. Those treaties were often confirmed by the exchange of blood or by other traditional rites according to native custom in concluding such pacts. The principal and subordinate chiefs were confirmed in their functions by the explorers, and authorized to carry them out as in the past.

At this stage, the character and structure of the native communities remained unaltered and their institutions continued unchanged. But the European pressure soon became stronger. The treaties of friendship were changed into treaties of sovereignty; the exchange of services between allies became obligations. Then the decision to speed up the development of the country engaged the forced collaboration of the natives. Resistance was not slow in developing. Quite logically the spearheads of this resistance were manifest in the established traditional institutions: brotherhoods, corporations, councils of elders, and other native associations or groups of community leaders.

By way of sanction, the Europeans took prohibitory measures which were evidently justified. Even the associations which were not forbidden could only with difficulty gain the confidence of the Europeans, who considered them as backward and hostile to progress.

Now these associations were precisely the mouthpiece of public opinion. It was through them that the people used to make known their wishes and wants. These associations furnished the real leadership of the native society. By ignoring these associations, by preventing them from participating in the new organization set up by the Europeans, the latter prevented the whole native community from adapting itself to the new way of life. The native organizations were thwarted in any desire to learn about the new situations and lines of conduct. The natives, however, searched for ways to overcome this exclusive action.

As a result of this conflict, several reactions can be noted. In certain regions, the European authorities adopted a more tolerant attitude and allowed the customary institutions to participate in the administration of the native districts. This participation was however restricted to certain juridical or political brotherhoods, neglecting most of the economic corporations (workshops of draftsmen, associations of hunters, fishermen, or farmers).

In other regions, the natives reacted in their own way. The reaction of the more primitive populations, which were stubborn and conservative, was characterized by the constant reappearance of the old associations. The repressive measures could not prevent this. In spite of the restraints and counteracting measures, the embers of misunderstanding continued, and occasionally flared up vigorously in rebellion.

In the regions where the organization of the populations has undergone greater changes on account of a more direct subjection, the new European concepts and principles have been more readily accepted, but the forms by which they were applied were rarely adapted to the natives' habits of mind and of work.

The Africans have then tried to create with their own resources a new social structure blending the new and old principles, the new and traditional aspirations, the Christian religion and their animistic and magical beliefs. They conceived a society based on the traditional organization but adapted to contemporary conditions. This resulted in the formation of religious sects such as the "Kibanguisme" and the "Kitawala," which tried to meet the spiritualistic aspirations of these populations. In these sects, better adapted to the native mind than are those of the Christian religion, they achieve a more satisfying result in their search for truth and right. The principles taught by these sects are at the same time in accordance with their traditional concepts and with contemporary living conditions.

Their main value is due to the fact that the natives feel at ease in their observances, devotions, and services of worship; that the religious practices are accomplished in their own midst by people with whom they are related by birth or by friendship (not by leaders who, after all, are strangers).

Elsewhere, social clubs have been founded which are imitations of European society with its complete hierarchy, organization and way of life. Their numbers bear European titles such as governor, commissioner, judge, doctor, nurse, and even "father" and "sister." These associations are seldom viewed with sympathy by the public authorities, who generally take restraining measures against them as soon as it becomes possible to show that they may become a menace to public peace and order.

It is nevertheless in those young movements, in those new associations, which are a reaction of the native society brought about by cultural contacts with the European civilization, that we should try to find the principles which will enable us to present a lasting solution to that problem.

Our modern administrative structure must be based on the organization of the native society. So if the latter offers a certain pattern of functional groups, it is such an organization that we should use as a base. However much we may respect tradition, it must never be considered as an end in itself. Tradition cannot be allowed to rule a human society but should be used as a factor of its progress. It will be useful only if it remains a living tradition, i.e., the expression of the actual strivings and aspirations of such a society.

It is therefore necessary to uphold and to encourage the active functioning groups such as:

the family; the groups of hereditary parentage (the tribes, clans, elders) with their systems of marriage and their group relationships; the juridical brotherhoods which are entrusted with the administration of justice; the political brotherhoods or fraternities who assure the government of the communities; the economic corporations: craftsmen, fishermen, hunters and farmers; the groups responsible for the organization of leisure time.

This new organization must not only have a conservative character but also be constructive; the old associations must eventually adapt themselves to the new forms and institutions lest they should disappear.

3. MEANS OF ACTION WITHIN THE AFRICAN RURAL COLLECTIVITIES (COMMUNITIES)

How will it be possible to realize such an adaptation of the traditional society with a minimum of inconvenience?

The necessary condition is that the economic and social development be a popular movement originating within the collectivities concerned and not a movement inspired from the exterior. This was also the conclusion adopted at the 28th session of the International Institute of the Various Civilizations, held at The Hague in September, 1953. This conclusion was recorded in the general report of Prof. Van Lier.

What happens generally when a colonial government decides to improve the living conditions of native populations?

The technicians of the governments concerned have a tendency to determine, on their own authority, the regions and localities where new realizations will be made and new techniques applied. This program is then executed without regard for the political, rural, and economic substratum of the populations concerned. This policy results in the creation of farmers' associations, co-operative stores, mutual-benefit societies, credit societies, and so many other institutions which enjoy a short-lived success because they are of foreign inspiration and have not developed roots in the communities concerned.

Now the rural and traditional collectivities often include institutions which pursue the same aims as the new institutions created by us. The native society already had its own co-operative societies, mutual-benefit and credit institutions, and also its own methods for the allotment of the soil.

These institutions are all intimately tied in with the needs of the abovementioned social groups: families, hereditary groups, brotherhoods and corporations. But owing to their archaic form, they are not quite able to accomplish their tasks in the modern world; they are not adapted to the modern way of life, and especially not to the modern techniques. They are nevertheless popular institutions which enjoy the sympathy of the people.

There are thus, on the one hand, institutions of European inspiration which are adapted to the modern living conditions but which are not in high favour with the natives because they are of foreign origin. On the other hand, there are the traditional native institutions which pursue the same objectives but are not at all adapted to modern living conditions. These institutions enjoy the sympathy and confidence of the natives because they are of local origin.

The solution of that problem is therefore evident: the structure of the traditional institutions must be adapted to the requirements of the modern world. This means that we must equip these institutions in such a way that they may become co-operative and mutual-benefit societies, conceive a family or collective system of credit instead of credit to the individual, accept collective ownership as a basis for farming communities, organize the progress of the craftsmen within their own traditional craft organizations.

Here are a few examples of possible realizations:

(1) *In the co-operative field*

(a) Give the *status of an agricultural co-operative society* to the village unit which is traditionally responsible for the regulation of the agricultural activities; their membership should be composed not of individuals but of family groups because the village according to custom is composed not of individuals but of families.

(b) Create *craftsmen's co-operative societies* traditionally linked to one another.

(c) Give the structure of *production co-operative societies* to the associations of hunters and fishermen.

(2) *In the field of the mutual benefit societies*

All the social groups which are traditionally entrusted with *mutual assistance* in case of birth, marriage, sickness or death could base their organization on that of the European mutual benefit societies, and constitute mutual benefit federations.

(3) *In the field of credit*

(a) It is advisable to consider the creation of a *family credit institution* for the matters of special interest of that social entity, for example: *housing* (building and loan associations).

(b) The creation of a *credit institution* in favour of craftsmen's workshop organized on the traditional corporative basis is also advisable.

(c) An *agricultural credit institution* may benefit villages grouped in co-operative societies.

(4) *In the field of education*

Workshops for the training of craftsmen could be created *at the level of the craftsmen's co-operative societies*, grouping the craftsmen's workshops of a region, technical group, tribe, or an economic group.

Courses of agricultural training could be organized at the level of the village or regional agricultural co-operative society.

These instances are not exhaustive, for it is evident that a thorough study of the native organization would reveal numerous possibilities of technical adaptation of the traditional institutions.

A second imperative, which is necessarily linked with the first, concerns the localization of all the realizations interesting the rural population; those realizations must be located in the rural areas concerned and not in posts occupied by Europeans.

In the Belgian Congo and in the Ruanda-Urundi, this principle has already resulted in a reorientation of the objects of the Native Welfare Fund (Fonds du Bien-être Indigène).

During the first years of the existence of that institution, many of its realizations were located in some of the principal establishments of the colonial government, in mission stations or in other small European settlements. This policy had been adopted because of the important facilities it afforded with regard to the servicing of those installations by European personnel. But the flaw inherent to such policy is that the African considers these European installations as foreign ones. They were not integrated into the native community. We did not attain our ends which is the development of the native rural collectivities.

The total structure of all the reorganizations aiming at the development of the native rural collectivities must be integrated into the native political and administrative structure and must contribute to the creation of a strong and harmonious native society.

Before taking any measures for the benefit of the rural groups, it is therefore necessary to examine the political, administrative and economic structures of the groups concerned and to investigate how these new projects could contribute not only to improve their sanitary, social and economic conditions, but also to consolidate their political and administrative structure. Every program concerning the rural collectivities must aim at the medical, social, and educational equipment of certain native administrative and political entities.

It is necessary to promote first the construction of a school, a dispensary, a maternity center, a piscicultural center, a farming center in a certain district or tribe. This means that it will be advisable to equip in the first place the main localities of native districts or sub-districts, which are the natural centers of native rural life.

Thirdly, it will be necessary to aid and permit the natives to collaborate in the social development so that they will be interested in their own prosperity and will contribute personally towards that goal. The natives should be given the opportunity to take the initiative and to assume responsibilities in the social field, and they should even be put in charge of key posts.

This presupposes the existence of a qualified personnel and the training of such a personnel. In this respect, the Government has recently taken certain initiatives: creation of a school of social service, of a social-service school of higher secondary degree, of a training center for directors of mutual-benefit and co-operative societies, and of schools for women social advisors, and also creation of six schools where native leaders will be taught how their collectivities can receive aid in the technical and social fields. These considerations have been approved by all the experts to whom they were communicated. These experts stated that they would follow with the greatest interest the experiments which Belgium intended to make in this field.

The first experiment of polyvalent social action according to this new formula was started in May, in the territory of Bongandanga, Equateur Province. It implies the co-operation, on the largest possible scale, of the new social center with all the governmental and semi-governmental services and with all the native institutions.

For lack of space, it is not possible to set out in detail a co-operation scheme. I shall therefore give only an outline, by way of example, of the different types of co-operation that can be established between the social workers and the traditional native institutions.

(1) *the native family* should receive the particular attention of the social worker;
(2) *the village group* and the several sub-groups which are responsible for the agricultural activities should be contacted by the agricultural expert and the teachers ("régentes") of rural domestic economy, through their land-chiefs or spokesmen;
(3) *the political juridical brotherhoods* should receive the attention of the social worker;
(4) *the sisterhoods with social character* should be visited by the social worker;
(5) *the fishermen's and hunters' association* should be contacted by the agricultural experts;
(6) *the craftsmen's workshops* should be advised by the iron and wood expert;
(7) *the traditional leisure associations and the masculine schools of initiation* should be contacted by the educator.

It appears therefore that, with regard to the rural areas, it is not impossible to organize the progress of the native collectivities by methods of technical assistance, applied as much as possible within the framework of the traditional institutions.

4. MEANS OF ACTION WITHIN THE AFRICAN URBAN CENTERS

The problem seems much more complicated in the urban agglomerations, the non-tribal centers or the cities located near government posts, the missions or the industrial and economic centers where people of extremely various origins are grouped.

When these communities are of recent creation, it will be difficult to find there any cohesion or organization at all; there will be no common ideas or feelings. This may account for the discouragement which sometimes pervades certain non-tribal centers and which results in insufficient output, ill-faith, desertions, lack of discipline, and drunkenness.

In the older urban centers we can, however, see a new structure emerging. There are first the native clubs of "évolués" (having received formal education), the associations of ex-pupils (alumni of mission schools) and other associations patronized by the Europeans.

Then there is the whole series of mutual-benefit associations, often founded on a common ethnical origin, and such associations as the Association for the unification of the language of the Bakongo, the Lulua Brothers, and the mutual-benefit Association of the Atetela. There are also women's mutual-benefit associations.

Why is it that there are so many associations in the urban centers? The best I can do to explain this is to refer you to what I wrote in a recent issue of the *Bulletin de l'Union des Femmes Coloniales*, in which I examined the modifications which the life of the native women has undergone in the industrial areas. In this article, I said that in their village of origin, the women led a very busy life: care of garden and livestock, picking berries and fruits, fishing, care of household and children, and also the social gatherings which she must attend.

The woman living in a city controls only a small piece of land, a great part of which is occupied by her dwelling. The consequence of her forced unemployment is that she feels lost. She gets bored in her new dwelling because she cannot carry out her usual activities. In addition to this, she feels that she is a stranger; there are no more relatives to whom she can talk about her good or bad luck, no more friends with whom she can evoke youthful memories, no more companions at whose side she used to till the soil under the hot rays of the sun, no more talks with the leader of the school initiation who continued to give her useful advice, no more help from her uncle, the notable who defended her rights at the village council.

If our black Eve wishes to become a city-dweller, she must adapt herself to her new surroundings. She must give up the simple blue loin-cloth because fashion requires her to wear multicoloured clothes. If she quarrels, she must apply to a tribunal composed of strange judges who apply a law of hybrid customs. She must buy her food at the market or in a shop.

How is she to establish social relations in this industrial urban center? When there is no other social link, no blood-relationship will act as the cement to bind her to others. The women try to become members of clubs grouping women of the same tribal origin. These clubs organize regular gatherings or festivities. In the beginning these clubs had no other purpose than the mutual assistance between their members in case of birth, marriage, illness or death.

The purpose of these mutual-benefit associations was gradually extended. The women soon gathered to chatter among themselves in the same way that our European ladies take their five o'clock tea. Fashion soon became a favourite conversation and often the principal purpose of the said associations, causing them to adopt frivolous club titles, such as "The Present Beauty," "Novelty," "Au Chic," "The Prettiest Dress," "The Lovely Girl," "Rosette," and many others equally frivolous.

It is justly feared that these associations will bring about the deprivation of their members, the more so as the latter gather in public houses, where drinks are served in an atmosphere which is not always conducive to a harmonious family and social life. It is therefore understandable that these women's associations are criticized. One should, however, always bear in mind that they are a manifestation of the women's aspirations for a new social life which could replace their social relations of the past.

It is the task of the social workers to meet those aspirations. They should acquire the conviction that they will have to deal in Africa with persons whose cast and habits of mind differ totally from ours. They must take into account the structure of the African society which is based on the principle of association and brotherhood, and not on that of individualism.

The social workers should therefore seek to contact the existing associations and try to make them pursue again their highly commendable primitive purpose: mutual assistance. These associations could also devote themselves to the organization of the leisure time of their members, but only incidentally and in a healthy spirit and atmosphere.

Thanks to the social worker, it will thus be possible for a new Eve, in the image of the old one of the African countryside, to emerge from the industrial "milieux."

If the social personnel could carry out its activities in such a spirit, it would render valuable services to Congo, for it would liberate the African from their present anguish, which is brought about by the unknown that confronts them.

Social assistance based on the African tradition is assuredly a most inspiring experiment. It is one of the best ways to bring about between Europeans and Africans the mutual understanding which will lead to a real Belgo-African Union. This union will be strong, in so far as it will be based not only on the understanding by the Africans of everything European, but on the appreciation by the Europeans of everything that is good and just in African civilization.

Antwerp, Belgium.

DE QUELQUES DIFFICULTÉS DUES AUX DIFFÉRENCES DE CULTURES, RENCONTRÉES DANS LES MISSIONS D'ASSISTANCE TECHNIQUE

Geneviève M. d'Haucourt

Les missions d'assistance technique aux États-Unis furent organisées en 1951 suivant accords passés entre une agence spécialisée du gouvernement américain et les pays européens amis et alliés. Elles ont depuis été étendues à des nations du monde entier. Le but était d'aide indirecte: les représentants de diverses parties de la population des pays intéressés, et notamment du secteur industriel, seraient invités à visiter les États-Unis, et tout spécialement leurs usines. Ils pourraient ainsi constater par eux-mêmes le niveau de productivité américain et les moyens employés pour l'atteindre. Forts de cette expérience, ils pourraient entreprendre d'améliorer de manière analogue la productivité de leurs propres entreprises, et contribuer ainsi au relèvement de leurs pays respectifs. Cette hypothèse de travail présumait généralement que la mentalité des visiteurs était semblable à celle des Américains. Mais cette présupposition était gratuite. Bien que les divers pays en jeu relevassent tous de ce qu'on est convenu d'appeler la "civilisation" occidentale, ils n'en présentaient pas moins des différences culturelles qui ne furent guère étudiées et qui furent à l'origine de nombre de difficultés, souvent irritantes et incompréhensibles pour les organisateurs. L'étude en est d'autant plus recommandable que les missions, les voyages d'échange se multiplient et mettent en jeu des peuples de civilisations tres différentes, et que les difficultés plus considérables qu'ils présenteront pourraient être prévues et prévenues s'il est tiré parti de l'expérience acquise.

Malgré leur nouveauté les missions d'assistance technique se situaient, pour les Américains, dans des patterns familiers, notamment celui de la coopération entre égaux. Depuis les temps de la "frontier," des "log rollings," des "house raisings," des "bees," les Américains ont eu l'habitude de donner un coup de main à autrui pour qu'il puisse ensuite se tirer d'affaire tout seul: "to help them to help themselves." De nos jours encore, nombreuses sont les manifestations de cet esprit d'entr'aide, telle la reconstruction en 1953 du quartier de Flint, Michigan, qui fut arasé par une tornade. La coopération existant, notamment dans les "Trade associations" entre personnes et sociétés par ailleurs concurrentes, est une autre manifestation, jusqu'ici peu pratique en Europe, de cet esprit de voisinage. Les industriels et participants américains furent amenés à donner leur coopération au programme dans la mesure où ils voyaient dans les membres des missions des "voisins" et des collègues, des égaux. Ce point ne faisait pas difficulté: l'Amérique est une nation formée d'immigrants, souvent de fraîche date, qui ne tardèrent pas à surmonter leurs différences originelles et à accéder au mode de vie américain. La qualité d'étranger n'est donc pas un obstacle à l'admission dans la communauté américaine et l'assimilation ne fait pas de doute. Tout Américain connait par son ascendance ou ses amis, nombre d'étrangers qui sont devenus Américains, c'est à dire qui ont choisi de s'établir aux États-Unis et ont préféré à leur ancien mode de vie, celui de leur nouvelle existence. Chacun de ces choix confirme l'ensemble des citoyens dans la conviction que la civilisation américaine est excellente, et cette réassurance a une grande valeur dans un pays où l'opinion des autres et notamment celle de la majorité est tenue en si haute estime pour la formation du jugement individuel.

Les participants et hôtes américains identifièrent donc les visiteurs avec le stéréotype de l'étranger en quête d'américanisation, avec les nouveaux venus dans la communauté auxquels les anciens doivent un coup de main. La cordialité de leur accueil était fonction de cette identification.

L'aide donnée aux nouveaux venus, à titre de membres de la communauté, suppose que ceux-ci acceptent le statut de membres et les responsabilités concomitantes, c'est à dire qu'il y aura de leur part, le cas échéant, services et aide, qu'une réciprocité est instituée. Aussi les programmes d'assistance techniques furent ils dénommés programmes d'échanges.

En conséquence, la présentation qui était faite des visiteurs à leurs hôtes américains, insistait-elle sur leur caractère de voisins, éprouvés par la guerre, de membres de la communauté des nations amies avec lesquelles il importait de "partager" les progrès et la prospérité américaine. La bonne grâce de l'accueil dépendait de la mesure dans laquelle les organisateurs avaient su faire accepter cette représentation.

Toutes ces motivations, si claires et acceptables pour les Américains, ne s'inséraient guère dans les patterns européens et par conséquent étaient tenues par nombre de visiteurs comme inexistantes et impossibles, ou au mieux comme douteuses. Les affirmations qui en étaient faites paraissaient alors ou mensongères, ou énigmatiques. Et il convenait alors de rechercher la "vraie" motivation, compréhensible et acceptable, selon des patterns plus familiers.

Pour beaucoup d'Européens qui faisaient à l'occasion des missions le premier grand voyage de leur vie et avaient une conscience aiguë d'être loin de chez eux, de leurs familles, dépaysés, les États-Unis n'étaient pas un voisin, mais un pays extrêmement lointain et déroutant. Tant qu'un contact vraiment personnel n'était pas établi, ils étaient plus sensibles aux différences qu'aux ressemblances et par conséquent se sentaient étrangers, affectivement éloignés. Ces contacts personnels ne pouvaient résulter des rencontres dans les usines, bureaux et hôtels, mais se trouvaient habituellement établis lors de réceptions dans les familles qui furent instamment demandées. Or ces réceptions, faciles à organiser étant donné la grande hospitalité américaine, le furent pourtant rarement, car elles étaient tenues, pour purement "sociales" et d'agrément, et sans relation avec le but technique des voyages. En vérité, le succès des voyages en dépendit dans une part non petite.

Si la plupart des Européens ne pouvaient ramener les voyages d'assistance technique aux stéréotypes américains qui ne leur étaient pas familiers, ils étaient enclins à interpréter la situation suivant d'autres stéréotypes. L'un d'eux leur était particulièrement déplaisant, c'était celui de l'"assistance." Si l'aide entre égaux (et sauf dans les milieux agricoles) ne s'exerçait pas souvent en Europe, avant la guerre, elle était normalement rendue sans objet par la cohésion du groupe familial et l'aide intra-familiale. L'arrivée à l'âge adulte se marquait par la possibilité de pouvoir se passer de l'aide des autres, se suffire, et aborder les autres adultes sur un pied d'égalité. Dans le cas où l'aide du groupe familial avait été déficiente (ce qui était regardé dans les milieux de classe moyenne où se recrutent la plupart des membres des missions, comme un malheur et comme une tare), ou qu'elle faisait défaut dans un moment d'exceptionnelle nécessité, il fallait se tourner vers des bienfaiteurs, des œuvres, le gouvernement. Cette assistance frappait ses bénéficiaires d'un tel stigmate d'infériorité que des familles préféraient se priver que de solliciter une aide à laquelle elles avaient droit, mais qui les déclassait, et que le fait d'avoir obtenu une bourse d'étude était soigneusement dissimulé par les bénéficiaires. L'attitude publique sur tous ces points a beaucoup changé notamment depuis la guerre; il en reste pourtant des traces profondes et dans la mesure où les Européens pouvaient interpréter les voyages d'assistance technique comme une forme d'"assistance," ils se sentaient placés dans la position notoirement inférieure et humiliante d'assistés.

Un autre stéréotype s'offrait à leur choix: celui d'invités du gouvernement américain. Il les portait à s'attendre à certains types d'égards officiels qu'ils s'étonnaient de ne pas rencontrer. Par ailleurs, il les induisait à rechercher les motifs de l'organisation de ces voyages dans des raisons de prestige. Le précédent qu'ils évoquaient était celui des visites de propagande organisées au XVIIIme siècle en Russie par Catherine II. A cette occasion, les étrangers de passage se voyaient montrer des villages de figurants édifiés à leur intention par Potemkine. Les États-Unis tâchaient probablement d'éblouir ses invités par des moyens analogues et il était de leur honneur de savoir démêler le vrai du faux, de refuser créance à l'invraisemblable, statistiques sans doute truquées, existence evidemment mythique des "discount houses," etc. Dans les cas, d'ailleurs rares, où les affirmations américaines étaient tenues pour mensongères, les visiteurs avaient le sentiment d'être non seulement trompés, mais insultés sur un point où ils étaient fort chatouilleux: "On les croyait donc bien bêtes si on les supposait capables de croire que . . ."

Un deuxième groupe de difficultés vint du fait que les missions d'assistance technique se présentaient comme des voyages d'étude, et que l'enseignement et le savoir évoquent aux États-Unis et en Europe, des notions, attitudes et valeurs dissemblables.

L'enseignement et l'information, aux États-Unis, ne se distinguent pas par des frontières définies, et se présentent l'un et l'autre comme moyens de se donner des connaissances utiles, portant essentiellement sur des faits et des règles applicables. Le savoir n'est pas prisé pour lui-même, mais comme un moyen au service de l'action. Le statut de l'industriel, de l'homme d'affaire, de l'homme d'action est supérieur à celui du maître d'école ou du savant. L'homme qui sait, l'expert, est un auxiliaire et un subordonné du chef d'entreprise. Ce dernier ne prétend pas connaître tous les détails de son affaire et appelle, même devant des tiers, pour se renseigner, les spécialistes au courant. Par ailleurs dans le monde moderne et notamment dans le secteur du progrès technique, inventions et changements sont incessants. Il s'ensuit que le savoir n'est jamais complet ni définitif et doit être tenu à jour par des mises au courant continuelles, d'où le grand domaine de l'éducation des adultes, l'usage extensif des cours du soir, instituts, sessions de week-end ou d'été, "refresher courses," l'instauration de cours spéciaux pour "executives" dont l'âge moyen dépasse la quarantaine, l'érection d'un bâtiment donné par Kellog, sur le "campus" de l'Université de Michigan, à Lansing, dédié à "l'éducation continuante."

A la différence de tout ceci, l'éducation européenne de type classique se propose la formation de l'esprit et la connaissance de données premières, de grands principes de jugement. La connaissance des faits est épisodique et secondaire et se trouve, pense-t-on, facilement maîtrisée par les esprits bien formés. Tandis que les écoles américaines vérifient les connaissances de leurs élèves par des "objective tests," les écoles européennes donnent à leurs étudiants des dissertations. Les programmes de ces dernières préfèrent l'enseignement des matières générales aux spécialités, réservées aux écoles jugées de rang inférieur. D'un autre côté l'éducation est administrée pendant les années d'enfance et de jeunesse dont elle est une caractéristique propre. "Quitter les bancs de l'école" est entrer dans l'âge adulte. Vouloir, pour un adulte, "retourner à l'école," même figurativement, est sujet de moquerie, c'est déroger à son âge.

D'un autre côté, pour les Européens, la somme des connaissances d'un adulte ne cesse de s'accroître, mais non par des procédés scolaires: par l'effet de l'expérience et de la réflexion qui murit, intègre les éléments du savoir dans une synthèse personnelle. La supériorité d'âge comporte donc théoriquement une supériorité de savoir, non seulement quantitative, mais qualitative, la transmutation personnelle de l'appris. Aussi tandis qu'aux États-Unis, le jeune homme récemment sorti de l'Université où il a reçu le dernier état des questions de sa spécialité, est particulièrement recherché et apprécié, au détriment de l'homme plus âgé, bourré sans doute de connaissances dépassées et qui doit "désapprendre" (unlearn), l'Europe estime l'homme que la vie a façonné, et "a des idées personnelles," elle préfère la connaissance à l'action, honore écrivains, savants, professeurs plus que les industriels, l'être plus que l'avoir. D'où, sans doute, les gérontocraties européennes; d'où aussi, le savoir étant un élément important du statut, le prestige du maître. L'instituteur américain doit plaire à ses élèves et son activité auprès d'eux rentre dans la catégorie des services; mais le maître européen a autorité sur ses étudiants qui n'osent guère discuter avec lui en classe: il détient une position hiérarchique supérieure, celle qui revient de droit à l'enseignant vis à vis de l'enseigné. Jusqu'à présent, il était généralement considéré en Europe comme une impertinence grave de la part d'un enfant, d'un jeune, d'un subordonné, de "prétendre" en savoir davantage que son père, une personne notablement plus âgée, un supérieur. Et ceux-ci, le cas échéant, ne confessaient pas facilement leur ignorance, et affichaient parfois une compétence qu'ils n'avaient point.

Cependant la simple possession ou transmission de renseignements, d'informations ne conférait ni prestige, ni statut.

Ces concepts souvent mal définis, mais vivement sentis par les Européens, donnèrent à certains d'entre eux une forte ambivalence vis à vis de l'assistance technique. Dans la mesure où ils voyaient dans les missions l'occasion de récolter des renseignements, d'observer des montages ingénieux, des "trucs," ils en acceptaient le principe. Mais quand elles leur apparaissaient comme le canal d'un enseignement offert par les États-Unis aux pays européens, ils se rebiffaient avec raideur, puisque la relation hiérarchique enseignant-enseigné, les mettait automatiquement en posture inférieure. Leur réaction était d'autant plus vive que leur qualité peu ou prou officielle de représentants de leur patrie les rendait solidaires et responsables de l'honneur national, et qu'il s'agissait d'un jeune pays faisant la leçon à plus âgé que lui. Ils étaient également choqués qu'à vouloir les enseigner, on les "prit pour des enfants," et qu'on fit de questions de soi secondaires, et indignes du rang de matières enseignables, le sujet de cours ou de présentations.

Les organisateurs américains, quand ils s'aperçurent de ces réactions et en devinèrent le sens, s'attachèrent à les prévenir, vantèrent les gloires de l'Europe, les contributions de ses savants, ce qui fit fort bonne impression sur les "missionnaires." Mais il fallait aller plus loin. Les membres des missions sentaient fort justement que ce qui les irritait, les confondait ou les intriguait, suivant les cas, était plus qu'un détail obscur et sans importance, mais le symptôme de l'affrontement de cultures différentes, et qu'ils ne seraient vraiment satisfaits que lorsqu'ils auraient compris ce qui leur échappait, c'est à dire l'ensemble où ce détail s'insérerait à sa place, prendrait sa signification et sa valeur.

Les organisateurs comptèrent, pour surmonter les difficultés et réticences de leurs visiteurs, sur la multiplication des visites et des invités, ce qui était supposer que les Européens apprennent de la même façon que les Américains, par accumulation de faits. Et certes, cette accumulation joua un rôle important, permettant une sorte d'accoutumance par répétition. Mais il faut aux Européens, pour qu'ils se sentent vraiment à l'aise, une explication intelligible, une vue d'ensemble, une synthèse. Le temps dont ils disposaient (de 4 à 6 semaines habituellement) était beaucoup trop court pour qu'ils pussent se procurer par eux-mêmes les éléments de cette synthèse valable. Les Américains qu'ils rencontraient leur présentaient habituellement des inductions et non le système de déductions auxquels ils sont habitués, et qui leur est assimilable. Il se trouva fort heureusement, mais plus ou moins fortuitement, auprès de nombre de missions, des médiateurs culturels dont le rôle fut considérable encore que peu aperçu, car il se trouvait dissimulé par leurs fonctions officielles, généralement celles d'interprètes linguistiques. Leur connaissance intime des deux civilisations en présence leur permettait de comprendre l'une et de l'expliquer à l'autre en des termes, des modes de pensées qui lui fussent compréhensibles. Le succès des voyages d'étude a dépendu dans une très large mesure du rôle qu'ont spontanément assumé ces médiateurs culturels et de l'heureux hasard qui les a faits à la fois disponibles et préparés. Mais il serait sans doute imprudent de compter sur la constance du hasard.

Les missions d'assistance technique et les contacts culturels qui vont se multipliant offrent à l'anthropologie appliquée un magnifique champ de recherche et d'activité: notamment l'appréciation des difficultés psychologiques qu'ils présentent du fait des différences culturelles, l'étude du rôle central des médiateurs culturels et du type de préparation qu'il serait souhaitable de leur donner pour les former à leurs délicates fonctions.

Washington, D.C.

APPLIED ANTHROPOLOGY, COMMUNITY WELFARE, AND HUMAN CONSERVATION

Laura Thompson

We live in an age when anthropologists are often consulted about practical human problems. For example, industrial managers call in anthropologists to improve the human relations in their factories so that production and profits may benefit; hospital authorities are beginning to turn to anthropologists to solve organizational problems so that a more "therapeutic" atmosphere may be fostered for the benefit of patients and staff alike; teachers are finding, often to their surprise, that anthropologists can help them to achieve understanding and perspective on problems of growth and development which they encounter daily in the classroom.

The increasing demand that anthropologists assume these and many other relatively new roles attests to their effectiveness in helping to solve important practical problems involving human relations. It also indicates of course that a degree of maturity has been achieved at least in certain aspects of the science of man.

The examples mentioned above deal with the improvement of social relations in factories, hospitals, classrooms, etc., in other words, in so-called "small groups" within the larger community. Many case studies could be cited reporting success of applied anthropologists working to improve social relations in "small groups." By way of illustration I refer to a wealth of case material on "small group" research reported in *Applied Anthropology* and *Human Organization.*

It should be noted that the variables considered relevant to the solution of such types of problems are primarily facts concerning human relations such as behavior patterns, social institutions, certain personality traits, and certain attitudes, values, etc. Geographic and historical settings receive a minimum of emphasis, if any. In other words, the basic approach has been primarily sociological or social anthropological.

For purposes of this paper, let us assume that the applied anthropologist using a basically sociological approach has proven himself professionally competent in the area of improving human relations in "small groups." But there is another area of problem to which the anthropologist is often asked to apply his skills—namely, the improvement of human welfare in local communities, rather than in "small groups" within the community. I refer to projects in technical aid, public health, mental hygiene, education, administration, resources conservation, etc., in village communities and rural communities throughout the world. What may we say of our professional competence and skill in this more complex, broader-gauged area of problem?

The present paper is concerned with this question. My aim is to discuss briefly some methodological problems that confront the applied anthropologist who attempts to work as a scientist toward the improvement of human welfare in local communities rather than in so-called "small groups."

Various methodological approaches, singly or in combination, have been used by applied anthropologists concentrating on the community welfare problem. One of the most fruitful has emphasized what might be called the psychiatric or mental-hygiene approach. A good description of contributions made by anthropologists toward the solution of community-welfare problems may be found in *Cultural Patterns and Technical Change*, a manual prepared by the World Federation for Mental Health and edited by Margaret Mead (UNESCO, 1954). The report contains many important insights regarding various aspects of the community-welfare problem as well as regarding the problem as a whole. It suggests that almost any aspect of the community-welfare problem—technical aid, social services, education, hospital services, public health, etc.—may be benefited by good applied anthropology using a sound psychiatric approach. But the Mead report also reveals the limitations of the essentially individualistic approach of psychiatry and mental hygiene when an attempt is made to cope with group problems. This and many other studies of similar methodological frame suggest implicitly or explicitly that to solve the over-all community-welfare problem viewed as a whole rather than piecemeal we need an adequate basic methodological approach which, like the sociological, is keyed primarily to group problems rather than to individual problems.

As we are all aware, the sociological or social anthropological approach has also been used frequently by applied anthropologists dealing with community-welfare problems. Although obviously it overcomes the methodological difficulties mentioned above, in that it is keyed to the group rather than to the individual as the significant unit of research, and although it has proven successful in improving social relations in "small groups," it has revealed serious limitations when used in broad-gauged community-welfare projects.

A well-documented illustration of this point is found in my *Personality and Government*, the final report of the Indian Personality and Administration Research project (*Instituto Indigenista Interamericano*, Mexico, D.F., 1951). This long-range project was sponsored jointly by the U.S. Office of Indian Affairs, the University of Chicago's Committee on Human Development, and subsequently by the Society for Applied Anthropology. It was initiated by Indian Service administrators who asked the scientific staff for evaluation of past efforts and for help in improving the welfare of Indians living in reservation communities. Although psychological, psychiatric, social anthropological, and other field techniques were employed by the field workers on this project, from the methodological viewpoint the basic approach used by the staff during the early phase of the research was sociological. But as the analysis progressed it became increasingly clear that an essentially sociological approach, although indispensable in illuminating certain aspects of the over-all problem, was in itself inadequate to the solution of the community-welfare problem viewed as a complex whole through time rather than as a cross-section involving many separate or loosely related problems. While a sociological approach was found to be extremely useful in throwing light on the social-relations aspects of community-welfare, a genuine resolution of the complex over-all welfare problem necessitated the development of an understanding of much more than social relations in the thirteen community sample. It also necessitated understanding the dynamic relationship patterns and value orientations of the several human groups to their changing ecological webs-of-life, their changing geological bases and soil profiles, their changing climates, etc. Indeed, as field work and analysis probed deeper toward the heart of the community-welfare problem, the elemental facts of community existence appeared to represent significant variables which underlay community welfare viewed *in toto*. It was found necessary to achieve an understanding of each group's effective natural resources, including its human resources, and how the group related itself to these resources, e.g., what these natural resources were, how they were changing in time, what boundaries to them, if any, were recognized by the group, how they were used, and to what extent each resource complex, natural or human, was regarded as precious, expendable, or otherwise by the group. For example, was fertility of the soil to be conserved for posterity, was it to be used up rapidly as needed in the present moment, etc.? We found that we needed to understand the deep-seated attitudes of the group toward what were regarded as the realities and limitations of the effective environment—the sun, the earth, mountains, clouds, rainfall, the animals and plants and the other people which the group considered important to its life-way, as well as the patterns whereby such attitudes and values had been structured and institutionalized and had become a part of the round of daily living. Basic biological patterns, especially ecological relationships, were found to underlie and reinforce human relationship patterns to such an extent that, without a thorough and detailed understanding of the former, sociological facts tended to remain deceptively out of context, and their meaning in terms of the total problem tended to be significantly skewed or lost.

In the later phases of this project a basic approach similar to that successfully developed and used by applied ecologists in the field—in soil-conservation work, moisture control, sustained-yield forestry, range management, etc.—was employed as the major methodological tool whereby the findings from the several field techniques (sociological, social anthropological, psychological, psychiatric, ecological, etc.) were successfully integrated. Since an ecological approach is oriented to the natural community rather than to the individual as the intelligible field of concentration, along with a sociological approach it eliminates the difficulty regarding the psychiatric approach mentioned above. But an ecological conceptual frame differs crucially from a basically sociological frame in that ecology has been developed to cope not only with significant relationships between the several units within a community but also with significant relationships between the community's changing web-of-life and its changing effective environment. An ecological frame, translated in terms of the long-range community-welfare problem, is potentially equipped to handle not only the social relations or sociological dimensions of the human problem under consideration but the environmental relations as well. Thus it provides an adequate multidimensional tool of sufficient dynamic and structural complexity to meet the methodological need posed by the long-range community-welfare problem viewed in broad environmental perspective.

When we analyse the findings of the Indian Personality and Administration project and others of similar broad scope, the importance of the applied ecologist's basic approach becomes apparent for the applied anthropologist who attempts to work out complex community-welfare problems as a whole through time rather than momentarily piece by piece. Indeed the variables relevant to the solution of such over-all community-welfare problems appear to include sociological and psychological facts concerning human relations as a matter of course, but also and *essentially* they appear to include basic ecological and physical facts concerning the dynamic, time-depth relations between the human community and its effective environment (including other human groups) too often neglected or underplayed by social scientists. To solve such problems successfully, from the scientific viewpoint, the anthropologist's view of the relevant variables has to be extended beyond the limited theoretical frame which has generally been found operationally adequate by social scientists in solving sociological problems, to a more extended dynamic and multi-dimensional frame encompassing relevant data from ecology and the earth sciences as well.

Thus it appears that one of the main differences between problems in applied anthropology which focus on the immediate improvement of human relations in "small groups" and those centering on the sustained improvement of human welfare viewed as a long-term total problem in the community has to do with the selection of relevant variables. In attempting to solve problems of the former type, as stated above, applied anthropologists have had considerable success by assuming operationally that mainly facts concerning human relations were relevant. On the other hand, when a similar basically sociological frame of reference has been used in an attempt to solve a local community's complex welfare problem viewed as a whole in time and space dimensions it has been found inadequate, whereas that of applied ecology has supplied the need.

These researches have had other significant results. For example, it seems that by experimenting with several approaches toward the community-welfare problem, used singly or in combination—for example, the sociological, the psychiatric, the ecological, as well as others which will not be discussed here since time is limited—applied anthropologists and other field workers in many parts of the world have begun to envisage a new concept of community-welfare.

What is this new emerging concept of community-welfare?

It may be described as a long-range view of human welfare in its manifold dynamic expressions as a changing total problem in the context of the changing local community setting rather than as a series of separate problems such as unemployment, social security, medical and public health problems, mental illness, alcoholism, drug addiction, schools and adult education, social services, crime and delinquency, natural-resources conservation, agricultural extension, range management, etc. It emphasizes the dynamic relatedness through time and in space of the many aspects of individual and group living heretofore treated as separate welfare problems. The emerging concept of community-welfare underlines the wholeness of man as an evolving social animal, and the wholeness of the evolving local community composed of groups of human beings uniquely related to one another through biological, social, and cultural ties, and uniquely related to the changing local setting with its unique combination of natural resources, uniquely regarded and used.

We have begun seriously to ask a new significant question: According to the findings of modern science what would be a valid concept of a healthy community from the positive therapeutic point of view?

Great progress has been made in our understanding of the healthy human individual from the medical, the psychiatric, and the psychosomatic viewpoints, but since man is social in nature our answers to questions of health and welfare are incomplete unless they encompass social facts. Recent experiments in community-welfare research indicate the direction in which a scientifically acceptable answer to the question may some day be found. What are scientifically relevant indices of community health?

Recent researches suggest that an adequate definition of the healthy community will not consist of a sum-total of points scored along a number of unrelated scales—censuses, health scales, standard-of-living indices, psychological-test findings, etc. They suggest that a scientifically relevant concept of the healthy community, when it finally emerges, will be phrased, rather, in terms of an optimum balance of interrelated relevant factors, a homeostatic type of balance, to borrow another concept from ecology, self-regulating and self-perpetuating, in which whole evolving human beings will be considered in transactional relationship with one another and with an organized evolving community viewed as a whole in effective changing environmental setting.

The final point I wish to make is that the emerging concept of community-welfare reinforces the view of man as an evolving social organism who may find fulfillment as a human being only through healthy community living. Man is seen as an integral part of an evolving web-of-life, as one species dominant in an organic community composed of many species of animals, plants, microorganisms, etc., transacting with one another and with a changing geographic setting specific to a unique area and region of the earth.

In this connection applied ecology, in the concept of resources conservation, supplies another "lead" which has been found useful by applied anthropologists working on community-welfare problems. Sustained-yield forestry may be regarded as a method of forest cultivation by which all parts of the forest, viewed as an ecological community, are subordinated to the concept of optimum, long-range production of certain types of healthy timber. In cutting, pruning and lumbering the sustained health and yield of the forest as a whole is the primary consideration. A similar approach underlies soil and moisture conservation practices which have proven so successful in preserving and increasing the long-range fertility and potential yield of the more than 2,500 soil conservation districts in the U.S.A. and which have been spreading to many parts of the world in the last twenty years.

Extending the resources-conservation idea of the applied ecologists to cover *human* resources as well as natural resources, we may have a conceptual tool of considerable promise for community-welfare workers. The healthy community may be viewed as one wherein optimal, long-range community living is sought *not* for the greatest possible number of human beings, but rather for the optimal number which in the long run will allow and foster a healthy community life-way in the specific area and under the specific conditions being considered. Within this context, for example, population control becomes a community responsibility, rather than a strictly private, personal or family concern. Similarly, the world's "runaway" population problem becomes a local community-welfare problem, an integral part of the long-range total health picture of the community as a whole.

In this conceptual frame, community health demands, as Harry Overstreet, Lawrence Frank, and others have emphasized, that the social institutions and the accepted behavior patterns of the community pass a test of their fitness for man as an evolving social organism in community context—and not vice versa. It demands that culture be recognized explicitly as man-made and as constantly in process of being revamped and recreated, a process which may be directed by the group toward greater fitness for man's healthy community living.

Kinsey's work, whatever its faults and virtues, was designed to provide basic information whereby statute law and custom in a fundamental area of living, namely that impinging on sex practices, marriage customs, etc., might be tested against man's actual needs, habits and aspirations as revealed by systematic research. This is just one example among many which might be cited of recent and current research designed to provide the kind of information needed toward the development of healthy communities, in theory and in practice.

It is not the role of the scientist to try to change society. Only society can change itself. But it is the role of the scientist to ask relevant questions and to provide basic facts, concepts, and perspectives which may illuminate man's perennial problems as they are expressed in contemporary forms, and to enhance man's understanding of himself and his potentialities. A certain responsibility for this role falls directly on the applied anthropologist who works on community-welfare problems.

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SECTION VIII

LINGUISTICS

THE SOUTHWEST PROJECT IN COMPARATIVE PSYCHOLINGUISTICS: A PRELIMINARY REPORT

Joseph B. Casagrande

The research on which I wish to report to you has the rather formidable title of "The Southwest Project in Comparative Psycholinguistics," "Southwest" because the research site was the Southwestern United States, "Comparative" because coordinate studies were undertaken among six Southwestern groups partaking of a variety of cultural traditions and speaking diverse languages—the Navaho, Hopi, Zuni, Spanish-American, Hopi-Tewa and the related Tewa-speaking people of Santa Clara Pueblo. In addition, data were collected on English-speaking persons from these groups, and on native white speakers of English, which together constitute a seventh comparative group. Finally, the project title includes the term "Psycholinguistics" because a major objective of the research is to test the general hypothesis that the structure of a language (specifically, its lexical, grammatical and phonetic features) will influence certain non-linguistic behavior of its speakers, e.g., cognitive processes such as perception, conceptualization, and problem-solving.

Planned and sponsored by the Social Science Research Council's Committee on Linguistics and Psychology,[1] the Southwest Project was launched in the early summer of 1955, with the support of the Carnegie Corporation of New York. John B. Carroll, a former chairman of the committee and associate professor of education at Harvard University, was appointed to serve as director of the project. During July and August of 1955 a seminar or workshop was held at the University of New Mexico in Albuquerque for all project personnel. This first phase of the project had a general research training and planning objective. Thus, the research staff received brief, but intensive instruction in disciplines and techniques relevant to the research; for example, psychologist staff members were trained in linguistics, while anthropologist and linguist staff members received instruction in relevant psychological concepts and techniques. Linguistic work was begun by all staff members with informants speaking the languages involved in the research, and through lectures, independent reading and field trips some familiarity was gained with the cultures being studied and with the varying field situations in anticipation of the experimental work to be begun the following summer. Perhaps most importantly, a series of experimental and observational procedures were designed and adapted for use in the field among the various groups being studied, and many of these procedures were pretested. Field teams, each consisting of at least one psychologist and one linguist or anthropologist, were formed and assigned to work respectively with the Navaho, Hopi, Zuni, Spanish-American, and Hopi-Tewa and Santa Clara Tewa groups.

As a guide to field workers and to insure greater comparability of data, a field manual outlining the experimental and observational procedures developed during the summer was compiled during the 1955–56 academic year by Doctors

Carroll and Susan Ervin of the project staff. This 156-page manual, together with supplementary materials prepared since its issue, describes about thirty field procedures designed to test comparatively a range of both general and specific hypotheses relating features of linguistic structure to non-linguistic phenomena at both the cultural and individual psychological levels. After revision in the light of the field experience of last summer, it is hoped that the manual can be made available to persons wishing to undertake psycholinguistic research among various other groups.

As has already been suggested, special attention is being given in the research to the empirical testing by experimental methods of the familiar Sapir-Whorf "linguistic relativity hypothesis"—the proposition which states that a language channels the psychological processes of its speakers and subtly but significantly shapes their view of the world.[2] However, the testing of this hypothesis is by no means the sole objective of the research. Indeed, such a global proposition cannot be tested *in toto*, nor can its limits be easily sounded; rather, it must necessarily be tested piecemeal. Should our results be affirmative or negative, they cannot be construed as either confirming or refuting the "linguistic relativity hypothesis" as a whole, but only in part.

On the assumption that an important component of culture is a *structure of shared meanings*, and that this structure is represented in symbol systems, one of which is language, procedures such as Charles E. Osgood's semantic differential,[3] tests of the generality of synesthesia and metaphor, and word association which are designed to tap these patterns of shared meanings, have been adapted and developed for cross-cultural use. It should be stressed that the concern is as much with the *generality* of psycholinguistic phenomena as with unique relationships between linguistic and non-linguistic behavior in particular languages and cultures. One can be only too easily impressed with the differences among languages and with the putative effects of these differences on their speakers; what seems to me to be even more impressive is the fact that languages everywhere are so very much alike.

Since many of the persons who served as subjects in the experimental studies spoke more than one language, particular attention was also given to the development of adequate measures of bilingualism in order that the effect of this variable could be assessed. Finally, it may be mentioned that a portion of the research is along more traditional ethnolinguistic lines. Thus, the writer has been doing some work on the range of reference of anatomical terms in Navaho, which he hopes ultimately to compare with similar terms in English.

The second, field-research phase of the Southwest Project was begun in June 1956. A total of seventeen social scientists were engaged in field work among the six Southwestern groups represented in the study. All but two of the research staff hold the doctorate in anthropology, linguistics or psychology, and most have been trained in two of the relevant disciplines and have a special interest in research on language behavior. We have all only very recently returned from the field, the writer having left the Navaho Reservation nine days ago. The data are in, but they have not yet been analyzed. At this stage one has only impressions of trends in the data, not firm research findings. It is somewhat presumptuous to appear before you and describe this rather ambitious undertaking in such general terms and to give you merely intimations of research results, and for this your indulgence is asked. I could not forego the opportunity to acquaint an international congress such as this with at least the general outlines of a research program which I feel is pioneering in its attempt to apply experimental methods in the field, across languages and across cultures, to a crucial set of problems.

It would be tedious, nor does space permit me to describe for you in detail the many field procedures that were undertaken or to give you the rationale for each. Suffice it to say that about half the 30-odd experiments were conducted among two or more of the groups represented in the study, several being run among all groups. Many of the remainder were either adapted to one group or of particular interest to a single research worker. Moreover, resources of time, energy and personnel did not permit the administration of all possible procedures to all of the six groups, nor did the general research design call for such a strategy. However, on the basis of reports from the various teams, it appears that sufficient subjects have been obtained for all groups to make possible statistical analysis of the data. Detailed reports on various aspects of the research will be published during the coming months, and present plans call for the preparation of a general monographic report on the project as a whole.

So much for a broad over-view of the larger project. I should like now, as an exemplification of the kind of research with which we have been concerned, to tell you in some detail about a study I conducted among Navaho children.

As many of you know, it is obligatory in the Navaho language to use a particular one of a set of verbal forms according to the shape or some other essential attribute of the object about which one is speaking. Thus, if I ask you in Navaho to hand me an object, I must select the appropriate verb stem depending on the nature of the object. If it is a long, flexible object such as a piece of string, I must say, "šańleh"; if it is a long, rigid object such as a stick, I must say, "šańtin"; if it is a flat, flexible material such as paper or cloth, I must say, "šańilcos," and so on. Because of this obligatory categorization of objects in Navaho, it seemed to the writer that Navaho-speaking children would learn to discriminate the formal attributes of objects at an earlier age than their English-speaking compeers. The finding of American (and European) child or developmental psychologists that children tend first to distinguish objects on the basis of size and color might, it was argued, at least at the level of verbal facility in dealing with these dimensions, be partly an artifact of the particular language they used. The hypothesis was, then, that this feature of the Navaho language would affect the relative order of emergence of such concepts as color, size, shape or form, and number in the Navaho-speaking child as compared with English-speaking children of the same age, and that Navaho-speaking children would be more inclined to perceive formal similarities between objects than would their English-speaking age-mates. This hypothesis was tested using a variety of experimental materials and several different procedures.

To put the child at ease and to accustom him to the experimental situation, as well as to gauge his ability to perform a simple task, he was first asked to match objects shown to him with each of a set of objects arrayed before him. This matching task was called Procedure I. Five sets were used in sequence. Set "A" consisted of six familiar, "functional" objects, each of which takes a different verb form in Navaho; these were a washcloth, a spoon, a piece of string, a bunch of wool, a wooden block and a little pile of corn. Set "B" consisted of a series of less familiar, "non-functional" objects, each corresponding to a member of Set "A" and each also taking a different verb stem; these were a piece of aluminium foil, a steel bit, a section of steel spring, a bit of steel wool, a metal cupcake mold, and a little heap of split lead shot. Set "C" consisted of a series of five variously shaped blue wooden blocks of comparable size:

a square, sphere, cylinder, pyramid and oblong, all of which in Navaho take the same verb stem. Set "D" consisted of five variously colored square blocks all the same size: white, black, red, yellow and blue. Set "E" consisted of large, small and medium-sized blocks painted blue and of the same basic shapes used in Set "C". Thus, each set of blocks varied in only one of the crucial dimensions in which I was interested: Sets "A" and "B" in the verbal form taken, Set "C" with respect to shape, Set "D" with respect to color, and Set "E" with respect to size. This matching task was easily accomplished by all children and they regarded it as a pleasant sort of game.

The second procedure involved the use of the same five sets of objects with the addition of a sixth set of small variously shaped blocks which were used to test the child's comprehension of the terms for the numbers 1 through 5. This time the child was given what was called a "minimal verbal cue" and instructed to hand the experimenter each object in the set, or, in the case of the sixth set (Set "F"), the appropriate number of objects, as they were designated. The verbal instructions were phrased so that only a single word which carried the essential information required to select the proper object varied within the same linguistic frame. The test was scored by simply noting the correct and incorrect choices on a tally sheet.

In the actual conduct of the experiment a third procedure was interposed between Procedures I and II and repeated again at the end of the experimental session after an interval of from 30 to 60 minutes to secure reliability data. Ten pairs of objects were used, each of which differed significantly in two dimensions, e.g., according to color/size, color/shape, size/shape, color/verb form, shape/verb form. These pairs of objects, called "Ambiguous Sets," were shown to the child one pair at a time. The child was then shown a third object similar to each member of the ambiguous set in only one of the two relevant dimensions, but of course matching neither, and asked to tell the experimenter which of the pair went best with object shown to him. For example, one pair consisted of a square white block and a blue pyramidal block of comparable size. The child was then shown a square blue block and could select one of the pair on the basis of either shape or color. Another pair consisted of a yellow stick and a piece of blue rope of comparable size. In this instance the child was shown a yellow rope and the basis of his choice could be either color again or material *and* verb stem, since different verbal forms are used for a length of rope and a stick. It might be mentioned that the children were not at all baffled by the ambiguity inherent in the task; their choices in whatever direction were invariably made with little or no hesitation.

A fourth and last procedure was used, but in a second experimental session and employing some children who had not participated in the first session. The same five sets of objects described above and an additional set of 55 pictures of commonplace things comprised the experimental materials. In this procedure both the child and its mother served as subjects. As before, the sets, including the pictures, were arrayed before both mother and child, but in this case only the mother was shown, one at a time, objects identical with those in the various sets, and she was instructed simply to ask her child to hand her the one in the set that was like the one shown to her. These sessions were tape-recorded for later transcription and analysis of the linguistic materials. This phase of the study had a twofold purpose: to collect under controlled conditions, from a sample of mothers and their children of various ages, a corpus that might yield information about "mothers' language"; and to see what characteristics of objects were singled out by mothers in describing these various objects for their children so that they could make the correct choices. An incidental hypothesis was that the younger the child, the greater would be the redundancy in the mother's instructions.

The testing was done for the most part in the hogans with the aid of an interpreter, and usually in the presence of parents, grandparents, older siblings and other interested and very curious relatives. The subjects were 140 Navaho children ranging in age from about 3 to about 10. Grouping three- and four-year olds and nine- and ten-year olds together, since differences in age at these extremes apparently do not significantly affect the results, these 140 subjects were about equally distributed among the various age brackets so that there are at least twenty subjects in each age interval.

As was mentioned above, the degree of bilingualism, or even multilingualism, is an important factor among all the groups figuring in the Southwest Project. In order to exercise some control over this variable, it was necessary to have a measure of bilingualism among the 140 Navaho children. This was obtained through the use of a simple test and through brief interviews with the child and the parents or other close relatives to secure background information on such relevant factors as schooling, language usually spoken at home, language usually spoken with playmates and siblings, etc. The bilingualism test consisted of two parts, a *decoding* task and an *encoding* task. The set of 55 cards picturing commonplace objects which was mentioned above in connection with the fourth procedure was divided into two decks, A and B. In the decoding task, the decks were arrayed successively before the child who was asked to point to the object named, using Navaho for deck A and English for deck B. In the encoding task the decks were switched and the child asked to name the pictures serially, deck B in Navaho and deck A in English. Since younger children frequently were not able to distinguish which of the two languages they were in fact using in naming the pictures, they were in most cases, in the encoding task, simply asked to name the pictures, and responded in English or Navaho as they chose. Later in the session the child was asked to recall the names of the pictures he had seen earlier in order to determine which language would predominate in the names recalled.

On the basis of the bilingualism test, the background information secured and the actual performance of the child on the experiment (the experiment being conducted in Navaho or, with the appropriate modifications in the instructions, in English as indicated), it should be possible to break down the sample of 140 subjects into five groups: monolingual in Navaho, Navaho predominate, balanced bilingual, English predominate, and monolingual in English. By design, subjects were impressionistically selected so that they would be fairly evenly distributed among these five groups.

What of results? It must be stated emphatically that at this stage the results are impressionistic and highly tentative and are not statistically confirmed. However, it is gratifying to report that the trend of the findings appears to be in the predicted direction. Navaho-speaking children, i.e., Navaho monolinguals and Navaho-predominate children, seem to be more alert to the essentially formal characteristics of objects than their English-speaking opposites. While this finding seems to emerge from several of the various procedures used in the study, it is perhaps clearest in Procedure III which, it will be recalled, involves the use of ten pairs of ambiguous objects. Navaho-speaking children of all ages, it appears, tend to make a greater number of "Navaho choices" (form and/or verb stem over color or size), than do English-speaking youngsters of like ages. The difference in the proportion of "Navaho choices" seems to be most marked at the younger age levels; using age and number of "Navaho choices" as the coordinates, the curves which one may imaginatively plot tend to converge as one goes up the age scale.

Should the tentative results of this study among Navaho children be sustained by further analysis, a small contribution will be made to the larger task of putting the "linguistic relativity hypothesis" to a rigorous empirical test. But whatever the results of this and the many other component studies in the Southwest Project, the writer shall remain convinced that this is a highly significant research area richly deserving of further work.

Social Science Research Council,
Washington, D.C.

Notes

1. Present members of the Committee on Linguistics and Psychology are James J. Jenkins, University of Minnesota, chairman; John B. Carroll, Harvard University; Alvin M. Liberman, University of Connecticut; Floyd G. Lounsbury, Yale University; Charles E. Osgood, University of Illinois; Thomas A. Sebeok, Indiana University; and Rulon S. Wells, Yale University. Past members are Joseph H. Greenberg of Columbia University and George A. Miller of Harvard University. Joseph B. Casagrande of the Social Science Research Council has served as staff for the committee since its establishment in October, 1952.

2. John B. Carrol, editor, *Language, Thought and Reality: Selected Writings of Benjamin Lee Whorf*, John Wiley and Sons, New York, and the Technology Press, Cambridge, Mass., 1956.

3. Charles E. Osgood, "The Nature and Measurement of Meaning," *Psychological Bulletin*, 49: 197–237, 1952.

THE URBANIZATION OF THE GUARANÍ LANGUAGE—A PROBLEM IN LANGUAGE AND CULTURE

Paul L. Garvin and Madeleine Mathiot[1]

This paper is based on the assumption that Redfield's concepts of folk and urban[2] are applicable to language as well as culture. The linguistic equivalent of the distinction between folk cultures and urban cultures is the differentiation made by the scholars of the Linguistic Circle of Prague and others between folk speech and the standard language,[3] which we here tentatively define as a condified form of a language, accepted by and serving as a model to, a larger speech community. The Prague School has formulated a set of criteria for differentiating a standard language from folk speech, the latter of which, conversely, is characterized by the absence of these criteria.

These criteria are such that they presuppose the existence of an urban culture in the speech community using, or aspiring to use, a standard language. Consequently, we may consider a standard language a major linguistic correlate of an urban culture, and we may furthermore consider the degree of language standardization in this technical sense a measure of the urbanization of the culture of the speakers.

Conversely, since folk speech has been defined negatively, a low degree of standardization or its absence, is here proposed as one possible diagnostic criterion of a near-folk or completely folk culture.

There are two possible scales of language standardization that can be applied here. In cross-cultural terms, different standard languages can be compared as to the degree to which they meet the formulated criteria, and one language can then be rated as more or less highly standardized than another, just as one culture can be called more urban or more folk than another. In intra-cultural terms, different segments of a speech community can be compared as to the degree to which the standard language has penetrated them, just as different subcultures of the same culture can be compared in terms of different degrees of penetration by urban elements.

This paper will be concerned with a concrete case of language standardization. The authors believe that by presenting some of the differential criteria for a standard language and applying them to their case, they can contribute to a further specification of the concept of urban, that is, non-folk culture.

We have chosen the recent ethnolinguistic development of Guaraní in Paraguay as our test case.

Two languages are spoken in Paraguay: Guaraní and Spanish. In rural areas, Guaraní is spoken almost exclusively. In the Asunción metropolitan area which includes a large percentage of the country's population, both Guaraní and Spanish are used. Traditionally, Spanish has been the official language and the language taught in the schools, but in recent years there has been a developing movement in the metropolitan area to give Guaraní equal status with Spanish, which Paraguayans call the Guaraní renascence. This movement exhibits certain significant parallels with the nationalistic movements of the post-Enlightenment period of Europe (late 18th and early 19th centuries) to put some of the "lesser" languages on a par with the "great" languages. Unlike much of the European development, however, the desire in Paraguay is not to eliminate the "great" language, Spanish, as a competitor, but to have Guaraní and Spanish coexist as equals.

In view of the parallelism referred to above, the criteria developed by the Prague School in discussing the formation of the modern Czech standard language in competition with German, supplemented by some more recent thinking,[4] are considered by the authors to be applicable to the recent development of Guaraní in competition with Spanish, and we propose the hypothesis that there is now in the process of formation a Guaraní standard language as part of an emergent bilingual urban culture.

To formulate our hypothesis in detail, and to prepare the ground for a procedure for its verification, we shall set forth the criteria for a standard language, and relate them to Guaraní data to the extent allowed by preliminary research at a distance.

We are proposing three sets of differentiative criteria for a standard language: (1) the intrinsic properties of a standard language, (2) the functions of a standard language within the culture of the speech community, (3) the attitudes of the speech community towards the standard language.

1. PROPERTIES OF A STANDARD LANGUAGE

We shall here consider two differential properties of a standard language: flexible stability as originally stated by Vilém Mathesius,[5] and intellectualization as originally stated by Bohuslav Havránek.[6] Both of these properties are gradual and allow quantitative comparison.

1.1. *Flexible Stability*. This is by Mathesius discussed as an ideal property: a standard language, in order to function efficiently, must be stabilized by appropriate codification; it must at the same time be flexible enough in its codification to allow for modification in line with culture change.

There are two things involved in codification: (1) the construction of a codified norm, contained in formal grammars and dictionaries; (2) the enforcement of the norm by control over speech and writing habits through orthoepy and orthography. The construction of the norm is entrusted to a condifying agency or agencies, the enforcement of the norm is achieved through the schools.

The flexibility of the norm is achieved by including in the normative code the necessary apparatus for modification and expansion, which includes provisions both for a systematic expansion of the lexicon, and an equally systematic expansion of stylistic and syntactic possibilities. This is the responsibility of the codifying agency or agencies.

In the case of Guaraní, the codifying agency is the recently founded Academía de Cultura Guaraní. Urban Paraguayans look upon the Academy as the final authority in language matters, to whom the language problem has been entrusted. The Paraguayan government has recently recognized the status of the Academy and accorded it a subsidy. The Academy is at present engaged in the preparation of normative orthographic, grammatical and lexical materials preparatory to an expected and hoped-for introduction of the teaching of Guaraní in the schools.

In these efforts, the Academía de Cultura Guaraní is continuing a normative tradition established by the Jesuit fathers of the 16th century (in their work on Língua Geral), and resumed informally during the Chaco War, when a military terminology was evolved to allow the use of Guaraní for communications understood by Paraguayans only.

In terms of the requirement of flexible stability, the revival of the interest in the normalization of Guaraní has not yet led to the achievement of this objective. But the conditions have been created for ultimately meeting this requirement, and there is a strong desire and expectation among Paraguayans to see it met.

1.2. *Intellectualization.* Havránek defines the intellectualization of a standard language as "its adaptation to the goal of making possible precise and rigorous, if necessary abstract, statements,"[7] in other words, a tendency towards increasingly more definite and accurate expression. This tendency "affects primarily the lexical, and in part the grammatical, structure."[8]

In the lexicon, intellectualization manifests itself by increased terminological precision achieved by the development of more clearly differentiated terms, as well as an increase in abstract and generic terms.

In grammar, intellectualization manifests itself by the development of word formation techniques and of syntactic devices allowing for the construction of elaborate, yet tightly knit, compound sentences, as well as the tendency to eliminate elliptic modes of expression by requiring complete constructions.

In essence, then, intellectualization consists in a tendency towards greater relational systematization and explicitness of statement. This is by Havránek summarized in a three-step scale of intellectualization, leading from simple intellegibility via definiteness to accuracy, to which correspond a conversational, workaday technical, and scientific functional dialects, respectively.[9]

Whereas folk speech is limited to the conversational and some phases of the workaday technical dialects, all three functional dialects are represented, at least as an ideal, in a standard language.

The degree of intellectualization of Guaraní remains to be tested. While there is a strong awareness on the part of our informants of the requirement of flexible stability, there is no comparable awareness of the requirement of intellectualization, beyond the expectation that Guaraní should develop into a language in which anything can be expressed adequately.

Our informants have made emphatic claims as to the precision and abundance of Guaraní terminology in certain limited areas. These claims will have to be checked. The work of the Guaraní Academy in reference to both terminology and syntax will have to be investigated and evaluated. No conclusions in this respect can be reached without a detailed linguistic analysis of Guaraní.

The entire question is thus still wide open.

2. FUNCTIONS OF A STANDARD LANGUAGE

We shall discuss three symbolic functions and one objective function of the standard language. The three symbolic functions are: (1) the unifying function, (2) the separatist function, (3) the prestige function; the objective function is (4) the frame-of-reference function.

2.1. *Unifying Function.* A standard language serves as a link between speakers of different dialects of the same language, and thus contributes to uniting them into a single speech community. A consequence of this is an identification of the individual speaker with the larger language community, in addition to or instead of, the smaller dialect community.

In the case of Guaraní, not enough is known about the dialect situation to evaluate whether the Asunción form of speech serves as an interdialectal lingua franca or not. In terms of the group identification under the unifying function the situation is, however, clearcut: Paraguayans think of themselves as speakers of Guaraní, and not as speakers of any of its dialects; they even go as far as to deny the existence of dialect differences within Paraguayan Guaraní.

2.2. *Separatist Function.* Whereas the unifying function opposes the standard language to the dialects, the separatist function opposes a standard language to other languages as a separate entity rather than a subdivision of a larger entity. It thus can serve as a powerful symbol of separate national identity, and the individual's identification with his language community is then no longer a matter of course but becomes highly emotionally charged.

This is the case with Paraguayans. Guaraní is what makes them into a distinct Paraguayan nation, rather than just another group of South Americans. To all of them, those who speak Guaraní are fellow Paraguayans, those who speak only Spanish—although they may live in Paraguay—are not; they are foreigners and they are called "gringos." Even Paraguayans who speak mainly Spanish at home will speak Guaraní to each other when they meet abroad because, as one of our informants put it, "nos acerca más de nuestra tierra."

The identification is with the language and not with Indian ancestry. Immigrants, in order to be accepted as Paraguayans, will learn Guaraní. As Justo Pastor Penítez, a leading Paraguayan historian and writer, puts it: "La iniciación se realiza por el idioma guaraní, vehículo de la identificación nacional,"[10] because "hablar guaraní es ser dos veces paraguayo."[11] Thus the President of the Republic, in spite of his German name, is "paraguayo," because he speaks Guaraní.

2.3. *Prestige Function.* There is prestige attached to the possession of a standard language; one of the ways of achieving equality with an admired high-prestige nationality is to make one's own language "as good as theirs," which in our terms means bringing it closer to the ideal properties of a standard language.

Making of Guaraní a "lengua de cultura" like Spanish is one of the major motivations of the work of the Academía de Cultura Guaraní. Although the achievement of a Guaraní lengua de cultura is so far only an ideal, Paraguayans take great national pride in being the only American nation possessing a language all of their own that is capable of such a development, and they think of themselves as a model for the other South American nations in their quest for national individualization. "La misión histórica que el Paraguay está destinado a cumplir en América . . . ," says Benítez, "es de dar algo propiamente americano, un destello del alma del Nuevo Mundo."[12]

The prestige function has thus been transferred from the possession of a functioning standard language to the possession of a potential standard language. This transfer of the prestige function has become possible because the functioning standard languages of South America, Spanish and Portuguese, are shared by several national units and thus are not capable of carrying the separatist function and serving as a vehicle of nationalist symbolism.

2.4. *Frame-of-Reference Function.* The standard language serves as a frame of reference for speech usage in general by providing a condified norm that constitutes a yardstick for correctness. Individual speakers and groups of speakers are then judged by their fellows in terms of their observance of this yardstick.

The standard language furthermore serves as a frame of reference for the manifestation of the esthetic function in language,[13] which by the Prague School is defined as the property of speech forms to attract attention primarily to themselves rather than to the message they convey. The esthetic function so conceived appears not only in literature and poetry, but also in humor, advertising, and any conspicuous linguistic usage in general. In a standard language community, routine standard usage is the culturally expected, and deviations from this usage have esthetic function in the above sense, since their cultural unexpectedness attracts attention irrespective of content. Thus, the standard language is a frame of reference for the esthetic function.

In the case of Guaraní, the desirability of a frame of reference for correct speech is strongly felt, but the frame-of-reference function in this regard exists as yet only potentially.

The situation with regard to the esthetic frame-of-reference function is not clear-cut at all. On the one hand, Paraguayans claim that Guaraní is "marvelous for poetry and humor," which would indicate a strong occurrence of the esthetic function. On the other hand, whether this esthetic function manifests itself against the background of the incipient codified norm, against the background of informal folk usage, or even against the background of Spanish, is as yet an open question.

The only known factor in this problem is that present-day Guaraní poetry, of which there is a good deal, follows a European rather than a folkloric Indian esthetic canon as to its form, although the motifs are often aboriginal folkloric.

3. ATTITUDES TOWARDS A STANDARD LANGUAGE

The functions of a standard language discussed above give rise to a set of cultural attitudes towards the standard: the unifying and separatist functions lead to an attitude of language loyalty, the prestige function arouses an attitude of pride, and the frame-of-reference function brings about an attitude of awareness of the norm.

Language loyalty and pride are closely similar positive attitudes; we differentiate between them by assigning the intellectual and nationalistic attitudes to language loyalty, and the personal emotional attachments to pride.

3.1. *Language Loyalty.* This is the name given by Uriel Weinreich[14] to the desire of a speech community to retain its language and, if necessary, to defend it against foreign encroachment. Although language loyalty may be given to a form of folk speech, it becomes highly organized and articulate when it is given to a standard language, especially one that has not yet become sufficiently stabilized and generally recognized. Then language loyalty commonly manifests itself in attempts to justify the incipient standard language and to prove its worth.

This is dramatically true in Paraguay, as illustrated by Guillermo Tell Bertoni's comments quoted by Robustiano Vera in *La Defensa de la Lengua Guaraní*:[15] "La rica lengua del Paraguay puede disputar un puesto entre las lenguas cultas y dignas de un país civilizado." Guaraní is worthy of preservation and improvement because, as Justo Pastor Benítez says, "el guaraní tiene tradición viva," it expresses "toda la gama del alma de una raza que vivía en contacto íntimo con la naturaleza."[16] Robustiano Vera attempts to prove the worth of Guaraní by attributing a high-prestige ancestry to it: "Hay muchas voces guaraníes que no sólo son análogas, sino idénticas a sus similares egipcia, griega y sanscrita . . . gracias a la filología comparada . . . quizás hallemos una primitiva lengua hablada por nuestra especie en una edad ignota."[17] Eloy Farina Nuñez in *El Idioma Guaraní*[18] sees the value of Guaraní in the understanding which its aboriginal character gives us of ancient cultures, including the Greek, and "un instrumento que, como el guaraní, en vez de alejarnos, nos aproxima mas de la intimidad de la cultura helénica, hasta ponernos en contacto con el misterio de sus mitos y el milagro de su sensibilidad poética, bien merece la atención de los estudiosos del Nuevo Mundo."

Guaraní is, however, not merely worthy of improvement but capable of it. In Moisés S. Bertoni's words, "es un sistema filológico mas único que raro, que posee en potencialidad miles de palabras jamás consignadas en ningún léxico, y probabilidades infinitas de formar cuantas se necesitan, aún para expresar lo que jamás se ha expresado, y siempre de una manera tan precisa y clara que todos han de comprender."[19]

Throughout our interviews with Paraguayans runs a current of appreciation and love for Guaraní. They love to speak Guaraní because, as one informant put it, "uno se siente mas dueño de sí mismo," or as another put it, "one has the feeling of having *said* something."

Guaraní finally plays an important part as an ingredient in Paraguayan patriotism. According to our informants, Paraguayan troops during the Chaco War, who would have reacted lethargically to Spanish commands, obeyed commands in Guaraní with enthusiasm and contempt for death.

3.2. *Pride.* As in the case of language loyalty, the possession of a form of folk speech as well as that of a standard language may be a source of pride for the speakers. Some positive attitude such as pride is a prerequisite for the desire to develop's one's language into a standard. This attitude of pride will usually be focused on one or the other real or alleged property of the language. As with language loyalty, pride is often the more militant, the less recognized the status of one's language is by others.

There is no question but that the Paraguayans are proud of having Guaraní. We have already discussed the significance they attribute to it under the prestige function further above.

Only a very small segment of the population, the nouveaux riches of Asunción and some of the immigrants—so our informants tell us—show a contrary attitude: they look down on or are ashamed of Guaraní because it is an Indian language. But even in this group the negative attitude is not always permanent: one of our informants told us that his mother, a German immigrant, did not want him to learn this Indian language. Nevertheless, he could not help learning it, and his adult attitude is one of pleasure and pride in knowing it. It is, he says, not a mere dialect but a real language, and the more educated Paraguayans become, the more they appreciate Guaraní and make a conscious effort to improve their command of it.

What the Paraguayans appreciate most about Guaraní is that it is "la lengua del corazón." It is better suited, they say, for expressing emotions than Spanish—or, as one informant put it, than any other language he knows.

In Benitez' words, "son verdaderamente asombroses el número como del donaire de sus modismos; giros que hablan de una honda penetración; equivocos que se prestan a una sutil ironía; palabras que resumen todo un estado de alma; suaves y delicadas voces para el amor; expresiones de energía y afirmación como un grito de guerra . . . "[20] The flavor of Guaraní, they feel, is untranslatable—hence, even Paraguayan Spanish is studded with Guaraní loans.

3.3. *Awareness of the Norm.* This is an attitude more specifically limited to a standard language, since it is essentially a positive attitude towards codification. The codified norm is considered good and necessary.

In Guaraní, this attitude manifests itself primarily in a feeling for the desirability of a norm. Hence the high regard of literate Paraguayans for the work of the Academía de Cultura Guaraní which is expected to produce one.

The Paraguayan conception of a desirable norm for Guaraní is highly puristic. This purism is a correlate of their pride in Guaraní which we have discussed above. Many Paraguayans are bilingual and would like to speak both Spanish and Guaraní elegantly; they feel that mixing them, especially introducing unnecessary Spanish loans into Guaraní, is sloppy. We have already mentioned the conscious efforts to expand Guaraní vocabulary from native resources, which can be related to this puristic attitude.

4

Our preliminary survey of the Guaraní situation indicates that the conceptual framework which we have formulated is applicable to it at least in broad outline. As we see it, the descriptive aspect of the problem involves a multitude of questions of detail, some of them technical linguistic, some of them ethnopsychological, some technical ethnographic. The broader ramifications of the problem—the relation of this linguistic phase of the culture to other phases of present-day urban Paraguayan culture—touch upon the core problem of the interpretation of modern cultures: what is a modern culture, as opposed to an aboriginal one? Even if one rejects the many proposed dichotomies of civilized versus primitive, Kulturvolk versus Naturvolk, or folk versus urban, there remains a strong impressionistic awareness that there is some difference.

We feel that a language-and-culture problem such as ours, which allows the introduction of certain quantitative technical criteria, constitutes a useful point of departure to throw further light upon the folk-urban problem.

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Notes

1. The conceptual framework of this paper has been the major responsibility of the senior author; the Guaraní data and their systematization have been the major responsibility of the junior author.
2. Cf. R. Redfield, *The Folk Cultures of Yucatán*, passim. Chicago, 1941.
3. See Bohuslav Havránek, "Úkoly spisovného jazyka a jeho kultura" (= "The Functions of the Standard Language and its Cultivation"), in *Cercle Linguistique de Prague, Spisovná čeština a jazyková kultura* (*Standard Czech and the Cultivation of Good Language*), Prague, 1932, pp. 32 ff. A portion translated as *The Functional Differentiation of the Standard Language in a Prague School Reader on Esthetics, Literary Structure, and Style*, Paul L. Garvin, Transl. (Publ. of the Washington Linguistic Club, No. 1, Washington, D.C., 1955), pp. 1–18.
4. Cf. Uriel Weinreich, *Languages in Contact*, Publication of the Linguistic Circle of New York, No. 1, New York, 1953.
5. Vilém Mathesius, *op. cit.* in fn. 3, pp. 14 ff.
6. B. Havránek, *loc. cit.*
7. *Op. cit.* in fn. 3, p. 45; transl. *op. cit.* in fn. 3, p. 5.
8. *Op. cit.*, p. 46; transl. *op. cit.*, *ibid.*
9. *Op. cit.*, p. 67 ff.; transl. *op. cit.* p. 15 ff.

10. *El Paraguay y su Ciudadanía*, América (Buenos Aires), May–June 1954.
11. *El Solar Guaraní* (n.p., 1947).
12. *Op. cit.* in fn. 11.
13. Jan Mukařovský, *Jazyk spisovný a jazyk básnický* (*Standard Language and Poetic Language*), *op. cit.* in fn. 3, pp. 123 ff., transl. *op. cit.* in fn. 3, pp. 19 ff.
14. *Op. cit.* in fn. 4, pp. 99 ff.
15. *Paraguay en Marcha*, Vol. II, núm. 13 (January, 1949).
16. *Op. cit.* in fn. 15.
17. *Ibid.*
18. *Revista del Turismo* (Asunción), May, 1945.
19. *Op. cit.* in fn. 15.
20. *Ibid.*

THE GENERAL CLASSIFICATION OF CENTRAL AND SOUTH AMERICAN LANGUAGES

Joseph H. Greenberg

The classification of Central and South American languages set forth in the appendix to this paper is provisional in some respects. The eight families listed are to be considered branches of a more inclusive stock which probably includes also all the remaining American languages except Na-Dene and Eskimauan. Among the groups listed here only Otomanguean, which has not yet been thoroughly investigated by the writer, is considered at all not likely to belong to this great family. The problem of the classification of American languages becomes, therefore, for the most part a vast problem in the subgrouping of this single dominant family. It follows that, if this view proves correct, it will not be sufficient merely to show that a certain set of languages are related in order to prove that they form a stock. We must demonstrate that all of the languages within the group are more closely related to each other than any are to any languages outside the group. We distinguish, therefore, between the establishment of a valid relationship and that of a valid genetic group at whatever level. For example, Swedish, Albanian and Hindi are all related but they do not form a valid genetic group, and culture-historical conclusions drawn from treating them in this manner would be highly misleading.

If the thesis presented here is correct it will be possible to show that certain apparently contradictory theses concerning genetic relationships are not, in fact, incompatible, and we may thus avoid useless controversies. For example, Freeland and Sapir considered certain languages in Mexico to be Penutian; Whorf and McQuown added others, notably Mayan, in a family which likewise includes Azteco-Tanoan. It is my opinion that these languages are related to both California Penutian and to the Azteco-Tanoan languages but more closely to the former. The problem thus becomes one of subgrouping within the larger stock. This situation obtained to such a degree in South America that the writer considers that, whereas the groupings designated here by capital letters, e.g., Paezan, Andean and Macro-Ge, are for the most part certain as valid genetic groupings, it is by no means inconceivable, though unlikely, that, for example, Andean might turn out to be closer to Macro-Ge in III than to Equatorial in II, or that Paezan is closer to Andean than to Chibcha proper, although this alternative was carefully considered and rejected. A degree of uncertainty in subgrouping exists also at the lowest level. Thus I am by no means entirely certain that Bororo might not be considered a Ge language proper rather than as having a separate status within Macro-Ge. This should not be too surprising. After a century or more of comparative Indo-European studies the existence of an Italo-Celtic grouping is still being debated.

Of the eight groups outlined here, the greatest uncertainty exists in the case of the two new vast groupings in South America, Andean-Equatorial and Ge-Pano-Carib. It should be emphasized that the doubt does not pertain to the relationship among all of these languages but rather to the correctness of these two assemblages of languages as valid genetic groupings in the sense described above.

The time at my disposal is, of course, too brief to discuss the methodological problems with any degree of thoroughness. These will be treated fully in a projected series of articles in which it is expected that the classification will be set forth in detail and each family treated separately. A few important methodological considerations may, however, be pointed out. Only those resemblances which involve both sound and meaning simultaneously are considered relevant for historical connections. When the morphemes involved are roots this is called lexical comparison, when they are affixes, grammatical. There is no contradiction in the results attained by lexical and grammatical comparison and both methods are employed as far as possible. For purely practical reasons it is easier and more fruitful to begin with the comparison of basic vocabulary items.

A second important consideration is that comparison should be on the widest possible scale and utilize data from all the languages for which material exists. It might be thought that surer results would be attained by comparing only a small number of languages based on hypotheses of very limited scope. It will not be possible to discuss here all the reasons which indicate that this is not the case, but a few relevant factors may be indicated. It is a basic principle of comparative linguistics that a form is an inherited one if it reappears in languages of other subgroups of the same stock, while there is no prima facie case for this if it does not. This principle is of primary importance in eliminating purely accidental resemblances between two languages. Moreover, by considering all the more closely related languages on either side, the form can also be tested to see if the tentatively reconstructed forms for both groups are similar. The comparative method has always been applied to groups of languages rather than languages in isolation.

If we choose a small number of languages and compare them on a narrow basis not only are our results less reliable, but even when they are correct they are less significant and even misleading to archeologists and ethnologists because they are unlikely to be valid genetic units in the sense described earlier. Finally it may be pointed out that the evidential bearing of valid etymologies depends on their distribution. Thus, on an isolated hypothesis connecting Panoan with the distant Totonac, we might note the striking resemblance of Totonac *makan* and Panoan *meken*, both meaning "hand." However, we might misinterpret this form, which is actually found in a number of major stocks, as evidence for a special relationship between Totonac and Panoan, if we do not consider it in terms of the total relevant distribution.

With these methodological factors in mind, a brief description of the actual procedures will be given. The first was in the nature of a preliminary survey designed to provide initial hypotheses concerning groupings in South America. This consisted in the compilation of about forty vocabulary items which experience had shown to be among the most stable, for approximately forty languages. Among these languages were a number of the Arawak, Tupi and Carib languages on the assumption that larger groups such as these provide greater depth for comparative purposes than single isolated languages or small language stocks. From this first comparison a number of groups emerged, notable the Andean, Macro-Ge, Macro-Panoan, and Equatorial. Each of these was then assigned a separate notebook and a list of over three hundred words was compiled for each language, insofar as they were obtainable. New languages were compared with each of these groups with emphasis not on stray resemblances with single languages but on the occurrence of forms derivable by known types of sound changes from the tentatively reconstructed originals based on recurrent forms. Where languages did not fit into any of the established groups, they were assigned to new separate groups and entered in other notebooks. As new languages were examined, adding to the precision with which the basic lexical fund of each group was known, it became evident that certain of these groups, for example Macro-Ge and Macro-Panoan, were particularly close to each other and formed parts of still larger groupings. Material from over 250 languages was entered into notebooks at this stage.

The third procedure involves the use of index cards, each assigned to one semantic sphere, e.g., sun and day. Utilizing the material in the notebooks, entries are made in separate paragraphs for each probable etymology involving any languages in the area covered. It is thus possible to see at a glance the distribution of each set of probably related forms. This part of the work is still in its initial stages. When it is completed it will be possible to marshall the evidence in systematic form to solve the more difficult problems of grouping.

A fourth procedure has been the compilation of grammatical information for approximately thirty languages. The information in this area is, of course, less extensive in the existing literature than the lexical. What has been recorded thus far does not contradict conclusions based on lexicon, and in some cases adds striking confirmation. Thus a common system of singular pronominal prefixes seems to run through the entire Ge-Pano-Carib group and includes an irregular alternation in the third person.

It is hoped that other linguists will independently try the method of mass comparison suggested here in order to test the writer's conclusions. The ultimate test is a pragmatic one. Those parts of Sapir's scheme such as Penutian and Na-Dene which are valid have proved fruitful in that workers have been able to carry on more advanced comparative investigation within their framework. On the other hand, various suggestions regarding Central America contained in Sapir's Encyclopedia Britannica article have, in general, been ignored. My own methods indicate that they are, for the most part, incorrect. In the same fashion, if the present classification is correct, it will prove its usefulness in future more advanced comparative investigations, and arguments raised against it will be disregarded. By the same token it cannot be saved by the most ingenious argumentation if it fails the crucial test of practice.

TENTATIVE LINGUISTIC CLASSIFICATION OF CENTRAL AND SOUTH AMERICA

I. Macro-Chibchan
   A. Chibchan proper
      1. Chibcha-Duit, Tunebo group, Aruaco group, Cuna-Cueva, Guaymi-Dorasque, Talamanca group, Rama-Guatuso
      2. Misumalpan, Paya, Xinca, Lenca
      3. Shiriana
   B. Paezan
      Choco, Cuaiquer, Andaki, Paez-Coconuco, Colorado-Cayapa, Warrau, Mura-Matanawi, Jirajira, Yunca, Atacameno, Itonama

- II. Andean-Equatorial
  - A. Andean
    - 1. Ona, Yahgan, Alakuluf, Tehuelche, Puelche, Araucanian
    - 2. Quechua, Aymara
    - 3. Zaparoan (including Omurano, Sabela), Cahuapana
    - 4. Leco, Sec, Culle, Xibito-Cholon, Catacao, Colan
    - 5. Simacu
  - B. Jibaro-Kandoshi, Esmeralda, Cofan, Yaruro
  - C. Macro-Tucanoan
    - 1. Tucano (including Auixira), Catuquina, Ticuna, Muniche, Auaque, Caliana, Macu, Yuri, Canichana, Mobima
    - 2. Puinave
  - D. Equatorial
    Arawak (including Chapacura-Uanhaman, Chamicuro, Apolista, Amuesha, Araua, Uru), Tupi (including Ariqueme), Timote, Cariri, Zamuco, Guahibo-Pamigua, Saliban, Otomaco-Taparita, Mocoa, Tuyuneri, Yurucare, Trumai, Cayuvava
- III. Ge-Pano-Carib
  - A. 1. Macro-Ge: Ge, Caingang, Camacan, Machacali, Puri, Patacho, Malali, Coropo, Botocudo, Chiquita, Guato, Fulnio, Oti (prob.)
    - 2. Bororo
    - 3. Caraja
  - B. Macro-Panoan
    Tacana-Pano, Moseten, Mataco, Lule, Vilela, Mascoy, Charrua, Guaycuru-Opaie
  - C. Nambicuara
  - D. Huarpe
  - E. Macro-Carib
    Carib (including Pimenteira and Palmella), Peban, Witotoan, Cucura (prob.)
  - F. Taruma
- IV. Oto-Mangue
- V. Tarascan
- VI. Hokan including Jicaque, Yurumangui (prob.)
- VII. Penutian including a Mexican branch: Mixe-Zoque, Huave, Mayan, Totonac
- VIII. Azteco-Tanoan

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THE ORIGIN OF THE HAUSA LANGUAGE

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The place of the Hausa language among the languages of Africa has remained indefinite for a long time and has caused much dispute. In modern science there seems to prevail the opinion that the Hausa language belongs to the Chado-Hamitic group of languages and that it forms a special branch of the great Semito-Hamitic family of languages. After the works of Johan Lucas and other linguists, who studied the languages of the Central Sudan, a possibility arose of outlining the relationship of the Hausa language with the other languages of the Lake Chad basin, which has greatly helped to establish their relationship with Semito-Hamitic languages. At any rate Joseph Greenberg in his new classification of African languages includes them in the group of Afro-Asiatic languages, as he calls the Semito-Hamitic family of languages. This point of view, however, has not yet found general recognition. Thus, a most prominent French semitologist, Marcel Cohen, investigating the problem of establishing the relationship of the Semito-Hamitic family of languages, limits the composition of this family to the following four groups: Semitic, Cushitic, Ancient Egyptian and Berber; the Chad-Hamitic group of languages has not been included here.

In his study in the field of vocabulary and comparative phonetics of the Hamito-Semitic languages, Cohen, while analysing 515 common Semito-Hamitic roots, gives only 60 examples taken from the Hausa language.[1]

Among them there are words obviously borrowed from the Arab and Berber languages, being of rather late origin.[2] On the whole the number of Hausa words taken for comparison is very small as compared to those belonging to the languages of the four above-mentioned groups. This made him draw the conclusion that the common features between the Hausa language and the Semito-Hamitic languages are due to borrowing. Moreover, the resemblance between the systems of personal pronouns in the Hausa and in the Semito-Hamitic languages, which Cohen had pointed out at first, in his later work was considered as the result of borrowing.[3] Not without reason he doubts any kind of relationship between the languages of the Central Sudan and the Hamito-Semitic family. Among all the Chad-Hamitic languages it is only the Hausa language that has been more or less made the subject of profound scientific research. It must be mentioned here that, owing to the excellent works of Bargery and of Abraham, the vocabulary of the Hausa language has been studied much better than that of other African languages.

The morphology of the Hausa language has not been studied sufficiently; there still remains much to be cleared up. Other languages that are related to the Hausa language have not been sufficiently studied as yet, because so far we have had at our disposal only small dictionaries, short grammar outlines and a very small number of texts. Therefore when considering the relationship between the Semito-Hamitic family of languages and the Chado-Hamitic group we have to base our conclusions mostly on the data of the Hausa language.

On the whole it may be admitted that the features of resemblance between the Hausa language and the Semito-Hamitic languages are more pronounced in morphology than in the vocabulary. Bearing in mind that morphology is much steadier than vocabulary we have every reason to suppose the existence of very ancient links between the Hausa and the Semito-Hamitic languages.

According to the data of the lexico-statistic or glottochronological linguistic method, suggested by Morris Swadish, the percentage of similar words, after the split of the language unity (further contact being excluded), would be in a thousand years—66%, in 2 thousand years about 43%, in 3 thousand years 29%, etc.[4]

Whatever may be said about this method, the main points of it are correct (the small percentage of similar words in the languages of Hausa and the Semito-Hamitic family may be the result of a very ancient split).

Therefore, when investigating the genetic relationship between the Hausa and the Semito-Hamitic languages, one has to proceed from the comparison of their grammatical structure.

There is no doubt about the existence of some elements of the Sudanese or Bantoid substratum in the structure of the Hausa language. Such features as, for instance, the survivals in the Hausa language of a special noun class of people are of considerable interest. The formatives for the singular (prefix *ba-*) and the plural (suffix—*awa*) are obviously related to similar formatives of the GUR group of languages, where they have the form of the affix "ba", which is sometimes used as a prefix and sometimes as a suffix. Another feature of the language substratum might be the verb forms ending in —*na*, that have the character of reciprocity, (e.g., the verbs *dofa—dofana*, *do'be—do'bana* and others). The formative *-ana* is characteristic of the Bantu languages and adds to the meaning of the word the character of reciprocity. To the features of the grammatical structure that are decidedly alien to the Semito-Hamitic languages belongs the formation of the passive voice by means of the impersonal pronoun *an buge shi*—"he is beaten." The influence of the language substratum reveals itself probably in the formation of the plural and in some other forms, but on the whole the main features of the grammatical structure of the Hausa language have a Semito-Hamitic character.

The most characteristic feature pointing to the relationship between the Hausa and the Semito-Hamitic languages is the system of pronouns. This was stated by H. Bart as early as the middle of the last century. Later, Shön, Lepsius, Friedrich Müller and others, basing their statements on this observation, pointed to the relationship between the Hausa and the Berber languages. The same was mentioned by semitologists Zimmern and Johann Bart in their works. The features of resemblance between the pronouns both in the Hausa and the Semito-Hamitic languages are given a brief but rather exhaustive description in the work of Greenberg, devoted to the classification of African languages.[5]

It is necessary that the system of pronouns in the Chado-Hamitic group of languages should be throughly investigated, but this is an object of a special study. Here I just wish to point out that in order to throw light on the relationship of the Semito-Hamitic languages with the Hausa language, the forms of the third person plural of personal and possessive pronouns are of great importance. The thing is that these forms differ from the forms of Arab pronouns with *h-* and are similar to the ancient Egyptian, Cushitic and Berber form with *s-*. This is important, because it excludes any kind of supposition concerning the borrowing of pronouns from the Arab language. As is known, in the ancient

Minoan inscriptions these pronouns also had the *s*- form. Thus we see that the Hausa language has preserved the ancient form common to the Semito-Hamitic languages.

Another characteristic feature is the presence of gender in the grammatical structure of the Hausa language, which singles out both the Hausa language and all the languages related to it, from the rest of the Sudanese languages. Here it is essential, of course, that the formative *a* of the feminine gender (with nouns) should be traced to the common Semito-Hamitic *at*-, as has been recently shown by A. Klingenheben.[6]

Among different ways of forming the plural there are some characteristics of the Semito-Hamitic languages, for instance, the formation of plural by means of the internal inflexion, namely with the help of the *a* and *u* formatives.[7]

It is typical of Semito-Hamitic languages that nouns are derived from verbs by means of the prefix *ma*. Participles, nouns denoting places, instruments, agents (nomina loci, instrumenti, et actionis), etc., are also formed by means of the formative *ma*. The wide use of this formative in all the Semitic languages proves that it belongs to the period of the Semitic unity.[8] The same prefixes, however, are used to form nouns denoting places, instruments and agents in the Old Egyptian language (examples have been found in the most ancient texts). For instance:

swr — 'to drink'	*mswr* — 'place for drinking'
śdr — 'to sleep'	*mśdr* — 'place for sleeping (ear)'
ḫ3j — 'to measure'	*mḫ3t* — 'scales'
wnḫ — 'to dress'	*mnḫ* — 'dress'
hnj — 'to row'	*mhnt* — 'stern'
uhsj — 'to watch'	*mnhs* — 'one who watches'

This way of wordbuilding is widely spread in the Hausa language and has the same functions as in the Semitic and Old Egyptian languages. Nouns denoting places, instruments and names of agents are also formed with the help of the prefix "*ma*."[9] For instance:

aikata — 'to work'	*maaikata* — 'workshop'
karanta — 'to write'	*makaranta* — 'school'
auna — 'to measure'	*ma'auna* — 'place where the grain is weighed'
busa — 'to blow'	*mabusa* — 'flute'
bu'da — 'to open'	*mabu'di* — 'key'
dafa — 'to cook'	*madafi* — 'cook'
aika — 'to send'	*ma'aiki* — 'messenger'
halbi — 'to shoot'	*mahalbi* — 'hunter'

In the Semitic languages the formative *ma* is related to the interrogative —relative particle *ma* — 'what?' It is the same in Old Egyptian, where this formative may be related to the interrogative pronoun *m* — 'who?' 'what?'.

In the Semito-Hamitic group of languages, as well as in other languages, the stem of this formative has been preserved in the interrogative pronoun "*me*," 'what?' and its derivatives *mene*, *menene* and others.

Another means of wordbuilding common to all the Semito-Hamitic languages is the formation of abstract nouns. With the help of the formative of the feminine gender *ta*, *taka*, for instance, *sarauta* 'kingdom', *mugunta* 'evil', *tajirtaka* 'wealth', etc. Words of this type are to be found in Egyptian texts dating from the period of the beginning of Ancient Kingdom (Pyramid texts) and in Berber inscriptions of the time of Massinissa—III century B.C.

Thus we see that many features of the grammatical structure of the Hausa language are decidedly of Semito-Hamitic origin.

The features of ancient common origin reveal themselves also in the system of the verb. As is known the verb in the Hausa language is conjugated by means of prefixation.[10] Verbal pronouns, like all the other pronouns, have elements common to Semito-Hamitic languages. The derivation of verb forms: causative by means of *s* and passive by means of *u* is of the same origin. Up to now, however, the question has never been raised as to whether there is to be found in the verbs of the Hausa language a change in the verb stem by means of doubling the root consonants, that is characteristic of Semito-Hamitic languages. There are examples in the Hausa language showing that it is the consonants that convey the semantic value of the word, whereas the vowels are of secondary importance. For instance, the verb *mutu*: *ya mutu* 'he died', *yana mutuwa* 'he is dying'; but the participles have the stem *mat*: *matacce*, *matacciya*, *mataccu*. The doubling of the root consonants, which is the result of (or accompanies) the reduplication of the syllables, is to be found in the formation of the intensive and iterative forms of the verb, in the formation of qualitative adjectives and in the forms of the past participle.

A most characteristic feature of the Semito-Hamitic languages—the change of the verb root by means of doubling one of the consonants of the root—is often to be found in the formation of the intensive form. Thus, for instance:

	intensive form
tara — 'to gather'	*tattara*
t'ba — 'to touch'	*tatta'ba*
girmama — 'to honour'	*giggirmama*
girba — 'to reap'	*giggirba*
haifa — 'to give birth to'	*hahhaifa*
girka — 'put on the fire'	*giggirka*

In all these examples not only the first syllable is reduplicated, but the consonant of the first syllable too. Many three-syllable verbs, however, reduplicate not the first but the second syllable and, accordingly, the consonant of the second syllable. For instance:

rubuta — 'to write'	*rububbuta*
kur'ba — 'to suck'	*kururru'ba*
halbi — 'to shoot'	*halallabi*

The forms are of the same kind as the iterative forms in the Amharic language: *sabābbara*, *batăttana*, *gamāmmasa* and so on. In these forms we find not only the reduplication of the second root consonant, which is typical of the formation of the intensive form in the Semitic languages, but also the reduplication of the syllable characteristic of the Hausa language. Brockelmann and Littmann believe that it is possible to consider these forms to be the result of the Cushitic influence on the Amharic language.[11] It is possible, however, to regard them as old Semito-Hamitic forms that have become extinct in most of the Semitic languages.

At any rate, identical forms have been found in the Ethiopian language, for instance, *hangaga* 'to be angry'. The reduplication of the syllable (*qataltal*) may be found in the Arab and other Semitic languages in intensive forms of adjectives: *asabsab* ‘hard’ (day), etc.[12] We also find the reduplication and iteration of the consonant in the Hausa language in the formation of the participle forms of the passive voice (the function of the participle of active voice is performed by nomina actions with *ma*). For instance:

	sg.		
	masc.	fem.	pl.
tara — ‘to collect’	*tarrare*	*tararriya*	*tarraru*
ta’ba — ‘to touch’	*ta’ba’b ’be*	*ta’ba’bbiya*	*ta’ba’bbu*
girmama — ‘to honour’	*girmamamme*	*girmamammiya*	*girmammama*
girba — ‘to reap’	*girbabbe*	*girbabbiya*	*girbabbu*
haifa — ‘to give birth to’	*haifaffe*	*haifaffiya*	*haifaffu*
girka — ‘put something on the fire’	*girkakke*	*girkakkiya*	*girkakku*

Participles formed from verbs that already have reduplication and, evidently, present the intensive forms of verbs whose original form is extinct, have the following aspect:

kididdiga — ‘count’	*kididdigagge, -iya, -ū*
gididdiba — ‘cut into pieces’	*gididdibabbe, -iya, -ū*

Alongside of features pointing to the common Semito-Hamitic origin, we find in the Hausa language features that bring it into close relationship to the Ancient Egyptian, Coptic and Berber languages only. First of all must be mentioned the means of formation of the possessive case—the possessive link *n*. This feature unites all these three groups of languages, which form in this respect a kind of Western group of Semito-Hamitic languages. It differs from the Eastern groups, that is the Semitic and Cushitic languages, which have developed their own system of case forms.

It is not my task to enumerate all the features of the Hausa grammatical structure, relating it to the languages of the Semito-Hamitic group. I only wished to stress the most essential points, important for the solution of the question of origin of the Hausa language, and point out certain new data which have not yet attracted the attention of linguists. It seems to me that at present we have sufficient material at our disposal in order to state that all the above-mentioned features of resemblance prove the presence of genetic relationship. There is no doubt, however, that for the final solution of this problem a thorough investigation of the Hausa vocabulary should be undertaken.

The investigation of the Hausa vocabulary undoubtedly reveals in it certain features common with the languages of the Semito-Hamitic family. These common features are sometimes wrongly ascribed to Arab borrowings. The latter, however, belong to a comparatively recent time and are limited to a rather narrow sphere. These are mainly words connected with the Koran, with Islam in general, with the Moslem legislation based on Shariat, and also terms connected with the Arabic written language.

Due to the absence of written data, it is not always easy to define whether this or that particular word is of recent borrowing. In this respect the language of Ancient Egypt is of tremendous help, because in some cases it allows a certain chronology to be set up.

By way of illustration allow me to take a Hausa word *yamma* 'west'. This word was usually considered to have been borrowed from the Arabian language, but in Arabic this word, with a three-radicals stem *ymn*, means 'right side', 'south', and therefore seem to point to the east. K. Sethe, the well-known Egyptologist, based his conclusions on this fact, when he proved the Asiatic origin of Egyptian culture. Since *ymn* in Old Egyptian means 'right side' and 'west', Sethe concluded that the ancestors of the Egyptians had penetrated into the valley of the Nile from the north of the delta and had moved up the river facing the south, with the west on the right side. There is no need to recall the fact that at the early period of the history of Egypt the delta of the Nile was a swamp and it was impossible to open it up; therefore the hypothesis put forward by Sethe has been rejected by archeologists.

The fact that the word *yamma* means 'west' in the Hausa language, otherwise, has the same meaning as *imn* in Old Egyptian, obviously proves that this word could not have been borrowed from the Arabic where it means south.

In some cases it is possible to establish a relationship between the Hausa language and the most ancient period of the Old Egyptian language. Thus the Hausa word *hanci* 'nose' has already been compared with the Ancient Egyptian *ḫnt* 'face', 'the frontal part of the head'. Evidently there was a time when it used to denote 'nose' in Ancient Egyptian; at any rate the word *ḫnt* in the hieroglyphic writing has it as its determinative. The word was found as early as the period of the Pyramid texts and was later used only as an obsolete word; it disappears from the late Egyptian texts, being replaced by the words *fnḏ* and *šrt* 'nose', the Coptic *šā*, *šāi*. This example shows that the relationship between the Old Egyptian and the Hausa languages refers to a period of several thousand years B.C.

To the group of words, not numerous as yet, proving the ancient relationship of the Hausa with the Semito-Hamitic language, belong the words *halshi* 'tongue', *mutu* 'to die', *mutum* 'man' and some others. Many comparisons may be found in the works of Vyčicl, Cohen, Calice, Greenberg. Quite a number of new words may be added to them. Thus, for instance, *faski* 'breadth'; Old Egyptian *wsḫ*; *marmari* 'desire', Old Egyptian *mr. j.* 'to love'; *sani* 'to know', Old Egyptian *S*33 'one who knows' (*Pyramid texts*), and many others. In some cases it is possible to prove that a Semitic word is of Semito-Hamitic origin and has not been borrowed from the Arab language, owing to the fact that the word occurs in the languages of tribes which have not undergone the influence of the Islam and the Moslem culture. Here belongs for instance the word *sama* 'sky', and the word-combination *ruwan sama* 'rain', which by many specialists on the Hausa language was considered Arab. However, the presence of the word in the vocabulary of the Logone language *sama* 'rain', the Gudu *zim*, the Mandara *samaya*[13] (with the same meaning) shows that it is of ancient origin. Probably the words *sarki* 'chief', 'king', Ancient Egyptian *sr* (Pyramid texts) and Akkad *šarru* also belong to the most ancient period of history.

How can we account for the genetic relationship of the Hausa language with the Semito-Hamitic family of languages? Is it possible to suppose the migration of Berber tribes to the south as was done by some scientists, who considered these hypothetical Berbers to be the ancestors of the Hausa? I believe that the presence of a multitude of small tribes in the marshes of Shari-Lagone and in the mountains of Bauchi, all speaking languages related to the Hausa language, contradicts this hypothesis. Nor is there any necessity of supposing the migration of some kind of Hamitic tribes that had come to the Chad Lake from the east.

In all likelihood the Hausa language and all the languages of the Chado-Hamitic group form the southern group of ancient tribes, that had once inhabited the regions of the Sahara. Archeological investigation showed that at the time of the neolithic age the Sahara was inhabited; at any rate this is true of many of its regions, if not of all of them. Its population must have consisted of the ancestors of the later Libyan tribes. As the Sahara gradually dried up and lost its grassy cover these tribes moved to big reservoirs. Part of them moved north —these are Libyan tribes, ancestors of the modern Berbers. Another group was formed by tribes that settled in the Nile valley—the ancestors of ancient Egyptians. And the last group, the third one, was formed by tribes that moved in the direction of the big reservoirs of the ancient Sahara—to the powerful river Bahr- el Ghazal (Soro) and the Chad Lake, which had once occupied a much greater area than now, to the river Tafassaset and to many other mountain streams which no longer exist. The archeological excavations in these regions on the banks of rivers, ceramics, ashes, bones, traces of settlements, have been found. (Iron has not been found.) The same picture was in the regions northwest to the Chad Lake. A big river which now is known as Vadi Tafassaset flowed into the Chad Lake near Ngigmi. It was not a single river which irrigated southern regions of Air. From the plateau Air a river named Vadi Tazizilet and other rivers flowed to the Niger. Archeological excavations in these areas proved the fact that all that area used to be rather densely populated. In the Air region many neolithic settlements, stone implements, ceramics, etc., have been found.[14] The modern Hausa population has preserved up to now the legends of their ancestors, who lived somewhere north, in the Air region.

Thus we must regard the languages of the Chado-Hamite group as the southern branch of the languages of the tribes that once used to populate the vast areas of the now barren Sahara. It is evidently in Sahara and, probably, in the grassy steppes of Ancient Arabia that the Semito-Hamitic family of languages sprang up. As a result of the gradual formation of the desert the Hausa and the tribes related to it lost contact with their former kinsfolk, and the only proof of their former relationship remains their language, which has preserved traces of common Semito-Hamitic origin.

Notes

1. M. Cohen. Essai comparatif sur le vocabulaire et la phonétique du chamito-sémitique. Paris, 1947.
2. For instance, 30, 217, 232. Besides there are doubtful comparisons, such as 268, 194, 66, 90, and others.
3. M. Cohen. Langues chamito-sémitiques et la linguistique historique. *Scienta*, XI, 1951. Ser. VI, 86: (475) 304–310.
4. Meeussen, A. E. Lexicostatistik van het Bantoe. *Kongo-Overzee*, 1956, 22: (1) 86–89.
5. Jos. H. Greenberg. Studies in African Linguistic Classification: IV. Hamito-Semitic. *Southwest Journal of Anthropology*, 6: (1) 53–55, 1950, where the first attempt was made to find features of resemblance between the grammatical structure of the Chad group and the Semito-Hamitic languages.
6. A. Klingenheben. Althamito-semitische nominale Genuseexponenten in heutigen Hamitensprachen. *ZDMG*, 101: 78–88, 1951.
7. The formation of plural by means of the *-a* formative is analyzed in Greenberg's article: "Internal *a*-Plurals in Afroasiatic (Hamito-Semitic)," *Afrikanistische Studien*, Deutsche Ak. d. Wiss. zu Berlin-Institut für Orientforschung Veröffentlichung 26: 198–204, 1955. Berlin.

8. N. S. Nyberg. Wortbildung mit Präfixen in den Semitischen Sprachen. *Le Monde Oriental*, 14: 177–291, 1920.

9. Grapow, H. Über die Wortbildung mit einem Präfix mim ägyptischen. *Abhandl.*, Preuss. Ak. b. Wiss, 5: 191 pp.

10. The only exception is the verb of movement—*za* 'go'; that is conjugated by means of suffixes.

11. C. Brockelmann. *Grundriss der vergleichenden Grammatik der Semitischen Sprachen*, 1: 516, 518, 1908. Berlin.

12. C. Brockelmann. *Op. cit.*, pp. 367–368, 508.

13. Greenberg, Studies in African Linguistic Classification, *op. cit.*, p. 61.

14. Seliquer. Eléments d'une étude archéologique des Pays Bas du Tchad. *Bull. de l'Inst. fr. d'Afrique Noire*, 7: 191–209, 1935; Mauny. The same edition, 11: 142, 1949.

TONALITY IN EFIK SIGNAL COMMUNICATION AND FOLKLORE

Donald C. Simmons

The Efik of Calabar Province, Nigeria, speak a language which utilizes tone phonemes in lexical and morphological distinctions, and send messages by means of the iron double-gong and the wood slit-gong.[1] The *ak$\overset{1}{\text{a}}$ŋk$\overset{1}{\text{a}}$ŋ* "iron double-gong" consists of two iron gongs welded along a flange and joined together by an arched handle.[2] The *obodom* "wood slit-gong" is a slit-gong hollowed from wood, usually red iron wood (*Lophira procera*), in such a manner that two lips of the exterior surface project over the excavated area; these lips vary in width and thickness, and the lip of smaller width produces a higher note than the wider lip. Each signal instrument produces two different notes.

The notes produced by these signal instruments correspond with the tones of the morphemes that constitute the phrases which Efik state these instruments send. The following examples illustrate the correspondence between the notes of the signal message and the tones of the spoken message. Symbols used to notate syllabic tones throughout this article are: low tone, no notation, high tone 1, mid tone 2 (this includes both mid tone and a tone higher than mid but lower than high which occurs with high toned verb roots), falling tone 3, and rising tone 4. Sounds produced by the signal instruments are notated L for the low note and H for the high note. Although these examples do not illustrate the total differences between the linguistic tones and instrument notes, certain modifications occur in the reproduction of linguistic tones on signal instruments since the instruments only produce two notes. Falling tones are represented by a high note followed by a low note. A low note followed by a high note designates rising tone. Mid tones may be represented by either high or low notes depending on the tone environment in which they occur; a high note designates a mid tone when the mid tone is preceded and followed by low tones or preceded by a low tone and followed by a high tone, but a low note may represent mid tone when the mid tone is preceded and followed by high tones. Low and high notes designate respectively low and high tones.

(1)	message:	*ɔs$\overset{1}{\text{ɔ}}$ŋ*	*$\overset{1}{\text{u}}$b$\overset{1}{\text{ɔ}}$k*	*ɔs$\overset{1}{\text{ɔ}}$ŋ*	*$\overset{1}{\text{i}}$nua*
		strong	arm	strong	mouth
	iron-gong:	L H	H H	L H	H L
	slit-gong:	L H	H H	L H	H L
(2)	message:	*$\overset{1}{\text{i}}$duot*	*$\overset{1}{\text{o}}$duk*	*ɔfɔŋ*	*$\overset{1}{\text{u}}$suk $\overset{1}{\text{u}}$suk*
		red	enters	cloth	slowly
	iron-gong:	H L	H L	L L	H L H L
	slit-gong:	H L	H L	L L	H L H L

Do signal messages represent a code or a true language?

Three factors suggest that the unit of semiosis in signal communication is the phrase and not the individual morpheme:

(*a*) Morphemes in the Efik language may be grouped into a limited number of classes the members of which all possess identical tones. Consequently, it would be difficult to distinguish any one morpheme of a class on the basis of tone alone.[3]

(*b*) Since the rate of transmission of the signal notes is approximately constant segmentation of the tones into the correct constituent morphemes would be extremely difficult, if at all possible.

(*c*) Analysis of the phrases used as signal messages reveals that these messages consist of aphoristic, proverb-like phrases.

Analysis of inherent toned morphemes in the Efik lexicon, the inherent tone for a verb root being the imperative singular and for a noun being the form when spoken alone, reveals that monosyllabic verb roots include 173 H, 152 L and 8 R toned roots, while disyllabic roots include 152 HH, 91 LH and 27 LL toned roots.[4] Disyllabic nouns include 204 HH, 168 HL, 158 LL, 150 LH, 61 LF, 32 HF, 12 HM, and 7 LR toned nouns. Trisyllabic nouns include 134 HHH, 104 LLL, 70 LHH, 58 LHL, 48 HLL, 47 LLH, 44 HLM, 26 HHL, 13 LHM, 9 LLF and 8 HLF toned nouns. All members of a tone class expərience the same tone changes under similar morphological circumstances.[5]

Table 1 represents some possible segmentations for the signal message *ɔs$\overset{1}{\text{ɔ}}$ŋ $\overset{1}{\text{u}}$b$\overset{1}{\text{ɔ}}$k ɔs$\overset{1}{\text{ɔ}}$ŋ $\overset{1}{\text{i}}$nua*. Line A shows that the inherent tone class of *ɔs$\overset{1}{\text{ɔ}}$ŋ* includes 173 members (since *ɔs$\overset{1}{\text{ɔ}}$ŋ* is an adjective derived from the monosyllabic high toned verb root *sɔŋ*), *$\overset{1}{\text{u}}$b$\overset{1}{\text{ɔ}}$k* 204 members, and *$\overset{1}{\text{i}}$nua* 168 members; consequently, the number of phrases which can be constructed with an identical tone pattern of the signal message is 173·204·173·168 or 1,025,609,088 phrases.[6] Many of these phrases will be meaningless, but *a priori* their semiotic content cannot be determined. Therefore, granting that hearers were able to segment correctly the signal notes it would still be necessary to determine which of the over one billion tonally identical phrases the signaller meant.

However, it is gratuitous to assume that the hearer would necessarily segment the signal notes into the correct pattern shown in line A. Lines B to G represent other possible segmentations which could produce the same over-all tone pattern. Thus, line B represents an adjective of the pattern LH followed by two nouns of the pattern HHL. In lines D to G a starred number shows that a verb root, with a necessary pronominal prefix, is assumed to occur. These variant interpretations, which *a priori* cannot be dismissed as impossible or meaningless, illustrate the difficulty in determining the correct segmentation of signal notes into the tones of the constituent morphemes if the signal notes are thought to be a language rather than a code. The interpretations listed A to G include over 2.9 billion possible phrase constructions which possess the same tone pattern of the signal message, and additional phrases can still be constructed.

The tone pattern of the signal message *$\overset{1}{\text{i}}$duot $\overset{1}{\text{o}}$duk ɔfɔŋ $\overset{1}{\text{u}}$suk $\overset{1}{\text{u}}$suk* is similar, assuming a segmentation into correct constituent morphemes and interpreting *$\overset{1}{\text{u}}$suk $\overset{1}{\text{u}}$suk* as derived from a low toned monosyllabic verb root, to a minimum of 613,272,576 phrases; however, ten alternative ways of possible segmentation include over 252.9 billion phrases each of which possess the same tone pattern as the signal message.

TABLE I

<table>
<tr><th></th><th>ƆS$\overset{1}{\text{Ɔ}}$Ŋ</th><th colspan="2">$\overset{1}{\text{U}}$B$\overset{1}{\text{Ɔ}}$K</th><th colspan="2">ƆS$\overset{1}{\text{Ɔ}}$Ŋ</th><th>INUA</th></tr>
<tr><td>A</td><td>173</td><td colspan="2">204</td><td colspan="2">173</td><td>168</td></tr>
<tr><td>B</td><td>173</td><td colspan="3">26</td><td colspan="2">26</td></tr>
<tr><td>C</td><td colspan="2">70</td><td colspan="2">168</td><td colspan="2">26</td></tr>
<tr><td>D</td><td>173</td><td colspan="2">173*</td><td colspan="2">173</td><td>168</td></tr>
<tr><td>E</td><td colspan="2">70</td><td colspan="2">152*</td><td colspan="2">26</td></tr>
<tr><td>F</td><td>173</td><td colspan="2">204</td><td colspan="2">173*</td><td>168</td></tr>
<tr><td>G</td><td>173</td><td colspan="3">152*</td><td colspan="2">26</td></tr>
</table>

The tone pattern of the signal message $\overset{1}{e}$*suk* $\overset{1}{u}$*rua* *et*$\overset{1}{i}$*b*$\overset{1}{i}$ $\overset{1}{m}$*b*$\overset{1}{i}$*et* *urua* $\overset{1}{i}$*nyamk*$\overset{2}{e}$ "beach of market grows grass market doesn't sell" may be interpreted, assuming correct segmentation, into 168·158·152·204·158·152 or over 19.7 trillion ways; adding variant segmentations, over 113.2 trillion phrases can be constructed which possess the same tone pattern as the signal message.

Sixty-one signal phrases are composed of fifty-five proverbs, four nicknames, and two alert signals. Analysis of the sixty-one phrases reveals that only two possess the same tone pattern. Investigation of these two messages shows each message equates in meaning to the other and that the two messages actually represent a single message which Efik happen to interpret in two slightly different ways due to the similarity of tone patterns of the spoken phrases. These two messages ($\overset{1}{i}$*sɔŋ* $\overset{1}{m}$*kpɔ* *ɔfɔŋ* $\overset{1}{i}$*yeh*$\overset{2}{e}$ $\overset{1}{i}$*d*$\overset{1}{e}$*m* "ground of squatting cloth doesn't beautify self" and $\overset{1}{i}$*kɔŋ* $\overset{1}{i}$*so* *ofim* $\overset{1}{i}$*yeh*$\overset{2}{e}$ $\overset{1}{i}$*d*$\overset{1}{e}$*m* "leaf before wind doesn't beautify self") are sent during obsequies, express sadness, and allude to the fact that relatives of the deceased will never again be as they were before the deceased's demise; the phrases are also nicknames for orphans.

Certain morphological constructions, notably the method of expressing genitive relationship, deviate from the norm when appearing in phrases used as signal messages (e.g. *ɔs*$\overset{1}{ɔ}$*ŋ* $\overset{1}{u}$*b*$\overset{1}{ɔ}$*k* used instead of the more usual *ɔs*$\overset{1}{ɔ}$*ŋ* $\overset{1}{u}$*bɔk*); possibly these deviations occur in order to differentiate the tone patterns of two phrases which otherwise would have identical tone patterns.

The fact that proverbs and proverb-like aphorisms serve as signal messages suggests that they are used because their tone patterns are well known and therefore easily recognized when heard as signal notes. Proverbs are assigned special meanings when used as signals. "Strong arm, strong mouth" is an encouragement meaning 'now fulfill your boasts and do with your hands as well as with your mouth'. "Red enters cloth slowly" means enemies approach slowly. The message "beach of market grows grass, market doesn't sell" signifies sadness, is sent during obsequies, and means 'things are not good for the markets are empty'.

Comparison of the data on Efik signals with data from other African tribes (cf. Rattray, 1923, pp. 256–8; Verbeken, 1920, p. 256; Van Goethem, 1927, p. 713; Carrington, 1949a, pp. 78–109, 1949b, pp. 35–39, and 1953) reveals that the utilization of proverbs and maxims, presumably since their tone patterns are well known, as the basis of signal communication is not unique to the Efik but wide spread in Africa.

A special form of folklore, which the writer denominates the tone riddle, answers the question whether Efik are conscious of similarity in tone patterns of sentences. A tone riddle consists of two phrases, a query and a response, which differ in meaning but usually possess either identical or very similar tone patterns. The degree of similarity in tone pattern of a query and its response varies from complete resemblance to almost complete dissimilarity; however, the latter condition is much less prevalent since in 94 tone riddles only 17 appear to possess complete dissimilarity between tone patterns of the query and response.

An analysis of 810 examples of proverbs, tone riddles, stereotyped sarcasm, riddles, and tongue twisters collected in Creek Town reveals that 94 examples of tone riddles surpass in number the 37 examples of stereotyped sarcasm, 21 ordinary riddles and 3 tongue twisters, although numerically less than the 655 proverb examples.[7]

The following example illustrates identical correspondence of tone pattern between the query and response of a tone ridde:

	1	1	1 1 1	1
Query:	*afak*	*ɔkɔk*	*ke etak*	*utɔŋ*
	placer	of chew-stick	under	ear
	1	1	1 1 1	1
Response:	*esin*	*enyin*	*ke ŋkpɔ*	*owo*
	putter	of eyes	in thing	of person

The second example instances a partial correspondence between the tone patterns of the query and response:

	1	1	1	1	
Query:	*ekpuk*	*eto*	*ɔsɔŋ*	*abiat*	*ekuri*
	knot	of tree	if strong	spoils	axe

	1	1	1	
Response:	*edidia*	*akaŋ*	*abiat*	*ufan*
	eating	famine	spoils	friendship

Tone riddles function as forms of amusement, greetings, erotic double-entendre, indirect method of cursing, and succinct explanations for an action.

Any example illustrates the usage of the tone riddle as a form of amusement. A small group of Efik may pass the time by citing tone riddles. The use of the tone riddle as a greeting to replace the customary greetings involves two friends greeting each other, the first saying a query of a tone riddle to which the other friend gives the correct response. Since friends usually greet whenever meeting tone riddles supplement the regular greetings and add an element of humor to what would otherwise be commonplace.

The tone riddle readily lends itself to the formation of erotic answers to ostensibly innocent statements. Thus, the apparently innocuous phrase
1 1 1 3 1 1 1
ntanta ɔyɔhɔ abasi obu ɔyɔhɔ mmɔŋ "stars fill God, shrimp fill water" possesses
1 1 1 1 1 1 1 1
the erotic response *aran itit ɔyɔhɔ obube nsene ɔyɔhɔ akwa* "vaginal secretions fill pit, semen fills bowl."[8]

A person who curses someone may be sued before the Native Court and fined heavily if found guilty. The tone riddle allows an individual to express contempt without actually cursing. One simply says to his enemy the query of a tone
1 1 1
riddle which possesses a derogatory answer. The query *ŋkpatari ɔbiɔk enyɔŋ*
1 1
ɔbiɔk isɔŋ "*ŋkpatari* vine grows above grows below" possesses the response
1 1 1 1
uka aman efik aman ekuŋ "your mother gives birth to hernia, gives birth to humpback" (i.e., your mother bears a child suffering hernia and a humpback).

The tone riddle is occasionally used as a periphrastic method of giving a reason for having done some particular action. If asked why he divorced his
1 1 1 1 1
wife a man might reply *ɔtɔ mkpɔŋ oyom mbɔ* "planter of cocoyam wants edible
1 1 1 1 1
root" which entails the response *ɔdɔ aŋwan oyom eyen* "marrier of wife wants child," thus obliquely informing the questioner he divorced his wife because of her barrenness.[9]

The tone riddle is known to occur in Ibibio and Oron folklore, but the distribution of the tone riddle in Africa is unknown at present. The tone riddle will probably be found to have wide distribution among African societies south of the Sahara.

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Notes

1. Efik also send messages by means of a trumpet made from antelope horn. Production of trumpet signals is believed to be similar to the production of slit-gong signals, but this is not categorically certain since trumpet signals were not investigated.
2. See Simmons, 1955a, for illustration of the iron double gong.
3. Both Rattray (1923, p. 257) and Carrington (1949, p. 233) have alluded to this aspect of African signal communication.
4. The symbols H, L, M, R, F respectively indicate high-, low-, mid-, rising-, and falling-toned syllables; the symbols read from left to right with the first symbol on the left indicating the tone of the initial syllable of the morpheme.
5. See Ward, 1933, for delineation of the changes which inherent tones may undergo.
6. I am indebted to Professor Frank D. Quigley, Mathematics Department, Yale University, for the method of computing the permutations involved in this type of problem.
7. See Simmons, 1955b, for illustrations of the various types of Efik folklore.
8. See Simmons, 1956, for specimens of erotic Ibibio tone riddles.
9. See Simmons, D. C., "Cultural Functions of the Efik Tone Riddle," *Journal of American Folklore*, Vol. 71, 1958, pp. 123–138, for additional examples of the tone riddle.

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